American Political Science Review

5

MARCH 1998
VOLUME 92 NUMBER 1

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CUK- 140 5438-J05-KP705

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CORRECTION: The following authors' note was omitted from Thomas J. Christensen and Jack Snyder, "Progressive Research on Degenerate Alliances," *American Political Science Review* 91(December 1997):919–22. Thomas J. Christensen is Associate Professor of Government, Cornell University, Ithaca, NY 14853. Jack Snyder is Professor of Political Science, Columbia University, New York, NY 10027.

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Prospecting for Participants: Rational Expectations and the Recruitment of Political Activists, **Henry E. Brady, Kay Lehman Schlozman, and Sidney Verba**

From Abe Fortas to Zoe Baird: Explaining Why Some Presidential Nominations Fail in the Senate **Richard Fleisher, Jon R. Bond, and Glen S. Krutz**

Macropartisanship: A Replication and Critique **Donald Green, Bradley Palmquist, and Eric Schickler**

Organized Interests and the Decision of Whom to Lobby in Congress **Marie E. Hojnacki and David Kimball**

When the Party Breaks Up: Exit and Voice among Japanese Legislators **Junko Kato**

Critique of 'Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?' by Charles Cameron, David Epstein, and Sharyn O'Halloran **David Lublin**

What Moves Macropartisanship? A Response to Green, Palmquist, and Schickler **Michael B. MacKuen, Robert S. Erikson, and James A. Stimson**

The Strange Case of Christianity and Republicanism: A Comment on Black **Cary J. Nederman**

Domestic Opposition and Signaling in International Crises **Kenneth A. Schultz**

Thoreau's Alternative Economics **Brian Walker**

ERRATA:

Authorship of the following book, reviewed on pp. 433–34 of the June 1998 issue of the *Review*, was given incorrectly. The correct citation follows.

The Power of Identity: Politics in a New Key. By Kenneth Hoover, with James Marcia and Kristen Parris. Chatham, NJ: Chatham House Publishers, 1997. 160p. $19.95.

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Editor's Notes

The relationship between electronic publication and traditional print journals such as the *American Political Science Review* is a matter of increasing discussion in both scholarly and editorial communities. The outcome of many issues will emerge only over time from the interplay of the interests of scholars, libraries, and publishers. For example, will print journals become merely archives for scholarly work rather than important vehicles in its development? "Never!" seems not to take account of the incredibly rapid growth of internet publication and the efficient way it responds to scholars' needs to communicate with one another and keep abreast of the latest advances. "Yes" to an archival role for print journals seems plausible until one contemplates the space limitations of most libraries and the financial pressures that have caused many to discontinue subscriptions to journals they deem of lesser importance to their constituents. But a response of "not even that" seems premature given that many resources and postings on the web that one day appear as part of the scholarly record may be missing the next. Many of the apparently most stable of the electronic resources are, at least at present, actually based on print journals, or are institution-sponsored compilations of conference papers or other "unpublished" works, or are designed as efficient ways to distribute materials by authors who are also pursuing traditional print publication. How long it will take for scholarly publications that are *only* electronic to achieve the authority, reliability, or permanence required to become a major or the primary part of the scholarly record remains to be seen.

Nevertheless, as an increasing number of scholarly works appear on the internet, editors of traditional print journals have been confronted with the question of whether these works should also be considered for journal publication. There is much to be said on both sides. On the one hand, the author of an article posted on the internet may not consider the work final; rather s/he may be seeking comments from colleagues precisely in the hope that these informal "reviews" will strengthen the product submitted to a print (or perhaps even electronic) journal. While editors of major journals generally hope to be "first with the news," they rightly should feel uncomfortable about discouraging this type of scholarly communication and the submission of more polished work. A strong case also can be made that in many cases posting an article or making it available in an electronic archive is merely an extension of the scholarly convention of distributing "working drafts" and is a more efficient substitute for postal distribution. Electronic posting also disseminates ideas quickly, and the subsequent rapid scholarly interaction can have synergistic effects on the advance of scholarship.

On the other hand, all scholars are faced with a glut of information and published work. Some have argued that devoting attention to electronic papers, which may be revised after posting, are impermanent, and—most important—are not subjected to the peer review process, leads to a collective loss of efficiency. Even more seriously, the rapid dissemination of wrong or misleading ideas can dissipate scientific energy. Some editors, particularly in the biomedical and health sciences, have argued that it is ethically and morally irresponsible for researchers to post papers that have not been vetted by peer review because they may spread invalid or inaccurate ideas or conclusions that may cause public harm or confusion (and are more likely to do so than those that have been vetted). Those who regard posting research papers that have not been peer reviewed as a violation of scientific norms argue that refusing to consider materials that have been posted electronically is an effective way to discourage this practice. Other editors are so concerned with the value of originality in their journal that ideas already "published" electronically are not considered acceptable submissions. Yet, this may have a chilling effect on scientific collaboration, and on the very ideas that generate the most interest and debate, and ultimately the journal is deprived of the opportunity to publish important work. Some editors have retreated from an extreme position on this issue. Still, many journals operate under severe space and financial constraints; faced with a choice between using reviewer resources and space for work already largely known to the relevant scientific public, or for work not widely disseminated, they opt for the latter. (The issue is similar to duplicate and overlapping publication in journals and books, or in journal articles that differ only marginally.)

For journals that emphasize "original work," the question may revolve around the meaning of *published.* Unfortunately, *Roget's Thesaurus of English Words and Phrases* (Longman's electronic version on CD, of course) is not much help. One class of root ideas refers to works "in circulation," "making the rounds," "in the headlines," "public," "known," "open," "distributed," "circularized," "disseminated," "broadcast," "ventilated," and "on the air" (my 1995 version of the publication does not include "on the net," but clearly that would fall within this meaning). Another underlying idea is "in print," or "printed," and such associated terms as "cold type," "hot metal set," "composed," "machined, etc." One wonders whether digital type can be described as "cold" or "hot," but surely many elegantly presented web documents are "composed," and all of them may be considered "machined." Moreover, since someone who passes a bad check can be charged with "uttering and publishing," definitional analysis may not be an efficient path to the heart of the matter.

What does seem clear is that some types of electronic availability are more akin to scholarly publishing than others. One important criterion is whether a work has been peer reviewed or subject to some other form of careful editorial selection. To the extent that this is

the case, the material can be similar in quality and authority to that appearing in traditional journals. To review such articles again for print publication would deny the electronic forum the very legitimacy it seeks. Electronic journals are currently struggling for legitimacy and authority in many disciplines. Within a decade that may be achieved, but for it to occur, they must be seen as simply a different publication mode, not a way station until a traditional print berth can be secured. In contrast, papers submitted to an electronic archive and made available without any selection process are more akin to conventional working papers. Even here, however, the modes of storage and distribution of electronic archives are similar in some respects to those of "on-demand" print publishers, although the latter more clearly choose what they will distribute.

A second simple criterion of "publication" is whether an author has transferred copyright to the host of the electronic site. If so, then to argue that the paper is not really "published" is to say that the copyright is invalid and the electronic site is not really part of the scholarly publication apparatus.

For all these reasons, online journals and other internet sites that have similar characteristics are properly defined as alternate publication modes and should be considered as such by authors in determining whether a potential submission to *APSR* has already been submitted or published, in whole or in part, in another journal or publication site. Undoubtedly, there will be cases in which it is difficult to decide whether a work posted to the internet is "published." A reasonable rule of thumb is to consider work published if it appears on electronic sites described as journals, run by traditional publishers, claiming peer review or other forms of filtering or editorial selection, and/or assuming copyright. At present, papers archived at electronic sites that have none of these characteristics are considered "unpublished" for purposes of *APSR* submission.

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For manuscripts containing quantitative evidence and analysis, authors should describe their empirical procedures in sufficient detail to permit reviewers to understand and evaluate what has been done and, in the event the article is accepted for publication, to permit other scholars to carry out similar analyses on other data sets. For example, for surveys, at the least, sampling procedures, response rates, and question wordings should be given; for experiments, full descriptions of experimental protocols should be provided. It is desirable for articles to be self-contained, and authors should not refer readers to other publications for descriptions of these basic research procedures.

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As part of the review process, authors may be asked to submit additional documentation if procedures are not sufficiently clear; the review process works most efficiently if such information is given in the initial submission. If readers are advised that additional information is available from the author, printed copies of that information should be submitted with the manuscript. If the amount of this supplementary information is extensive, please inquire about alternate procedures.

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Manuscripts that are largely or entirely critiques or commentaries on previously published articles will be reviewed using the same general procedures as for regular articles with one exception. In addition to the usual number of reviewers, such manuscripts will also

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Manuscripts being submitted for publication should be sent to Professor Ada W. Finifter, Editor, *American Political Science Review*, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032. Correspondence concerning manuscripts under review may be sent to the same address or, preferably, by e-mail to apsr@ssc.msu.edu. Please note that the *APSR* will not process *new* manuscripts during August 1998. *New manuscripts* should be mailed to arrive either before or after this period. *Invited revisions and reviews* may be submitted at any time and will be processed upon arrival.

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Editor's Notes

This is the last number of Volume 92 and the index for this volume can be found at the back of the issue. This issue also marks the end of my first term as Editor of the *Review*. I thank all those who have made the last three years so stimulating and challenging, and who have helped to produce the high quality research and book reviews contained in these three volumes: authors, manuscript reviewers, Editorial Board, *APSR* interns and editorial office staff at Michigan State University, book reviewers, the Book Review Editor and his assistants at both the University of Colorado and the University of California-Riverside, and the three institutions involved for their generous support of this scholarly enterprise.

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As I have done for the previous two years, I am happy to include at the back of this issue the list of manuscript reviewers who assisted in the peer review process during 1997–98. Almost 900 different scholars participated this year, and nearly 1,600 have participated over the last three years, during which we reviewed more than 1,500 manuscripts submitted for publication. This list is the only public recognition most of these individuals receive for their valuable services, but their behind-the-scenes work makes an enormous contribution to the quality of articles that appear in the *Review*.

Thank you all for your contributions to the *APSR*.

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A Behavioral Approach to the Rational Choice Theory of Collective Action
Presidential Address, American Political Science Association, 1997

ELINOR OSTROM *Indiana University*

Extensive empirical evidence and theoretical developments in multiple disciplines stimulate a need to expand the range of rational choice models to be used as a foundation for the study of social dilemmas and collective action. After an introduction to the problem of overcoming social dilemmas through collective action, the remainder of this article is divided into six sections. The first briefly reviews the theoretical predictions of currently accepted rational choice theory related to social dilemmas. The second section summarizes the challenges to the sole reliance on a complete model of rationality presented by extensive experimental research. In the third section, I discuss two major empirical findings that begin to show how individuals achieve results that are "better than rational" by building conditions where reciprocity, reputation, and trust can help to overcome the strong temptations of short-run self-interest. The fourth section raises the possibility of developing second-generation models of rationality, the fifth section develops an initial theoretical scenario, and the final section concludes by examining the implications of placing reciprocity, reputation, and trust at the core of an empirically tested, behavioral theory of collective action.

Let me start with a provocative statement. You would not be reading this article if it were not for some of our ancestors learning how to undertake collective action to solve social dilemmas. Successive generations have added to the stock of everyday knowledge about how to instill productive norms of behavior in their children and to craft rules to support collective action that produces public goods and avoids "tragedies of the commons."[1] What our ancestors and contemporaries have learned about engaging in collective action for mutual defense, child rearing, and survival is not, however, understood or explained by the extant theory of collective action.

Yet, the theory of collective action is *the* central subject of political science. It is the core of the justification for the state. Collective-action problems pervade international relations, face legislators when devising public budgets, permeate public bureaucracies, and are at the core of explanations of voting, interest group formation, and citizen control of governments in a democracy. If political scientists do not have an empirically grounded theory of collective action, then we are hand-waving at our central questions. I am afraid that we do a lot of hand-waving.

The lessons of effective collective action are not simple—as is obvious from human history and the immense tragedies that humans have endured, as well as the successes we have realized. As global relationships become even more intricately intertwined and complex, however, our survival becomes more dependent on empirically grounded scientific understanding. We have not yet developed a *behavioral theory of collective action* based on models of the individual consistent with empirical evidence about how individuals make decisions in social-dilemma situations. A behavioral commitment to theory grounded in empirical inquiry is essential if we are to understand such basic questions as why face-to-face communication so consistently enhances cooperation in social dilemmas or how structural variables facilitate or impede effective collective action.

Social dilemmas occur whenever individuals in interdependent situations face choices in which the maximization of short-term self-interest yields outcomes leaving all participants worse off than feasible alternatives. In a public-good dilemma, for example, all those who would benefit from the provision of a public good—such as pollution control, radio broadcasts, or weather forecasting—find it costly to contribute and would prefer others to pay for the good instead. If everyone follows the equilibrium strategy, then the good is not provided or is underprovided. Yet, everyone would be better off if everyone were to contribute.

Social dilemmas are found in all aspects of life, leading to momentous decisions affecting war and peace as well as the mundane relationships of keeping promises in everyday life. Social dilemmas are called by many names, including the public-good or collective-good problem (Olson 1965, P. Samuelson 1954), shirking (Alchian and Demsetz 1972), the free-rider problem (Edney 1979, Grossman and Hart 1980), moral hazard (Holmstrom 1982), the credible commitment dilemma (Williams, Collins, and Lichbach 1997), generalized social exchange (Ekeh 1974; Emerson 1972a,

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The author gratefully acknowledges the support of the National Science Foundation (Grant #SBR-9319835 and SBR-9521918), the Ford Foundation, the Bradley Foundation, and the MacArthur Foundation. My heartiest thanks go to James Alt, Jose Apesteguia, Patrick Brandt, Kathryn Firmin-Sellers, Roy Gardner, Derek Kauneckis, Fabrice Lehoucq, Margaret Levi, Thomas Lyon, Tony Matejczyk, Mike McGinnis, Trudi Miller, John Orbell, Vincent Ostrom, Eric Rasmusen, David Schmidt, Sujai Shivakumar, Vernon Smith, Catherine Tucker, George Varughese, Jimmy Walker, John Williams, Rick Wilson, Toshio Yamagishi, and Xin Zhang for their comments on earlier drafts and to Patty Dalecki for all her excellent editorial and moral support.

[1] The term "tragedy of the commons" refers to the problem that common-pool resources, such as oceans, lakes, forests, irrigation systems, and grazing lands, can easily be overused or destroyed if property rights to these resources are not well defined (see Hardin 1968).

1972b; Yamagishi and Cook 1993), the tragedy of the commons (G. Hardin 1968), and exchanges of threats and violent confrontations (Boulding 1963). The prisoners' dilemma has become the best-known social dilemma in contemporary scholarship. Among the types of individuals who are posited to face these kinds of situations are politicians (Geddes 1994), international negotiators (Sandler 1992, Snidal 1985), legislators (Shepsle and Weingast 1984), managers (Miller 1992), workers (Leibenstein 1976), long-distance traders (Greif, Milgrom, and Weingast 1994), ministers (Bullock and Baden 1977), oligopolists (Cornes, Mason, and Sandler 1986), labor union organizers (Messick 1973), revolutionaries (Lichbach 1995), homeowners (Boudreaux and Holcombe 1989), even cheerleaders (Hardy and Latané 1988), and, of course, all of us—whenever we consider trusting others to cooperate with us on long-term joint endeavors.

In prehistoric times, simple survival was dependent both on the aggressive pursuit of self-interest and on collective action to achieve cooperation in defense, food acquisition, and child rearing. Reciprocity among close kin was used to solve social dilemmas, leading to a higher survival rate for those individuals who lived in families and used reciprocity within the family (Hamilton 1964). As human beings began to settle in communities and engage in agriculture and long-distance trade, forms of reciprocity with individuals other than close kin were essential to achieve mutual protection, to gain the benefits of long-distance trading, and to build common facilities and preserve common-pool resources.[2] Evolutionary psychologists have produced substantial evidence that human beings have evolved the capacity—similar to that of learning a language—to learn reciprocity norms and general social rules that enhance returns from collective action (Cosmides and Tooby 1992). At the same time, cognitive scientists have also shown that our genetic inheritance does not give us the capabilities to do unbiased, complex, and full analyses without substantial acquired knowledge and practice as well as reliable feedback from the relevant environment. Trial-and-error methods are used to learn individual skills as well as rules and procedures that increase the joint returns individuals may obtain through specialization, coordination, and exchange. All long-enduring political philosophies have recognized human nature to be complex mixtures of the pursuit of self-interest combined with the capability of acquiring internal norms of behavior and following enforced rules when understood and perceived to be legitimate. Our evolutionary heritage has hardwired us to be boundedly self-seeking at the same time that we are capable of learning heuristics and norms, such as reciprocity, that help achieve successful collective action.

One of the most powerful theories used in contemporary social sciences—rational choice theory—helps us understand humans as self-interested, short-term maximizers. Models of complete rationality have been highly successful in predicting marginal behavior in competitive situations in which selective pressures screen out those who do not maximize external values, such as profits in a competitive market or the probability of electoral success in party competition. Thin models of rational choice have been unsuccessful in explaining or predicting behavior in one-shot or finitely repeated social dilemmas in which the theoretical prediction is that no one will cooperate. In indefinitely (or infinitely) repeated social dilemmas, standard rational choice models predict a multitude of equilibria ranging from the very best to the very worst of available outcomes without any hypothesized process for how individuals might achieve more productive outcomes and avert disasters.[3] Substantial evidence from experiments demonstrates that cooperation levels for most one-shot or finitely repeated social dilemmas far exceed the predicted levels and are systematically affected by variables that play no theoretical role in affecting outcomes. Field research also shows that individuals systematically engage in collective action to provide local public goods or manage common-pool resources without an external authority to offer inducements or impose sanctions. Simply assuming that individuals use long-range thinking "to achieve the goal of establishing and/or maintaining continued mutual cooperation" (Pruitt and Kimmel 1977, 375) is not a sufficient theory either. It does not explain why some groups fail to obtain joint outcomes easily available to them or why initial cooperation can break down.

We now have enough scholarship from multiple disciplines to expand the range of rational choice models we use. For at least five reasons, we need to formulate a behavioral theory of boundedly rational and moral behavior.

First, behavior in social dilemmas is affected by many structural variables, including size of group, heterogeneity of participants, their dependence on the benefits received, their discount rates, the type and predictability of transformation processes involved, the nesting of organizational levels, monitoring techniques, and the information available to participants.[4] In theories that predict either zero or 100% cooperation in one-shot or finitely repeated dilemmas, structural variables do not affect levels of cooperation at all. A coherent explanation of the relationship among structural variables and the likelihood of individuals solving social dilemmas depends on developing a behavioral theory of rational choice. This will allow scholars who stress structural explanations of human behavior and those who stress individual choice to find common ground, rather than continue the futile debate over whether structural

[2] The term "reciprocal altruism" is used by biologists and evolutionary theorists to refer to strategies of conditional cooperation with *nonkin* that produce higher benefits for the individuals who follow these strategies if they interact primarily with others who are reciprocators (Trivers 1971). Since these strategies benefit the individual using them in the long term, I prefer the term "reciprocity."

[3] See Farrell and Maskin (1989) for a different approach to this problem.

[4] This is only a short list of the more important variables found to affect behavior within social dilemmas (for summary overviews of this literature, see Goetze and Orbell 1988; Ledyard 1995; Lichbach 1996; E. Ostrom 1990, n.d.; E. Ostrom, Gardner, and Walker 1994; Sally 1995; Schroeder 1995).

variables or individual attributes are the most important.

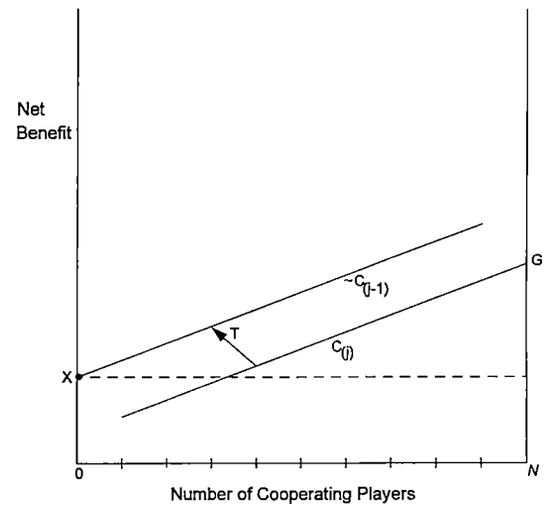
Second, scholars in all the social and some biological sciences have active research programs focusing on how groups of individuals achieve collective action. An empirically supported theoretical framework for the analysis of social dilemmas would integrate and link their efforts. Essential to the development of such a framework is a conception of human behavior that views complete rationality as one member of a family of rationality models rather than the only way to model human behavior. Competitive institutions operate as a scaffolding structure so that individuals who fail to learn how to maximize some external value are no longer in the competitive game (Alchian 1950, Clark 1995, Satz and Ferejohn 1994). If all institutions involved strong competition, then the thin model of rationality used to explain behavior in competitive markets would be more useful. Models of human behavior based on theories consistent with our evolutionary and adaptive heritage need to join the ranks of theoretical tools used in the social and biological sciences.

Third, sufficient work by cognitive scientists, evolutionary theorists, game theorists, and social scientists in all disciplines (Axelrod 1984; Boyd and Richerson 1988, 1992; Cook and Levi 1990; Güth and Kliemt 1995; Sethi and Somanathan 1996; Simon 1985, 1997) on the use of heuristics and norms of behavior, such as reciprocity, has already been undertaken. It is now possible to continue this development toward a firmer behavioral foundation for the study of collective action to overcome social dilemmas.

Fourth, much of our current public policy analysis—particularly since Garrett Hardin's (1968) evocative paper, "The Tragedy of the Commons"—is based on an assumption that rational individuals are helplessly trapped in social dilemmas from which they cannot extract themselves without inducement or sanctions applied from the outside. Many policies based on this assumption have been subject to major failure and have exacerbated the very problems they were intended to ameliorate (Arnold and Campbell 1986, Baland and Platteau 1996, Morrow and Hull 1996). Policies based on the assumptions that individuals can learn how to devise well-tailored rules and cooperate conditionally when they participate in the design of institutions affecting them are more successful in the field (Berkes 1989, Bromley et al. 1992, Ellickson 1991, Feeny et al. 1990, McCay and Acheson 1987, McKean and Ostrom 1995, Pinkerton 1989, Yoder 1994).

Fifth, the image of citizens we provide in our textbooks affects the long-term viability of democratic regimes. Introductory textbooks that presume rational citizens will be passive consumers of political life—the masses—and focus primarily on the role of politicians and officials at a national level—the elite—do not inform future citizens of a democratic polity of the actions they need to know and can undertake. While many political scientists claim to eschew teaching the normative foundations of a democratic polity, they actually introduce a norm of cynicism and distrust without providing a vision of how citizens could do anything to challenge corruption, rent seeking,[5] or poorly designed policies.

FIGURE 1. *N*-Person Social Dilemma



Note: *N* players choose between cooperating (*C*) or not cooperating (~*C*). When *j* individuals cooperate, their payoffs are always lower than the *j*-1 individuals who do not cooperate. The predicted outcome is that no one will cooperate and all players will receive *X* benefits. The temptation (*T*) not to cooperate is the increase in benefit any cooperator would receive for switching to not cooperating. If all cooperate, they all receive *G-X* more benefits than if all do not cooperate.

The remainder of this article is divided into six sections. In the first I briefly review the theoretical predictions of currently accepted rational choice theory related to social dilemmas. The next will summarize the challenge to the sole reliance on a complete model of rationality presented by extensive experimental research. Then I examine two major empirical findings that begin to show how individuals achieve results that are "better than rational" (Cosmides and Tooby 1994) by building conditions in which reciprocity, reputation, and trust can help to overcome the strong temptations of short-run self-interest. The following section raises the possibility of developing second-generation models of rationality, and the next develops an initial theoretical scenario. I conclude by examining the implications of placing reciprocity, reputation, and trust at the core of an empirically tested, behavioral theory of collective action.

THEORETICAL PREDICTIONS FOR SOCIAL DILEMMAS

The term "social dilemma" refers to a large number of situations in which individuals make independent choices in an interdependent situation (Dawes 1975, 1980; R. Hardin 1971). In all *N*-person social dilemmas, a set of participants has a choice of contributing (*C*) or not contributing (~*C*) to a joint benefit. While I represent this as an either-or choice in Figure 1, it

[5] The term "rent seeking" refers to nonproductive activities directed toward creating opportunities for profits higher than would be obtained in an open, competitive market.

frequently is a choice of how much to contribute rather than whether to contribute or not.

If everyone contributes, they get a net positive benefit (G). Everyone faces a temptation (T) to shift from the set of contributors to the set of those who do not contribute. The theoretical prediction is that everyone will shift and that no one will contribute. If this happens, then the outcome will be at the intercept. The difference between the predicted outcome and everyone contributing is $G - X$. Since the less-valued payoff is at a Nash equilibrium, no one is independently motivated to change his or her choice, given the choices of other participants. These situations are dilemmas because at least one outcome exists that yields greater advantage for *all* participants. Thus, a Pareto-superior alternative exists, but rational participants making isolated choices are not predicted to realize this outcome. A conflict is posed between individual and group rationality. The problem of collective action raised by social dilemmas is to find a way to avoid Pareto-inferior equilibria and to move closer to the optimum. Those who find ways to coordinate strategies in some fashion receive a "cooperators' dividend" equal to the difference between the predicted outcome and the outcome achieved.

Many models of social dilemmas exist in the literature (see Schelling 1978 and Lichbach 1996 for reviews of alternative formalizations). In all models, a set of individuals is involved in a game in which a strategy leading to a Nash equilibrium for a single iteration of the game yields less than an optimal outcome for all involved. The equilibrium is thus Pareto inferior. The optimal outcome could be achieved if those involved "cooperated" by selecting strategies other than those prescribed by an equilibrium solution to a noncooperative game (Harsanyi and Selten 1988). Besides these assumptions regarding the structure of payoffs in the one-shot version of the game, other assumptions are made in almost all formal models of social dilemmas. (1) All participants have common knowledge of the exogenously fixed structure of the situation and of the payoffs to be received by all individuals under all combinations of strategies. (2) Decisions about strategies are made independently, often simultaneously. (3) In a symmetric game, all participants have available the same strategies. (4) No external actor (or central authority) is present to enforce agreements among participants about their choices.

When such a game is finitely repeated, participants are assumed to solve the game through backward induction. Assumptions about the particular payoff functions differ. Rather than describe the Nash equilibrium and Pareto-efficient outcome for all models considered in this article, aggregate behavior consistent with the Nash equilibrium will be described as zero cooperation, while behavior consistent with the efficient outcome will be described as 100% cooperation.

The grim predictions evoked considerable empirical challenges as well as important theoretical breakthroughs. The predictions ran counter to so many everyday experiences that some scholars turned to survey and field studies to examine the level of voluntary contributions to public goods (see Lichbach 1995 for a summary). Others turned to the experimental lab and confirmed much higher than predicted levels of cooperation in one-shot experiments. Game theorists were challenged to rethink their own firm conclusions and to pose new models of when cooperation might emerge (see Benoit and Krishna 1985).

The introduction of two kinds of uncertainty into repeated games—about the number of repetitions and about the types of players participating in a social dilemma—has led to more optimistic predictions. When individuals, modeled as fully rational actors with low discount rates, interact in a repeated social dilemma whose end point is determined stochastically, it is now theoretically well established that it is *possible* for them to achieve optimal or near optimal outcomes and avoid the dominant strategies of one-shot and finitely repeated games that yield suboptimal outcomes (Fudenberg and Maskin 1986). This is possible when players achieve self-enforcing equilibria by committing themselves to punish noncooperators sufficiently to deter noncooperation. Kreps et al. (1982) introduced a second kind of uncertainty related to whether all the players use complete rationality as their guide to action. The probability of the presence of an "irrational" player, who reciprocates cooperation with cooperation, is used as the grounds for a completely rational player to adopt the strategy of cooperating early in a sequence of games and switching to noncooperation at the end. Once either of these two forms of uncertainty is introduced, the number of possible equilibria explode in number (Abreau 1988). Everything is predicted: optimal outcomes, the Pareto-inferior Nash equilibria, and everything in between.

To generate predictions other than noncooperation, theorists using standard rational choice theory have found it necessary to assume real uncertainty about the duration of a situation or to assume that some players are "irrational" in their willingness to reciprocate cooperation with cooperation. To assume that if some players *irrationally* choose reciprocity, then others can *rationally* choose reciprocity is a convoluted explanation—to say the least—of the growing evidence that reciprocity is a core norm used by many individuals in social dilemma situations.

THE LACK OF A GENERAL FIT

In all the social sciences, experiments have been conducted on various types of social dilemmas for several decades. While some scholars question the value of laboratory experiments for testing the predictions of major theories in the social sciences, this method has many advantages. First, one can design experiments that test multiple predictions from the same theory under controlled conditions. Second, replication is feasible. Third, researchers can challenge whether a particular design adequately captures the theoretically posited variables and conduct further experiments to ascertain how changes in a design affect outcomes. The evidence discussed below is based on multiple studies by diverse research teams. Fourth, experimental meth-

ods are particularly relevant for studying human choice under diverse institutional arrangements. Subjects in experimental studies draw on the modes of analysis and values they have learned throughout their lives to respond to diverse incentive structures. Experiments thus allow one to test precisely whether individuals behave within a variety of institutional settings as predicted by theory (Plott 1979, Smith 1982).

In this section, I will summarize four consistently replicated findings that directly challenge the general fit between behavior observed in social-dilemma experiments and the predictions of noncooperative game theory using complete rationality and complete information for one-shot and finitely repeated social dilemmas. I focus first on the fit between theory and behavior, because the theoretical predictions are unambiguous and have influenced so much thinking across the social sciences. Experiments on market behavior do fit the predictions closely (see Davis and Holt 1993 for an overview). If one-shot and finitely repeated social-dilemma experiments were to support strongly the predictions of noncooperative game theory, then we would have a grounded theory with close affinities to a vast body of economic theory for which there is strong empirical support. We would need to turn immediately to the problem of indefinitely repeated situations for which noncooperative game theory faces an embarrassment of too many equilibria. As it turns out, we have a different story to tell. The four general findings are as follows.

1. High levels of initial cooperation are found in most types of social dilemmas, but the levels are consistently less than optimal.
2. Behavior is not consistent with backward induction in finitely repeated social dilemmas.
3. Nash equilibrium strategies are not good predictors at the individual level.
4. Individuals do not learn Nash equilibrium strategies in repeated social dilemmas.

High but Suboptimal Levels of Initial Cooperation

Most experimental studies of social dilemmas with the structure of a public-goods provision problem have found levels of cooperative actions in one-shot games, or in the first rounds of a repeated game, that are significantly above the predicted level of zero.[6] "In a wide variety of treatment conditions, participants rather persistently contributed 40 to 60 percent of their token endowments to the [public good], far in excess of the 0 percent contribution rate consistent with a Nash equilibrium" (Davis and Holt 1993, 325). Yet, once an experiment is repeated, cooperation levels in public-good experiments tend to decline. The individual variation across experiment sessions can be very great.[7] While many have focused on the unexpectedly high rates of cooperation, it is important to note that in sparse institutional settings with no feedback about individual contributions, cooperation levels never reach the optimum. Thus, the prediction of zero levels of cooperation can be rejected, but cooperation at a suboptimal level is consistently observed in sparse institutional settings.

Behavior in Social Dilemmas Inconsistent with Backward Induction

In all finitely repeated experiments, players are predicted to look ahead to the last period and determine what they would do in that period. In the last period, there is no future interaction; the prediction is that they will not cooperate in that round. Since that choice would be determined at the beginning of an experiment, the players are presumed to look at the second-to-last period and ask themselves what they would do there. Given that they definitely would not cooperate in the last period, it is assumed that they also would not cooperate in the second-to-last period. This logic would then extend backward to the first round (Luce and Raiffa 1957, 98–9).

While backward induction is still the dominant method used in solving finitely repeated games, it has been challenged on theoretical grounds (Binmore 1997, R. Hardin 1997). Furthermore, as discussed above, uncertainty about whether others use norms like tit-for-tat rather than follow the recommendations of a Nash equilibrium may make it rational for a player to signal a willingness to cooperate in the early rounds of an iterated game and then defect at the end (Kreps et al. 1982). What is clearly the case from experimental evidence is that players do not use backward induction in their decision-making plans in an experimental laboratory. Amnon Rapoport (1997, 122) concludes from a review of several experiments focusing on resource dilemmas that "subjects are not involved in or capable of backward induction."[8]

[6] See Isaac, McCue, and Plott 1985; Kim and Walker 1984; Marwell and Ames 1979, 1980, 1981; Orbell and Dawes 1991, 1993; Schneider and Pommerehne 1981. An important exception to this general finding is that when subjects are presented with an experimental protocol with an opportunity to invest tokens in a common-pool resource (the equivalent of harvesting from a common pool), they tend to *overinvest* substantially in the initial rounds (see E. Ostrom, Gardner, and Walker 1994 and comparison of public goods and common-pool resource experiments in Goetze 1994 and E. Ostrom and Walker 1997). Ledyard (1995) considers common-pool resource dilemmas to have the same underlying structure as public good dilemmas, but behavior in common-pool resource experiments without communication is consistently different from public good experiments without communication. With repetition, outcomes in common-pool resource experiments approach the Nash equilibrium from below rather than from above, as is typical in public good experiments.

[7] In a series of eight experiments with different treatments conducted by Isaac, Walker, and Thomas (1984), in which the uniform theoretical prediction was zero contributions, contribution rates varied from nearly 0% to around 75% of the resources available to participants.

[8] Subjects in Centipede games also do not use backward induction (see McKelvey and Palfrey 1992).

Nash Equilibrium Strategies Do Not Predict Individual Behavior in Social Dilemmas

From the above discussion, it is obvious that individuals in social dilemmas tend not to use the predicted Nash equilibrium strategy, even though this is a good predictor at both an individual and group level in other types of situations. While outcomes frequently approach Nash equilibria at an aggregate level, the variance of individual actions around the mean is extremely large. When groups of eight subjects made appropriation decisions in repeated common-pool resource experiments of 20 to 30 rounds, the unique symmetric Nash equilibrium strategy was never played (Walker, Gardner, and Ostrom 1990). Nor did individuals use Nash equilibrium strategies in repeated public good experiments (Dudley 1993; Isaac and Walker 1991, 1993). In a recent set of thirteen experiments involving seven players making ten rounds of decisions without communication or any other institutional structure, Walker et al. (1997) did not observe a single individual choice of a symmetric Nash equilibrium strategy in the 910 opportunities available to subjects. Chan et al. (1996, 58) also found little evidence to support the use of Nash equilibria when they examined the effect of heterogeneity of income on outcomes: "It is clear that the outcomes of the laboratory sessions reported here cannot be characterized as Nash equilibria outcomes."

Individuals Do Not Learn Nash Equilibrium Strategies in Social Dilemmas

In repeated experiments without communication or other facilitating institutional conditions, levels of cooperation fall (rise) toward the Nash equilibrium in public-good (common-pool resource) experiments. Some scholars have speculated that it just takes some time and experience for individuals to learn Nash equilibrium strategies (Ledyard 1995). But this does not appear to be the case. In all repeated experiments, there is considerable pulsing as subjects obtain outcomes that vary substantially with short spurts of increasing and decreasing levels of cooperation, while the general trend is toward an aggregate that is consistent with a Nash equilibrium (Isaac, McCue, and Plott 1985; E. Ostrom, Gardner, and Walker 1994).[9] Furthermore, there is substantial variation in the strategies followed by diverse participants within the same game (Dudley 1993; Isaac and Walker 1988b; E. Ostrom, Gardner, and Walker 1994).

It appears that subjects learn something other than Nash strategies in finitely repeated experiments. Isaac, Walker, and Williams (1994) compare the rate of decay when experienced subjects are explicitly told that an experiment will last 10, 40, or 60 rounds. The rate of decay of cooperative actions is inversely related to the number of decision rounds. Instead of learning the noncooperative strategy, subjects appear to be learning how to cooperate at a moderate level for even longer periods. Cooperation rates approach zero only in the last few periods, whenever these occur.

TWO INTERNAL WAYS OUT OF SOCIAL DILEMMAS

The combined effect of these four frequently replicated, general findings represents a strong rejection of the predictions derived from a complete model of rationality. Two more general findings are also contrary to the predictions of currently accepted models. At the same time, they also begin to show how individuals are able to obtain results that are substantially "better than rational" (Cosmides and Tooby 1994), at least as *rational* has been defined in currently accepted models. The first is that simple, cheap talk allows individuals an opportunity to make conditional promises to one another and potentially to build trust that others will reciprocate. The second is the capacity to solve second-order social dilemmas that change the structure of the first-order dilemma.

Communication and Collective Action

In noncooperative game theory, players are assumed to be unable to make enforceable agreements.[10] Thus, communication is viewed as cheap talk (Farrell 1987). In a social dilemma, self-interested players are expected to use communication to try to convince others to cooperate and promise cooperative action, but then to choose the Nash equilibrium strategy when they make their private decision (Barry and Hardin 1982, 381; Farrell and Rabin 1996, 113).[11] Or, as Gary Miller (1992, 25) expresses it: "It is obvious that simple communication is not sufficient to escape the dilemma."[12]

From this theoretical perspective, face-to-face communication should make no difference in the outcomes achieved in social dilemmas. Yet, consistent, strong, and replicable findings are that substantial increases in the levels of cooperation are achieved when individuals are allowed to communicate face to face.[13] This holds

[9] The pulsing cannot be explained using a complete model of rationality, but it can be explained as the result of a heuristic used by subjects to raise or lower their investments depending upon the average return achieved on the most recent round (see E. Ostrom, Gardner, and Walker 1994).

[10] In cooperative game theory, in contrast, it is assumed that players can communicate and make *enforceable agreements* (Harsanyi and Selten 1988, 3).

[11] In social-dilemma experiments, subjects make anonymous decisions and are paid privately. The role of cheap talk in coordination experiments is different since there is no dominant strategy. In this case, preplay communication may help players coordinate on one of the possible equilibria (see Cooper, DeJong, and Forsythe 1992).

[12] As Aumann (1974) cogently points out, the players are faced with the problem that whatever they agree upon has to be self-enforcing. That has led Aumann and most game theorists to focus entirely on Nash equilibria which, once reached, are self-enforcing. In coordination games, cheap talk can be highly efficacious.

[13] See E. Ostrom, Gardner, and Walker 1994 for extensive citations to studies showing a positive effect of the capacity to communicate. Dawes, McTavish, and Shaklee 1977; Frey and Bohnet 1996; Hackett, Schlager, and Walker 1994; Isaac and Walker 1988a, 1991; Orbell, Dawes, and van de Kragt 1990; Orbell, van de Kragt, and Dawes 1988, 1991; E. Ostrom, Gardner, and Walker 1994; Sally 1995.

true across all types of social dilemmas studied in laboratory settings and in both one-shot and finitely repeated experiments. In a meta-analysis of more than 100 experiments involving more than 5,000 subjects conducted by economists, political scientists, sociologists, and social psychologists, Sally (1995) finds that opportunities for face-to-face communication in one-shot experiments significantly raise the cooperation rate, on average, by more than 45 percentage points. When subjects are allowed to talk before each decision round in repeated experiments, they achieve 40 percentage points more on average than in repeated games without communication. No other variable has as strong and consistent an effect on results as face-to-face communication. Communication even has a robust and positive effect on cooperation levels when individuals are not provided with feedback on group decisions after every round (Cason and Khan 1996).

The efficacy of communication is related to the capability to talk face to face. Sell and Wilson (1991, 1992), for example, developed a public-good experiment in which subjects could signal promises to cooperate via their computer terminal. There was much less cooperation than in the face-to-face experiments using the same design (Isaac and Walker 1988a, 1991). Rocco and Warglien (1995) replicated all aspects of prior common-pool resource experiments, including the efficacy of face-to-face communication.[14] They found, however, that subjects who had to rely on computerized communication did not achieve the same increase in efficiency as did those who were able to communicate face to face.[15] Palfrey and Rosenthal (1988) report that no significant difference occurred in a provision point public-good experiment in which subjects could send a computerized message stating whether they intended to contribute.

The reasons offered by those doing experimental research for why communication facilitates cooperation include (1) transferring information from those who can figure out an optimal strategy to those who do not fully understand what strategy would be optimal, (2) exchanging mutual commitment, (3) increasing trust and thus affecting expectations of others' behavior, (4) adding additional values to the subjective payoff structure, (5) reinforcement of prior normative values, and (6) developing a group identity (Davis and Holt 1993; Orbell, Dawes, and van de Kragt 1990; Orbell, van de Kragt, and Dawes 1988; E. Ostrom and Walker 1997). Carefully crafted experiments demonstrate that the effect of communication is not primarily due to the first reason. When information about the individual strategy that produces an optimal joint outcome is clearly presented to subjects who are not able to communicate, the information makes little difference in outcomes achieved (Isaac, McCue, and Plott 1985; Moir 1995).

[14] Moir (1995) also replicated these findings with face-to-face communication.

[15] Social psychologists have found that groups who perform tasks using electronic media do much better if they have had an opportunity to work face to face prior to the use of electronic communication only (Hollingshead, McGrath, and O'Connor 1993).

Consequently, exchanging mutual commitment, increasing trust, creating and reinforcing norms, and developing a group identity appear to be the most important processes that make communication efficacious. Subjects in experiments do try to extract mutual commitment from one another to follow the strategy they have identified as leading to their best joint outcomes. They frequently go around the group and ask each person to promise the others that they will follow the joint strategy. Discussion sessions frequently end with such comments as: "Now remember everyone that we all do much better if we all follow *X* strategy" (see transcripts in E. Ostrom, Gardner, and Walker 1994). In repeated experiments, subjects use communication opportunities to lash out verbally at unknown individuals who did not follow mutually agreed strategies, using such evocative terms as scumbuckets and finks. Orbell, van de Kragt, and Dawes (1988) summarize the findings from ten years of research on one-shot public-good experiments by stressing how many mutually reinforcing processes are evoked when communication is allowed.[16] Without increasing mutual trust in the promises that are exchanged, however, expectations of the behavior of others will not change. Given the very substantial difference in outcomes, communication is most likely to affect individual trust that others will keep to their commitments. As discussed below, the relationships among trust, conditional commitments, and a reputation for being trustworthy are key links in a second-generation theory of boundedly rational and moral behavior.

As stakes increase and it is difficult to monitor individual contributions, communication becomes less efficacious, however. E. Ostrom, Gardner, and Walker (1994) found that subjects achieved close to fully optimal results when each subject had relatively low endowments and was allowed opportunities for face-to-face communication. When endowments were substantially increased—increasing the temptation to cheat on prior agreements—subjects achieved far more in communication experiments as contrasted to non-communication experiments but less than in small-stake situations. Failures to achieve collective action in field settings in which communication has been feasible point out that communication alone is not a sufficient mechanism to assure successful collective action under all conditions.

Innovation and Collective Action

Changing the rules of a game or using scarce resources to punish those who do not cooperate or keep agreements are usually not considered viable options for participants in social dilemmas, since these actions create public goods. Participants face a second-order social dilemma (of equal or greater difficulty) in any effort to use costly sanctions or change the structure of a game (Oliver 1980). The predicted outcome of any effort to solve a second-order dilemma is failure.

[16] See also Banks and Calvert (1992a, 1992b) for a discussion of communication in incomplete information games.

Yet, participants in many field settings and experiments do exactly this. Extensive research on how individuals have governed and managed common-pool resources has documented the incredible diversity of rules designed and enforced by participants themselves to change the structure of underlying social-dilemma situations (Blomquist 1992, Bromley et al. 1992, Lam n.d., McKean 1992, E. Ostrom 1990, Schlager 1990, Schlager and Ostrom 1993, Tang 1992). The particular rules adopted by participants vary radically to reflect local circumstances and the cultural repertoire of acceptable and known rules used generally in a region. Nevertheless, general design principles characterize successfully self-organized, sustainable, local, regional, and international regimes (E. Ostrom 1990). Most robust and long-lasting common-pool regimes involve clear mechanisms for monitoring rule conformance and graduated sanctions for enforcing compliance. Thus, few self-organized regimes rely entirely on communication alone to sustain cooperation in situations that generate strong temptations to break mutual commitments. Monitors—who may be participants themselves—do not use strong sanctions for individuals who rarely break rules. Modest sanctions indicate to rule breakers that their lack of conformance has been observed by others. By paying a modest fine, they rejoin the community in good standing and learn that rule infractions are observed and sanctioned. Repeated rule breakers are severely sanctioned and eventually excluded from the group. Rules meeting these design principles reinforce contingent commitments and enhance the trust participants have that others are also keeping their commitments.

In field settings, innovation in rules usually occurs in a continuous trial-and-error process until a rule system is evolved that participants consider yields substantial net benefits. Given the complexity of the physical world that individuals frequently confront, they are rarely ever able to "get the rules right" on the first or second try (E. Ostrom 1990). In highly unpredictable environments, a long period of trial and error is needed before individuals can find rules that generate substantial positive net returns over a sufficiently long time horizon. Nonviolent conflict may be a regular feature of successful institutions when arenas exist to process conflict cases regularly and, at times, to innovate new rules to cope with conflict more effectively (V. Ostrom 1987; V. Ostrom, Feeny, and Picht 1993).

In addition to the extensive field research on changes that participants make in the structure of situations they face, subjects in a large number of experiments have also solved second-order social dilemmas and consequently moved the outcomes in their first-order dilemmas closer to optimal levels (Dawes, Orbell, and van de Kragt 1986; Messick and Brewer 1983; Rutte and Wilke 1984; Sato 1987; van de Kragt, Orbell, and Dawes 1983; Yamagishi 1992). Toshio Yamagishi (1986), for example, conducted experiments with subjects who had earlier completed a questionnaire including items from a scale measuring trust. Subjects who ranked higher on the trust scale consistently contributed about 20% more to collective goods than those who ranked lower. When given an opportunity to contribute to a substantial "punishment fund" to be used to fine the individual who contributed the least to their joint outcomes, however, low-trusting individuals contributed significantly more to the punishment fund and also achieved the highest level of cooperation. In the last rounds of this experiment, they were contributing 90% of their resources to the joint fund. These results, which have now been replicated with North American subjects (Yamagishi 1988a, 1988b), show that individuals who are initially the least trusting are willing to contribute to sanctioning systems and then respond more to a change in the structure of the game than those who are initially more trusting.

E. Ostrom, Walker, and Gardner (1992) also examined the willingness of subjects to pay a "fee" in order to "fine" another subject. Instead of the predicted zero use of sanctions, individuals paid fees to fine others at a level significantly above zero.[17] When sanctioning was combined with a single opportunity to communicate or a chance to discuss and vote on the creation of their own sanctioning system, outcomes improved dramatically. With only a single opportunity to communicate, subjects were able to obtain an average of 85% of the optimal level of investments (67% with the costs of sanctioning subtracted). Those subjects who met face to face and agreed by majority vote on their own sanctioning system achieved 93% of optimal yield. The level of defections was only 4%, so that the costs of the sanctioning system were low, and net benefits were at a 90% level (E. Ostrom, Walker, and Gardner 1992).

Messick and his colleagues have undertaken a series of experiments designed to examine the willingness of subjects to act collectively to change institutional structures when facing common-pool resource dilemmas (see Messick et al. 1983, Samuelson et al. 1984, C. Samuelson and Messick 1986). In particular, they have repeatedly given subjects the opportunity to relinquish their individual decisions concerning withdrawals from the common resource to a leader who is given the authority to decide for the group. They have found that "people want to change the rules and bring about structural change when they observe that the common resource is being depleted" (C. Samuelson and Messick 1995, 147). Yet, simply having an unequal distribution of outcomes is not a sufficient inducement to affect the decision whether to change institutional structure.

What do these experiments tell us? They complement the evidence from field settings and show that individuals temporarily caught in a social-dilemma structure are likely to invest resources to innovate and change the structure itself in order to improve joint outcomes. They also strengthen the earlier evidence that the currently accepted, noncooperative game-

[17] Furthermore, they invested more when the fine was lower or when it was more efficacious, and they tended to direct their fines to those who had invested the most on prior rounds. Given the cost of the sanctioning mechanism, subjects tended to overuse it and to end up with a less efficient outcome after sanctioning costs were subtracted from their earnings. This finding is consistent with the Boyd and Richerson (1992) result that moralistic strategies may result in negative net outcomes.

theoretical explanation relying on a particular model of the individual does not adequately predict behavior in one-shot and finitely repeated social dilemmas. Cooperative game theory does not provide a better explanation. Since both cooperative and noncooperative game theory predict extreme values, neither provides explanations for the conditions that tend to enhance or detract from cooperation levels.

The really big puzzle in the social sciences is the development of a consistent theory to explain why cooperation levels vary so much and why specific configurations of situational conditions increase or decrease cooperation in first- or second-level dilemmas. This question is important not only for our scientific understanding but also for the design of institutions to facilitate individuals' achieving higher levels of productive outcomes in social dilemmas. Many structural variables affect the particular innovations chosen and the sustainability and distributional consequences of these institutional changes (Knight 1992). A coherent theory of institutional change is not within reach, however, with a theory of individual choice that predicts no innovation will occur. We need a second-generation theory of boundedly rational, innovative, and normative behavior.

TOWARD SECOND-GENERATION MODELS OF RATIONALITY

First-generation models of rational choice are powerful engines of prediction when strong competition eliminates players who do not aggressively maximize immediate external values. While incorrectly confused with a general theory of human behavior, complete rationality models will continue to be used productively by social scientists, including the author. But the thin model of rationality needs to be viewed, as Selten (1975) points out, as the limiting case of bounded or incomplete rationality. Consistent with all models of rational choice is a general theory of human behavior that views all humans as complex, fallible learners who seek to do as well as they can given the constraints that they face and who are able to learn heuristics, norms, rules, and how to craft rules to improve achieved outcomes.

Learning Heuristics, Norms, and Rules

Because individuals are boundedly rational, they do not calculate a complete set of strategies for every situation they face. Few situations in life generate information about all potential actions that one can take, all outcomes that can be obtained, and all strategies that others can take. In a model of complete rationality, one simply assumes this level of information. In field situations, individuals tend to use heuristics—rules of thumb—that they have learned over time regarding responses that tend to give them good outcomes in particular kinds of situations. They bring these heuristics with them when they participate in laboratory experiments. In frequently encountered, repetitive situations, individuals learn better and better heuristics that are tailored to particular situations. With repetition, sufficiently large stakes, and strong competition, individuals may learn heuristics that approach best-response strategies.

In addition to learning instrumental heuristics, individuals also learn to adopt and use norms and rules. By *norms* I mean that the individual attaches an internal valuation—positive or negative—to taking particular types of action. Crawford and Ostrom (1995) refer to this internal valuation as a delta parameter that is added to or subtracted from the objective costs of an action.[18] Andreoni (1989) models individuals who gain a "warm glow" when they contribute resources that help others more than they help themselves in the short term. Knack (1992) refers to negative internal valuations as "duty."[19] Many norms are learned from interactions with others in diverse communities about the behavior that is expected in particular types of situations (Coleman 1987). The change in preferences represents the internalization of particular moral lessons from life (or from the training provided by one's elders and peers).[20] The strength of the commitment (Sen 1977) made by an individual to take particular types of future actions (telling the truth, keeping promises) is reflected in the size of the delta parameter. After experiencing repeated benefits from other people's cooperative actions, an individual may resolve that s/he should always initiate cooperative actions in the future.[21] Or, after many experiences of being the "sucker" in such experiences, an individual may resolve never to be the first to cooperate.

Since norms are learned in a social milieu, they vary substantially across cultures, across individuals within any one culture, within individuals across different types of situations they face, and across time within any particular situation. The behavioral implications of assuming that individuals acquire norms do not vary substantially from the assumption that individuals learn to use heuristics. One may think of norms as heuristics that individuals adopt from a moral perspective, in that these are the kinds of actions they wish to follow in living their life. Once some members of a population acquire norms of behavior, they affect the expectations of others.

By *rules* I mean that a group of individuals has

[18] When constructing formal models, one can include overt delta parameters in the model (see Crawford and Ostrom 1995, Palfrey and Rosenthal 1988). Alternatively, one can assume that these internal delta parameters lead individuals to enter new situations with differing probabilities that they will follow norms such as reciprocity. These probabilities not only vary across individuals but also increase or decrease as a function of the specific structural parameters of the situation and, in repeated experiments, the patterns of behavior and outcomes achieved in that situation over time.

[19] The change in valuations that an individual may attach to an action-outcome linkage may be generated strictly internally or may be triggered by external observation and, thus, a concern with how others will evaluate the normative appropriateness of actions.

[20] Gouldner (1960, 171) considers norms of reciprocity to be universal and as important in most cultures as incest taboos, even though the "concrete formulations may vary with time and place."

[21] See Selten (1986) for a discussion of his own and John Harsanyi's (1977) conception of "rule utilitarianism" as contrasted to "act utilitarianism."

developed shared understandings that certain actions in particular situations must, must not, or may be undertaken and that sanctions will be taken against those who do not conform. The distinction between internalized but widely shared norms for what are appropriate actions in broad types of situations and rules that are self-consciously adopted for use in particular situations is at times difficult to draw when doing fieldwork. Analytically, individuals can be thought of as learning norms of behavior that are general and fit a wide diversity of particular situations. Rules are artifacts related to particular actions in specific situations (V. Ostrom 1980, 1997). Rules are created in private associations as well as in more formalized public institutions, where they carry the additional legal weight of being enforced legal enactments.[22] Rules can enhance reciprocity by making mutual commitments clear and overt. Alternatively, rules can assign authority to act so that benefits and costs are distributed inequitably and thereby destroy reliance on positive norms.

Reciprocity: An Especially Important Class of Norms

That humans rapidly learn and effectively use heuristics, norms, and rules is consistent with the lessons learned from evolutionary psychology (see Barkow, Cosmides, and Tooby 1992), evolutionary game theory (see Güth and Kliemt 1996, Hirshleifer and Rasmusen 1989),[23] biology (Trivers 1971), and bounded rationality (Selten 1990, 1991; Selten, Mitzkewitz, and Uhlich 1997; Simon 1985). Humans appear to have evolved specialized cognitive modules for diverse tasks, including making sense out of what is seen (Marr 1982), inferring rules of grammar by being exposed to adult speakers of a particular language (Pinker 1994), and increasing their long-term returns from interactions in social dilemmas (Cosmides and Tooby 1992). Humans dealt with social dilemmas related to rearing and protecting offspring, acquiring food, and trusting one another to perform future promised action millennia before such oral commitments could be enforced by external authorities (de Waal 1996). Substantial evidence has been accumulated (and reviewed in Cosmides and Tooby 1992) that humans inherit a strong capacity to learn reciprocity norms and social rules that enhance the opportunities to gain benefits from coping with a multitude of social dilemmas.

Reciprocity refers to a family of strategies that can be used in social dilemmas involving (1) an effort to identify who else is involved, (2) an assessment of the likelihood that others are conditional cooperators, (3) a decision to cooperate initially with others if others are trusted to be conditional cooperators, (4) a refusal to cooperate with those who do not reciprocate, and (5) punishment of those who betray trust. All reciprocity norms share the common ingredients that individuals tend to react to the positive actions of others with positive responses and the negative actions of others with negative responses. Reciprocity is a basic norm taught in all societies (see Becker 1990, Blau 1964, Gouldner 1960, Homans 1961, Oakerson 1993, V. Ostrom 1997, Thibaut and Kelley 1959).

By far the most famous reciprocal strategy—tit-for-tat—has been the subject of considerable study from an evolutionary perspective. In simulations, pairs of individuals are sampled from a population, and they then interact with one another repeatedly in a prisoners' dilemma game. Individuals are each modeled as if they had inherited a strategy that included the fixed maxims of always cooperate, always defect, or the reciprocating strategy of tit-for-tat (cooperate first, and then do whatever the others did in the last round). Axelrod and Hamilton (1981) and Axelrod (1984) have shown that when individuals are grouped so that they are more likely to interact with one another than with the general population, and when the expected number of repetitions is sufficiently large, reciprocating strategies such as tit-for-tat can successfully invade populations composed of individuals following an all-defect strategy. The size of the population in which interactions are occurring may need to be relatively small for reciprocating strategies to survive potential errors of players (Bendor and Mookherjee 1987; but see Boyd and Richerson 1988, 1992; Hirshleifer and Rasmusen 1989; Yamagishi and Takahashi 1994).

The reciprocity norms posited to help individuals gain larger cooperators' dividends depend upon the willingness of participants to use retribution to some degree. In tit-for-tat, for example, an individual must be willing to "punish" a player who defected in the last round by defecting in the current round. In grim trigger, an individual must be willing to cooperate initially but then "punish" everyone for the rest of the game if any defection is noticed in the current round.[24]

Human beings do not inherit particular reciprocity norms via a biological process. The argument is more subtle. Individuals inherit an acute sensitivity for learning norms that increase their own long-term benefits when confronting social dilemmas with others who have learned and value similar norms. The process of growing up in any culture provides thousands of incidents (learning trials) whereby parents, siblings, friends, and teachers provide the specific content of the type of mutual expectations prevalent in that culture. As Mueller (1986) points out, the first dilemmas that humans encounter are as children. Parents reward and punish them until cooperation is a learned response. In

[22] Crawford and Ostrom (1995) discuss these issues in greater depth. See also Piaget ([1932] 1969).

[23] The evolutionary approach has been strongly influenced by the work of Robert Axelrod (see, in particular, Axelrod 1984, 1986; Axelrod and Hamilton 1981; and Axelrod and Keohane 1985).

[24] The grim trigger has been used repeatedly as a support for cooperative outcomes in infinitely (or indefinitely) repeated games (Fudenberg and Maskin 1986). In games in which substantial joint benefits are to be gained over the long term from mutual cooperation, the threat of the grim trigger is thought to be sufficient to encourage everyone to cooperate. A small error on the part of one player or exogenous noise in the payoff function, however, makes this strategy a dangerous one to use in larger groups, where the cooperators' dividend may also be substantial.

the contemporary setting, corporate managers strive for a trustworthy corporate reputation by continuously reiterating and rewarding the use of key principles or norms by corporate employees (Kreps 1990).

Since particular reciprocity norms are *learned*, not everyone learns to use the same norms in all situations. Some individuals learn norms of behavior that are not so "nice." Clever and unscrupulous individuals may learn how to lure others into dilemma situations and then defect on them. It is possible to gain substantial resources by such means, but one has to hide intentions and actions, to keep moving, or to gain access to power over others. In any group composed only of individuals who follow reciprocity norms, skills in detecting and punishing cheaters could be lost. If this happens, it will be subject to invasion and substantial initial losses by clever outsiders or local deviants who can take advantage of the situation. Being too trusting can be dangerous. The presence of some untrustworthy participants hones the skills of those who follow reciprocity norms.

Thus, individuals vary substantially in the probability that they will use particular norms, in how structural variables affect their level of trust and willingness to reciprocate cooperation in a particular situation, and in how they develop their own reputation. Some individuals use reciprocity only in situations in which there is close monitoring and strong retribution is likely. Others will only cooperate in dilemmas when they have publicly committed themselves to an agreement and have assurances from others that their trust will be returned. Others find it easier to build an external reputation by building their own personal identity as someone who always trusts others until proven wrong. If this trust proves to be misplaced, then they stop cooperating and either exit the situation or enter a punishment phase. As Hoffman, McCabe, and Smith (1996a, 23–4) express it:

> A one-shot game in the laboratory is part of a life-long sequence, not an isolated experience that calls for behavior that deviates sharply from one's reputational norm. Thus we should expect subjects to rely upon reciprocity norms in experimental settings unless they discover in the process of participating in a particular experiment that reciprocity is punished and other behaviors are rewarded. In such cases they abandon their instincts and attempt other strategies that better serve their interests.

In any population of individuals, one is likely to find some who use one of three reciprocity norms when they confront a repeated social dilemma.[25]

1. Always cooperate first; stop cooperating if others do not reciprocate; punish noncooperators if feasible.
2. Cooperate immediately only if one judges others to be trustworthy; stop cooperating if others do not reciprocate; punish noncooperators if feasible.
3. Once cooperation is established by others, cooperate oneself; stop cooperating if others do not reciprocate; punish noncooperators if feasible.

In addition, one may find at least three other norms.

4. Never cooperate.
5. Mimic (1) or (2), but stop cooperating if one can successfully free ride on others.
6. Always cooperate (an extremely rare norm in all cultures).

The proportion of individuals who follow each type of norm will vary from one subpopulation to another and from one situation to another.[26] Whether reciprocity is advantageous to individuals depends sensitively on the proportion of other individuals who use reciprocity and on an individual's capacity to judge the likely frequency of reciprocators in any particular situation and over time. When there are many others who use a form of reciprocity that always cooperates first, then even in one-shot situations cooperation may lead to higher returns when diverse situations are evaluated together. Boundedly rational individuals would expect other boundedly rational individuals to follow a *diversity* of heuristics, norms, and strategies rather than expect to find others who adopt a single strategy—except in those repeated situations in which institutional selection processes sort out those who do not search out optimal strategies. Investment in detection of other individuals' intentions and actions improves one's own outcomes. One does not have to assume that others are "irrational" in order for it to be rational to use reciprocity (Kreps et al. 1982).

Evidence of the Use of Reciprocity in Experimental Settings

Laboratory experiments provide evidence that a substantial proportion of individuals use reciprocity norms even in the very short-term environments of an experiment (McCabe, Rassenti, and Smith 1996). Some evidence comes from experiments on ultimatum games. In such games, two players are asked to divide a fixed sum of money. The first player suggests a division to the second, who then decides to accept or reject the offer. If the offer is accepted, then the funds are divided as proposed. If it is rejected, then both players receive zero. The predicted equilibrium is that the first player will offer a minimal unit to the second player, who will then accept anything more than zero. This prediction has repeatedly been falsified, starting with the work of Güth, Schmittberger, and Schwarze (1982; see Frey and Bohnet 1996; Güth and Tietz 1990; Roth 1995; Samuelson, Gale, and Binmore 1995).[27] Subjects assigned to the first position tend to offer

[25] This is not the complete list of all types of reciprocity norms, but it captures the vast majority.

[26] The proportion of individuals who follow the sixth norm—cooperate always—will be minuscule or nonexistent. Individuals following the first norm will be those, along with those following the sixth norm, who cooperate in the first few rounds of a finitely repeated experimental social dilemma without prior communication. Individuals following the second norm will cooperate (immediately) in experiments if they have an opportunity to judge the intentions and trustworthiness of the other participants and expect most of the others to be trustworthy. Those following the third norm will cooperate (after one or a few rounds) in experiments in which others cooperate.

[27] The results obtained by Hoffman, McCabe, and Smith (1996b) related to dictator games under varying conditions of social distance are also quite consistent with the behavioral approach of this article.

substantially more than the minimum unit. They frequently offer the "fair" division of splitting the sum. Second movers tend to reject offers that are quite small. The acceptance level for offers tends to cluster around different values in diverse cultures (Roth et al. 1991). Given that the refusal to accept the funds offered contradicts a basic tenet in the complete model of rationality, these findings have represented a major challenge to the model's empirical validity in this setting.

Several hypotheses have been offered to explain these findings, including a "punishment hypothesis" and a "learning hypothesis."

> The punishment hypothesis is in essence a reciprocity argument. In contrast to adaptive learning, punishment attributes a motive to the second mover's rejection of an unequal division asserting that it is done to punish the first mover for unfair treatment. This propensity toward negative reciprocity is the linchpin of the argument. Given this propensity, first movers should tend to shy away from the perfect equilibrium offer out of fear of winding up with nothing (Abbink et al. 1996, 6).

Abbink and his colleagues designed an experiment in which the prediction of the learning and punishment hypotheses is clearly different and found strong support for the punishment hypothesis. "We found that second movers were three times more likely to reject the unequal split when doing so punished the first mover ... than when doing so rewarded the first mover" (Albrink et al. 1996, 15–6). Consequently, second movers do appear to punish first movers who propose unfair divisions.

Two additional findings from one-shot social dilemmas provide further evidence of the behavioral propensities of subjects. First, those who intend to cooperate in a particular one-shot social dilemma also expect cooperation to be returned by others at a much higher rate than those who intend to defect (Dawes, McTavish, and Shaklee 1977; Dawes, Orbell, and van de Kragt 1986). As Orbell and Dawes (1991, 519) summarize their own work: "One of our most consistent findings throughout these studies—a finding replicated by others' work—is that cooperators expect significantly more cooperation than do defectors." Second, when there is a choice whether to participate in a social dilemma, those who intend to cooperate exhibit a greater willingness to enter such transactions (Orbell and Dawes 1993). Given these two tendencies, reciprocators are likely to be more optimistic about finding others following the same norm and disproportionately enter more voluntary social dilemmas than nonreciprocators. Given both propensities, the feedback from such voluntary activities will generate confirmatory evidence that they have adopted a norm which serves them well over the long run.

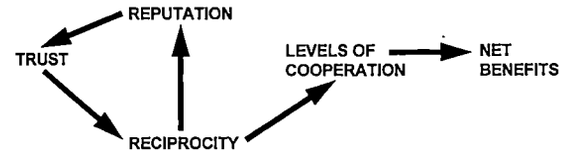
Thus, while individuals vary in their propensity to use reciprocity, the evidence from experiments shows that a substantial proportion of the population drawn on by social science experiments has sufficient trust that others are reciprocators to cooperate with them even in one-shot, no-communication experiments. Furthermore, a substantial proportion of the population is also willing to punish noncooperators (or individuals who do not make fair offers) at a cost to themselves. Norms are learned from prior experience (socialization) and are affected by situational variables yielding systematic differences among experimental designs. The level of trust and resulting levels of cooperation can be increased by (1) providing subjects with an opportunity to see one another (Frey and Bohnet 1996, Orbell and Dawes 1991), (2) allowing subjects to choose whether to enter or exit a social-dilemma game (Orbell and Dawes 1991, 1993; Orbell, Schwartz-Shea, and Simmons 1984; Schuessler 1989; Yamagishi 1988c; Yamagishi and Hayashi 1996), (3) sharing the costs equally if a minimal set voluntarily contributes to a public good (Dawes, Orbell, and van de Kragt 1986), (4) providing opportunities for distinct punishments of those who are not reciprocators (Abbink et al. 1996; McCabe, Rassenti, and Smith 1996), and, as discussed above, (5) providing opportunities for face-to-face communication.

The Core Relationships: Reciprocity, Reputation, and Trust

When many individuals use reciprocity, there is an incentive to acquire a *reputation* for keeping promises and performing actions with short-term costs but long-term net benefits (Keohane 1984; Kreps 1990; Milgrom, North, and Weingast 1990; Miller 1992). Thus, trustworthy individuals who trust others with a reputation for being trustworthy (and try to avoid those who have a reputation for being untrustworthy) can engage in mutually productive social exchanges, even though they are dilemmas, so long as they can limit their interactions primarily to those with a reputation for keeping promises. A reputation for being trustworthy, or for using retribution against those who do not keep their agreements or keep up their fair share, becomes a valuable asset. In an evolutionary context, it increases fitness in an environment in which others use reciprocity norms. Similarly, developing *trust* in an environment in which others are trustworthy is also an asset (Braithwaite and Levi n.d., Fukuyama 1995, Gambetta 1988, Putnam 1993). Trust is the expectation of one person about the actions of others that affects the first person's choice, when an action must be taken before the actions of others are known (Dasgupta 1997, 5). In the context of a social dilemma, trust affects whether an individual is willing to initiate cooperation in the expectation that it will be reciprocated. Boundedly rational individuals enter situations with an initial probability of using reciprocity based on their own prior training and experience.

Thus, at the core of a behavioral explanation are the links between the trust that individuals have in others, the investment others make in trustworthy reputations, and the probability that participants will use reciprocity norms (see Figure 2). This mutually reinforcing core is affected by structural variables as well as the past experiences of participants. In the initial round of a repeated dilemma, individuals do or do not initiate cooperative behavior based on their own norms, how

FIGURE 2. The Core Relationships



much trust they have that others are reciprocators (based on any information they glean about one another), and how structural variables affect their own and their expectation of others' behavior.

If initial levels of cooperation are moderately high, then individuals may learn to trust one another, and more may adopt reciprocity norms. When more individuals use reciprocity norms, gaining a reputation for being trustworthy is a better investment. Thus, levels of trust, reciprocity, and reputations for being trustworthy are positively reinforcing. This also means that a decrease in any one of these can lead to a downward spiral. Instead of explaining levels of cooperation directly, this approach leads one to link structural variables to an inner triangle of trust, reciprocity, and reputation as these, in turn, affect levels of cooperation and net benefits.

Communication and the Core Relationships

With these core relationships, one can begin to explain why repeated face-to-face communication substantially changes the structure of a situation (see discussion in E. Ostrom, Gardner, and Walker 1994, 199). With a repeated chance to see and talk with others, a participant can assess whether s/he trusts others sufficiently to try to reach a simple contingent agreement regarding the level of joint effort and its allocation. In a contingent agreement, individuals agree to contribute X resources to a common effort so long as at least Y others also contribute. Contingent agreements do not need to include all those who benefit. The benefit to be obtained from the contribution of Y proportion of those affected may be so substantial that some individuals are willing to contribute so long as Y proportion of others also agree and perform.

Communication allows individuals to increase (or decrease) their trust in the reliability of others.[28] When successful, individuals change their expectations from the initial probability that others use reciprocity norms to a higher probability that others will reciprocate trust and cooperation. When individuals are symmetric in assets and payoffs, the simplest agreement is to share a contribution level equally that closely approximates the optimum joint outcome. When individuals are not symmetric, finding an agreement is more difficult, but various fairness norms can be used to reduce the time and effort needed to achieve an agreement (see Hackett, Dudley, and Walker 1995; Hackett, Schlager, and Walker 1994).

Contingent agreements may deal with punishment of those who do not cooperate (Levi 1988). How to punish noncooperative players, keep one's own reputation, and sustain any initial cooperation that has occurred in N-person settings is more difficult than in two-person settings.[29] In an N-person, uncertain situation, it is difficult to interpret from results that are less than expected whether one person cheated a lot, several people cheated a little, someone made a mistake, or everyone cooperated and an exogenous random variable reduced the expected outcome. If there is no communication, then the problem is even worse. Without communication and an agreement on a sharing formula, individuals can try to signal a willingness to cooperate through their actions, but no one has agreed to any particular contribution. Thus, no one's reputation (external or internal) is at stake.

Once a verbal agreement in an N-person setting is reached, that becomes the focal point for further action within the context of a particular ongoing group. If everyone keeps to the agreement, then no further reaction is needed by someone who is a reciprocator. If the agreement is not kept, however, then an individual following a reciprocity norm—without any prior agreement regarding selective sanctions for nonconformance—needs to punish those who did not keep their commitment. A frequently posited punishment is the grim trigger, whereby a participant plays the Nash equilibrium strategy forever upon detecting any cheating. Subjects in repeated experiments frequently discuss the use of a grim trigger to punish mild defections but reject the idea because it would punish everyone—not just the cheater(s) (E. Ostrom, Gardner, and Walker 1994). A much less drastic punishment strategy is the measured reaction. "In a measured reaction, a player reacts mildly (if at all) to a small deviation from an agreement. Defections trigger mild reactions instead of harsh punishments. If defections continue over time, the measured response slowly moves from the point of agreement toward the Nash equilibrium" (pp. 199–200).

For several reasons, this makes sense as the initial "punishment" phase in an N-person setting with a minimal institutional structure and no feedback concerning individual contributions. If only a small deviation occurs, then the cooperation of most participants is already generating positive returns. By keeping one's own reaction close to the agreement, one keeps up

[28] Frank, Gilovich, and Regan (1993) found, for example, that the capacity of subjects to predict whether others would play cooperatively was significantly better than chance after a face-to-face group discussion. Kikuchi, Watanabe, and Yamagishi (1996) found that high trusters predicted other players' trustworthiness significantly better than did low trusters.

[29] In a two-person situation of complete certainty, individuals can easily follow the famous tit-for-tat (or tit-for-tat or exit) strategy even without communication. When a substantial proportion of individuals in a population follows this norm, and they can identify with whom they have interacted in the past (to either refuse future interactions or to punish prior uncooperative actions), and when discount rates are sufficiently low, tit-for-tat has been shown to be a highly successful strategy, yielding higher payoffs than are available to those using other strategies (Axelrod 1984, 1986). With communication, it is even easier.

one's own reputation for cooperation, keeps cooperation levels higher, and makes it easier to restore full conformance. Using something like a grim trigger immediately leads to the unraveling of the agreement and the loss of substantial benefits over time. To supplement the measured reaction, effort is expended on determining who is breaking the agreement, on using verbal rebukes to try to get that individual back in line, and on avoiding future interactions with that individual.[30]

Thus, understanding how trust, reciprocity, and reputation feed one another (or their lack, which generates a cascade of negative effects) helps to explain why repeated, face-to-face communication has such a major effect. Coming to an initial agreement and making personal promises to one another places at risk an individual's own identity as one who keeps one's word, increases trust, and makes reciprocity an even more beneficial strategy. Tongue-lashing can be partially substituted in a small group for monetary losses and, when backed by measured responses, can keep many groups at high levels of cooperation. Meeting only once can greatly increase trust, but if some individuals do not cooperate immediately, the group never has a further opportunity to hash out these problems. Any evidence of lower levels of cooperation undermines the trust established in the first meeting, and there is no further opportunity to build trust or use verbal sanctioning. It is also clearer now why sending anonymous, computerized messages is not as effective as face-to-face communication. Individuals judge one another's trustworthiness by watching facial expressions and hearing the way something is said. It is hard to establish trust in a group of strangers who will make decisions independently and privately without seeing and talking with one another.

ILLUSTRATIVE THEORETICAL SCENARIOS

I have tried to show the need for the development of second-generation models of rationality in order to begin a coherent synthesis of what we know from empirical research on social dilemmas. Rather than try to develop a new formal model, I have stayed at the theoretical level to identify the attributes of human behavior that should be included in future formal models. The individual attributes that are particularly important in explaining behavior in social dilemmas include the expectations individuals have about others' behavior (trust), the norms individuals learn from socialization and life's experiences (reciprocity), and the identities individuals create that project their intentions and norms (reputation). Trust, reciprocity, and reputation can be included in formal models of individual behavior (see the works cited by Boyd and Richerson 1988, Güth and Yaari 1992, Nowak and Sigmund 1993).

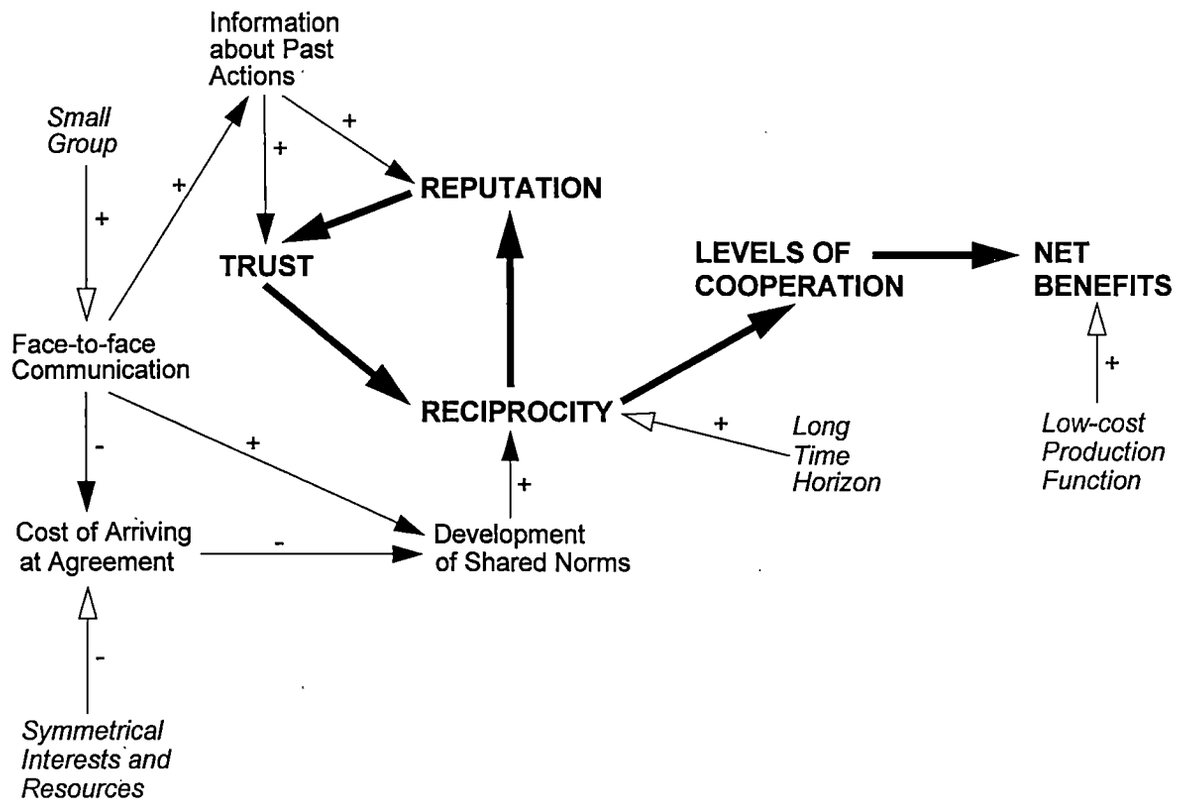
In this section, I construct theoretical scenarios of how exogenous variables combine to affect endogenous structural variables that link to the core set of relationships shown in Figure 2. It is not possible to relate all structural variables in one large causal model, given the number of important variables and the fact that many depend for their effect on the values of other variables. It is possible, however, to produce coherent, cumulative, theoretical scenarios that start with relatively simple baseline models. One can then begin the systematic exploration of what happens as one variable is changed. Let me illustrate what I mean by theoretical scenarios.

Let us start with a scenario that should be conducive to cooperation—a small group of ten farmers who own farms of approximately the same size. These farmers share the use of a creek for irrigation that runs by their relatively flat properties. They face the problem each year of organizing one collective workday to clear out the fallen trees and brush from the prior winter. All ten expect to continue farming into the indefinite future. Let us assume that the creek delivers a better water supply directly in response to how many days of work are completed. All farmers have productive opportunities for their labor that return more at the margin than the return they would receive from their own input into this effort. Thus, free riding and hoping that the others contribute labor is objectively attractive. The value to each farmer, however, of participation in a successful collective effort to clear the creek is greater than the costs of participating.

Now let us examine how some structural variables affect the likelihood of collective action (see Figure 3). As a small group, it would be easy for them to engage in face-to-face communication. Since their interests and resources are relatively symmetric, arriving at a fair, contingent agreement regarding how to share the work should not be too difficult. One simple agreement that is easy to monitor is that they all work on the same day, but each is responsible for clearing the part of the creek going through his or her property. Conformance to such an agreement would be easy to verify. While engaged in discussions, they can reinforce the importance of everyone participating in the workday. In face-to-face meetings, they can also gossip about anyone who failed to participate in the past, urge them to change their ways, and threaten to stop all labor contributions if they do not "shape up." Given the small size of the group, its symmetry, and the relatively low cost of providing the public good, combined with the relatively long time horizon, we can predict with some confidence that a large proportion of individuals facing such a situation will find a way to cooperate and overcome the dilemma. Not only does the evidence from experimental research support that prediction, but also substantial evidence from the field is consistent with this explanation (see E. Ostrom n.d.).

[30] In a series of 18 common-pool resource experiments, each involving eight subjects in finitely repeated communication experiments, E. Ostrom, Gardner, and Walker (1994, 215) found that subjects kept to their agreements or used measured responses in two-thirds of the experiments. In these experiments, joint yields averaged 89% of optimum. In the six experiments in which some players deviated substantially from agreements and measured responses did not bring them back to the agreement, cooperation levels were substantially less, and yields averaged 43% of optimum (which is still far above zero levels of cooperation).

FIGURE 3. A Simple Scenario



This is a rough but coherent causal theory that uses structural variables (small size, symmetry of assets and resources, long time horizon, and a low-cost production function) to predict with high probability that participants can themselves solve this social dilemma. Changes in any of the structural variables of this relatively easy scenario affect that prediction. Even a small change may suffice to reverse the predicted outcome. For example, assume that another local farmer buys five parcels of land with the plan to farm them for a long time. Now there are only six farmers, but one of them holds half the relevant assets. If that farmer shares the norm that it is fair to share work allocated to a collective benefit in the same ratio as the benefits are allocated, then the increased heterogeneity will not be a difficult problem to overcome. They would agree—as farmers around the world have frequently agreed (see Lam n.d., Tang 1992)—to share the work in proportion to the amount of land they own. If the new farmer uses a different concept of fairness, then the smaller group may face a more challenging problem than the larger group due to its increased heterogeneity.

Now, assume that the five parcels of land are bought by a local developer to hold for future use as a suburban housing development. The time horizon of one of the six actors—the developer—is extremely short with regard to investments in irrigation. From the developer's perspective, he is not a "free rider," as he sees no benefit to clearing out the creek. Thus, such a change actually produces several: A decrease in the N of the group, an introduction of an asymmetry of interests and resources, and the presence of one participant with half the resources but a short time horizon and no interest in the joint benefit. This illustrates how changes in one structural variable can lead to a cascade of changes in the others, and thus how difficult it is to make simple bivariate hypotheses about the effect of one variable on the level of cooperation. In particular, this smaller group is much less likely to cooperate than the larger group of ten symmetric farmers, exactly the reverse of the standard view of the effect of group size.

IMPLICATIONS

The implications of developing second-generation models of empirically grounded, boundedly rational, and moral decision making are substantial. Puzzling research questions can now be addressed more systematically. New research questions will open up. We need to expand the type of research methods regularly used in political science. We need to increase the level of understanding among those engaged in formal theory, experimental research, and field research across the social and biological sciences. The foundations of policy analysis need rethinking. And civic education can be based on empirically validated theories of

collective action empowering citizens to use the "science and art of association" (Tocqueville [1835 and 1840] 1945) to help sustain democratic polities in the twenty-first century.

Implications for Research

What the research on social dilemmas demonstrates is a world of *possibility* rather than of *necessity*. We are neither trapped in inexorable tragedies nor free of moral responsibility for creating and sustaining incentives that facilitate our own achievement of mutually productive outcomes. We cannot adopt the smug presumption of those earlier group theorists who thought groups would always form whenever a joint benefit would be obtained. We can expect many groups to fail to achieve mutually productive benefits due to their lack of trust in one another or to the lack of arenas for low-cost communication, institutional innovation, and the creation of monitoring and sanctioning rules (V. Ostrom 1997). Nor can we simply rest assured that only one type of institution exists for all social dilemmas, such as a competitive market, in which individuals pursuing their own preferences are led to produce mutually productive outcomes. While new institutions often facilitate collective action, the key problems are to design new rules, motivate participants to conform to rules once they are devised, and find and appropriately punish those who cheat. Without individuals viewing rules as appropriate mechanisms to enhance reciprocal relationships, no police force and court system on earth can monitor and enforce all the needed rules on its own. Nor would most of us want to live in a society in which police were really the thin blue line enforcing all rules.

While I am proposing a further development of second-generation theories of rational choice, theories based on complete but thin rationality will continue to play an important role in our understanding of human behavior. The clear and unambiguous predictions stemming from complete rational choice theories will continue to serve as a critical benchmark in conducting empirical studies and for measuring the success or failure of any other explanation offered for observed behavior. A key research question will continue to be: What is the difference between the predicted equilibrium of a complete rationality theory and observed behavior? Furthermore, game theorists are already exploring ways of including reputation, reciprocity, and various norms of behavior in game-theoretic models (see Abbink et al. 1996; Güth 1995; Kreps 1990; Palfrey and Rosenthal 1988; Rabin 1994; Selten 1990, 1991). Thus, bounded and complete rationality models may become more complementary in the next decade than appears to be the case today.

For political scientists interested in diverse institutional arrangements, complete rational choice theories provide well-developed methods for analyzing the vulnerability of institutions to the strategies devised by talented, analytically sophisticated, short-term hedonists (Brennan and Buchanan 1985). Any serious institutional analysis should include an effort to understand how institutions—including ways of organizing legislative procedures, formulas used to calculate electoral weights and minimal winning coalitions, and international agreements on global environmental problems—are vulnerable to manipulation by calculating, amoral participants.[31] In addition to the individuals who have learned norms of reciprocity in any population, others exist who may try to subvert the process so as to obtain very substantial returns for themselves while ignoring the interests of others. One should always know the consequences of letting such individuals operate in any particular institutional setting.

The most immediate research questions that need to be addressed using second-generation models of human behavior relate to the effects of structural variables on the likelihood of organizing for successful modes of collective action. It will not be possible to relate all structural variables in one large causal theory, given that they are so numerous and that many depend for their effect on the values of other variables. What is possible, however, is the development of coherent, cumulative, theoretical scenarios that start with relatively simple baseline models and then proceed to change one variable at a time, as briefly illustrated above. From such scenarios, one can proceed to formal models and empirical testing in field and laboratory settings. The kind of *theory* that emerges from such an enterprise does not lead to the global bivariate (or even multivariate) predictions that have been the ideal to which many scholars have aspired. Marwell and Oliver (1993) have constructed such a series of theoretical scenarios for social dilemmas involving large numbers of heterogeneous participants in collective action. They have come to a similar conclusion about the nature of the theoretical and empirical enterprise: "This is not to say that general theoretical predictions are impossible using our perspective, only that they cannot be simple and global. Instead, the predictions that we can validly generate must be complex, interactive, and conditional" (p. 25).

As political scientists, we need to recognize that political systems are complexly organized and that we will rarely be able to state that one variable is always positively or negatively related to a dependent variable. One can do comparative statics, but one must know the value of the other variables and not simply assume that they vary around the average.

The effort to develop second-generation models of boundedly rational and moral behavior will open up a variety of *new* questions to be pursued that are of major importance to all social scientists and many biologists interested in human behavior. Among these questions are: How do individuals gain trust in other individuals? How is trust affected by diverse institutional arrangements? What verbal and visual clues are used in evaluating others' behavior? How do individuals gain

[31] Consequently, research on the effect of institutional arrangements on strategies and outcomes continues to be crucial to future developments. See Agrawal n.d.; Alt and Shepsle 1990; Bates 1989; Dasgupta 1993; Eggertsson 1990; Gibson n.d.; Levi 1997; V. Ostrom 1997; V. Ostrom, Feeny, and Picht 1993; Scharpf 1997.

common understanding so as to craft and follow self-organized arrangements (V. Ostrom 1990)? John Orbell (personal communication) posits a series of intriguing questions: "Why do people join together in these games in the first place? How do we select partners in these games? How do our strategies for selecting individual partners differ from our strategies for adding or removing individuals from groups?"

An important set of questions is related to how institutions enhance or restrict the building of mutual trust, reciprocity, and reputations. A recent set of studies on tax compliance raises important questions about the trust heuristics used by citizens and their reactions to governmental efforts to monitor compliance (see Scholz n.d.). Too much monitoring may have the counterintuitive result that individuals feel they are *not* trusted and thus become less trustworthy (Frey 1993). Bruno Frey (1997) questions whether some formal institutional arrangements, such as social insurance and paying people to contribute effort, reduce the likelihood that individuals continue to place a positive intrinsic value on actions taken mainly because of internal norms. Rather, they may assume that formal organizations are charged with the responsibility of taking care of joint needs and that reciprocity is no longer needed (see also Taylor 1987).

Since all rules legitimate the use of sanctions against those who do not comply, rules can be used to assign benefits primarily to a dominant coalition. Those who are, thus, excluded have no motivation to cooperate except in order to avoid sanctions. Using first-generation models, that is what one expects in any case. Using second-generation models, one is concerned with how constitutional and collective-choice rules affect the distribution of benefits and the likelihood of reciprocal cooperation. While much research has been conducted on long-term successful self-organized institutions, less has been documented about institutions that never quite got going or failed after years of success. More effort needs to be made to find reliable archival information concerning these failed attempts and why they failed.

It may be surprising that I have relied so extensively on experimental research. I do so for several reasons. As theory becomes an ever more important core of our discipline, experimental studies will join the ranks of basic empirical research methods for political scientists. As an avid field researcher for the past 35 years, I know the importance and difficulty of testing theory in field settings—particularly when variables function interactively. Large-scale field studies will continue to be an important source of empirical data, but frequently they are a very expensive and inefficient method for addressing how institutional incentives combine to affect individual behavior and outcomes. We can advance much faster and more coherently when we examine hypotheses about contested elements among diverse models or theories of a coherent framework. Careful experimental research designs frequently help sort out competing hypotheses more effectively than does trying to find the precise combination of variables in the field. By adding experimental methods to the battery of field methods already used extensively, the political science of the twenty-first century will advance more rapidly in acquiring well-grounded theories of human behavior and of the effect of diverse institutional arrangements on behavior. Laboratory research will still need to be complemented by sound field studies to meet the criteria of external validity.

Implications for Policy

Using a broader theory of rationality leads to potentially different views of the state. If one sees individuals as helpless, then the state is the essential external authority that must solve social dilemmas for everyone. If, however, one assumes individuals can draw on heuristics and norms to solve some problems and create new structural arrangements to solve others, then the image of what a national government might do is somewhat different. There is a very considerable role for large-scale governments, including national defense, monetary policy, foreign policy, global trade policy, moderate redistribution, keeping internal peace when some groups organize to prey on others, provision of accurate information and of arenas for resolving conflicts with national implications, and other large-scale activities. But national governments are too small to govern the global commons and too big to handle smaller scale problems.

To achieve a complex, multitiered governance system is quite difficult. Many types of questions are raised. How do different kinds of institutions support or undermine norms of reciprocity both within hierarchies (Miller 1992) and among members of groups facing collective action problems (Frohlich and Oppenheimer 1970, Galjart 1992)? Field studies find that monitoring and graduated sanctions are close to universal in all robust common-pool resource institutions (E. Ostrom 1990). This tells us that without some external support of such institutions, it is unlikely that reciprocity *alone* completely solves the more challenging common-pool resource problems. Note that sanctions are graduated rather than initially severe. Our current theory of crime—based on a strict expected value theory—does not explain this. If people can learn reciprocity as the fundamental norm for organizing their lives, and if they agree to a set of rules contingent upon others following these rules, then graduated sanctions do something more than deter rule infractions.

Reciprocity norms can have a dark side. If punishment consists of escalating retribution, then groups who overcome social dilemmas may be limited to very tight circles of kin and friends, who cooperate only with one another, embedded in a matrix of hostile relationships with outsiders (R. Hardin 1995). This pattern can escalate into feuds, raids, and overt warfare (Boyd and Richerson 1992, Chagnon 1988, Elster 1985, Kollock 1993). Or tight circles of individuals who trust one another may discriminate against anyone of a different color, religion, or ethnicity. A focus on the return of favors for favors can also be the foundation for corruption. It is in everyone else's interest that some social

dilemmas are *not* resolved, such as those involved in monopolies and cartel formation, those that countervene basic moral standards and legal relationships, and those that restrict the opportunities of an open society and an expanding economy. Policies that provide alternative opportunities for those caught in dysfunctional networks are as important as those that stimulate and encourage positive networks (Dasgupta 1997).

Implications for Civic Education

Human history teaches us that autocratic governments often wage war on their own citizens as well as on those of other jurisdictions. Democracies are characterized by the processing of conflict among individuals and groups without resort to massive killings. Democracies are, however, themselves fragile institutions that are vulnerable to manipulation if citizens and officials are not vigilant (V. Ostrom 1997). For those who wish the twenty-first century to be one of peace, we need to translate our research findings on collective action into materials written for high school and undergraduate students. All too many of our textbooks focus exclusively on leaders and, worse, only national-level leaders. Students completing an introductory course on American government, or political science more generally, will not learn that they play an essential role in sustaining democracy. Citizen participation is presented as contacting leaders, organizing interest groups and parties, and voting. That citizens need additional skills and knowledge to resolve the social dilemmas they face is left unaddressed. Their moral decisions are not discussed. We are producing generations of cynical citizens with little trust in one another, much less in their governments. Given the central role of trust in solving social dilemmas, we may be creating the very conditions that undermine our own democratic ways of life. It is ordinary persons and citizens who craft and sustain the workability of the institutions of everyday life. We owe an obligation to the next generation to carry forward the best of our knowledge about how individuals solve the multiplicity of social dilemmas—large and small—that they face.

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Convicting the Innocent: The Inferiority of Unanimous Jury Verdicts under Strategic Voting

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It is often suggested that requiring juries to reach a unanimous verdict reduces the probability of convicting an innocent defendant while increasing the probability of acquitting a guilty defendant. We construct a model that demonstrates how strategic voting by jurors undermines this basic intuition. We show that the unanimity rule may lead to a high probability of both kinds of error and that the probability of convicting an innocent defendant may actually increase *with the size of the jury. Finally, we demonstrate that a wide variety of voting rules, including simple majority rule, lead to much lower probabilities of both kinds of error.*

According to Lord Devlin, "Trial by jury is not an instrument of getting at the truth; it is a process designed to make it as sure as possible that no innocent man is convicted" (Klaven and Zeisel 1966, 190). It is commonly thought that requiring juries to reach a unanimous verdict is exactly the mechanism that protects innocent defendants and that this protection comes at the cost of an increased probability of acquitting a guilty defendant. We construct a model that demonstrates how strategic voting by jurors undermines this basic intuition. The unanimity rule may lead to a high probability of both errors, and the probability of convicting an innocent defendant may actually increase with the size of the jury. We also demonstrate that the unanimity rule is an exceptionally bad rule. A wide variety of voting rules, including simple majority, lead to much lower probabilities of both errors.

There is a large literature on juries and jury decision making (e.g., Adler 1994, Klaven and Zeisel 1966, Levine 1992, and McCart 1964). A central argument for juries, formalized in the literature on Condorcet's jury theorem, is that a group will make a better decision than an individual (Klevorick et al. 1984, Ladha 1992, Miller 1986, and Young 1988). In Condorcet's jury theorem it is assumed that jurors have private information about the guilt or innocence of the defendant. This captures the idea that jurors interpret evidence differently because of their different life experiences and competencies. Since a jury vote aggregates their private information, juries make fewer errors than any individual.

Until recently, the literature has assumed that each juror will behave as if her vote alone determines the outcome. Several recent articles have demonstrated that such behavior by jurors is frequently irrational and that the combination of private information and common interests creates an incentive for jurors to vote strategically (Austen-Smith and Banks 1996; Feddersen and Pesendorfer 1996, 1997a, 1997b; McLennan 1996; Myerson 1997; Wit 1996).[1] There is even experimental research that finds empirical support for the strategic voting hypothesis (Ladha, Miller and Oppenheimer 1996). The incentive to vote strategically arises because a juror's vote only matters when a vote is pivotal and because the information possessed by other jurors is relevant for a juror's decision. For example, under the unanimity rule, a vote is pivotal only if all the other jurors have voted to convict. The fact that all other jurors have voted to convict reveals additional information about the guilt of the defendant. Such information may overwhelm the juror's private assessment of the case and cause a juror otherwise inclined to vote for acquittal to vote for conviction instead.

In this article we examine the implications of strategic voting by jurors and demonstrate that basic intuitions about the consequences of requiring a unanimous vote may be dramatically wrong. We construct a model of jury decision making that incorporates private information and strategic voting. We show that the requirement of a unanimous verdict may actually result in a significantly higher probability of convicting an innocent defendant than would, for example, simple majority rule. We conclude with a few brief remarks on the implications of our results for jury reform.

THE MODEL

There are n jurors, $j = 1, \ldots, n$, who must decide the fate of a defendant. The defendant is either guilty (G) or innocent (I).[2] We assume that G and I occur with equal probability.[3]

Jurors are uncertain whether the defendant is guilty or innocent. We assume that each juror gets a signal g or i that is correlated with the true state. Specifically, we assume that

$$\Pr(g|G) = \Pr(i|I) = p.$$

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[1] Jurors have common interests to the extent that all prefer to convict a guilty defendant and acquit an innocent defendant.

[2] Feddersen and Pesendorfer (1997a, 1997b) generalize the simple two-state model to multiple states. Adding additional states will not fundamentally alter our results.

[3] This assumption can be easily relaxed without significantly changing our results.

Hence, the parameter $p \in (.5, 1)$ is the probability that a juror receives the "correct" signal (g if the defendant is guilty, i if the defendant is innocent), and $1 - p$ is the probability that the juror receives the "incorrect" signal (i if the defendant is guilty, g if the defendant is innocent).[4]

We assume the signal is private information. Since jurors observe the same facts at the trial and engage in deliberations prior to taking the final vote, the assumption may seem inappropriate. Yet, there are several reasons the complete disclosure of private information through the deliberation process may not occur. For example, some jurors may have technical knowledge that is relevant for the decision but that cannot be fully communicated in the limited amount of time available. Furthermore, while all jurors agree that they prefer convicting the guilty and acquitting the innocent, each may have a different threshold of reasonable doubt.[5] Even such minimal preference diversity may create incentives for jurors not to reveal their private information in deliberations. For example, a juror predisposed to convict may be reluctant to reveal her innocent signal lest another juror with a higher threshold who received a guilty signal vote to acquit. Since we do not model the effect of jury deliberations, determining that effect from a theoretical standpoint is beyond the scope of this article.

The jury reaches a decision by taking a simultaneous vote. Each juror must vote either to convict or to acquit. If the number of votes to convict is larger than or equal to $\hat{k}$, then the defendant is convicted; otherwise, the defendant is acquitted. The number $\hat{k}$ defines the voting rule used by the jury. Thus, if $\hat{k} = n$, then a unanimous verdict is required to convict; if $\hat{k} = (n + 1)/2$, then a conviction is obtained by a simple majority vote. There are two possible outcomes of the jury's vote: either the defendant is convicted (C) or acquitted (A).

Given the voting rule $\hat{k}$, juror j's behavior can be described by the strategy σ_j: $\{g, i\} \rightarrow [0, 1]$, which maps the set of signals into a probability of voting to convict.

We assume all jurors have preferences, given by $u(A, I) = u(C, G) = 0$, $u(C, I) = -q$, and $u(A, G) = -(1 - q)$, where $q \in (0, 1)$. Thus, if a guilty defendant is convicted or an innocent defendant is acquitted, then each juror's payoff is zero. If an innocent defendant is convicted, then the juror's payoff is $-q$; if a guilty defendant is acquitted, then the juror's payoff is $-(1 - q)$. The parameter q exactly characterizes a juror's threshold of reasonable doubt. A juror who believes the defendant is guilty with probability higher than q will prefer the defendant to be convicted. The larger the value of q, the less concern jurors have for acquitting a guilty defendant relative to convicting an innocent defendant. We assume that jurors employ the same standard of reasonable doubt, that is, q is identical for all jurors. This assumption is made for technical convenience. Below, we indicate how our results generalize to the case in which jurors' preferences are represented by different values of q. (See Feddersen and Pesendorfer 1996, 1997a, 1997b and Coughlan 1997 for examples of strategic voting under preference diversity.)

Let $\beta(k, n)$ denote the posterior probability that the defendant is guilty, conditional on observing n signals, k of which are guilty:

$$\beta(k, n) = \frac{p^k(1-p)^{n-k}}{p^k(1-p)^{n-k} + (1-p)^k p^{n-k}}.$$

If $\beta(k, n) > q$, then, given all the information available to the jury, the defendant is guilty beyond a reasonable doubt. Therefore, the optimal outcome from the jurors' point of view is to convict. Similarly, if $\beta(k, n) < q$, then the optimal outcome for the jurors is to acquit. We assume that there is a k^* with $n \geq k^* \geq 1$, such that

$$\beta(k^* - 1, n) < q < \beta(k^*, n).$$

This assumption implies, on the one hand, that if the jurors know they all have received the guilty signal, then they always want to convict the defendant. On the other hand, if the jurors believe they all have received an innocent signal, then they always want to acquit.

As a baseline for comparison we first consider nonstrategic voting. We say that a juror votes informatively if she votes guilty when she receives a guilty signal and innocent when she receives an innocent signal. If jurors vote informatively, then the unanimity rule ($\hat{k} = n$) leads to a lower probability of convicting an innocent defendant than would any other rule ($\hat{k} < n$). The probability an innocent defendant is convicted under the unanimity rule given informative voting is $(1 - p)^n$. It is easy to see that this probability is smaller than the probability of convicting an innocent defendant under any other rule.[6] Under the unanimity rule, the probability a convicted defendant is guilty is given by

$$\frac{p^n}{p^n + (1-p)^n}.$$

This probability converges to one as the size of the jury grows, that is, $n \rightarrow \infty$. Conversely, the probability of acquitting a guilty defendant is strictly higher under the unanimity rule than under any other rule.[7]

As has been shown in the literature cited above, informative voting does not typically constitute equi-

[4] The assumption that the probability of receiving signal i in state I is identical to the probability of receiving signal g in state G can be relaxed without significantly changing our results.

[5] Support for the hypothesis of differing thresholds of reasonable doubt can be found in Klaven and Ziesel (1966, chap. 14). They show that when the verdict the judge would have chosen differs from the verdict chosen by the jury, it is more frequently the case that the jury acquits when the judge would have convicted than the other way around.

[6] If the rule requires $\hat{k}$ of n votes to be guilty, then under informative voting the probability of convicting an innocent defendant is $\Sigma_{j=\hat{k}}^{n} \binom{n}{j}(1-p)^j p^{n-j}$.

[7] This probability is $1 - p^n$ under the unanimity rule, which is strictly larger than $\Sigma_{j=0}^{\hat{k}} \binom{n}{j}(1-p)^{n-j} p^j$, which is the corresponding probability for the rule that requires $\hat{k} < n$ guilty votes.

librium behavior. In the following we analyze Nash equilibria of the described voting game. This allows us to compare the performance of the various voting rules.

THE UNANIMITY RULE

In this section we examine Nash equilibria under the unanimity rule, that is, $\hat{k} = n$.

Consider the case in which $k^* = n$, that is, $\beta(n-1, n) \leq q < \beta(n, n)$. In this case informative voting is an equilibrium under the unanimity rule. To see this, suppose that all jurors vote to convict if and only if they receive the signal g. If a juror is pivotal and receives the signal i, then he knows that $n-1$ of n jurors received the signal g. Therefore, he believes that the defendant is guilty with probability $\beta(n-1, n) \leq q$, that is, not guilty beyond a reasonable doubt, and hence he (weakly) prefers that the defendant be acquitted. Thus, a vote to acquit is optimal. Conversely, if the juror receives the signal g, then he believes that the defendant is guilty with probability $\beta(n, n) > q$, and hence he prefers that the defendant be convicted.

Now consider the case in which

$$\beta(n-1, n) > q. \tag{1}$$

This condition says that if a juror could observe all the signals, and if $n-1$ of the n signals are g, then the juror prefers to convict the defendant. Note that, for any fixed q, there is an $\bar{n}$ such that condition 1 is true for any $n > \bar{n}$.[8]

Suppose that condition 1 is satisfied and a juror believes that the other jurors are voting informatively. A rational juror will condition her vote not only on her private information but also on what she believes others must know in the event her vote is pivotal. Under the unanimity rule a vote is pivotal only when all the other jurors have voted to convict. A juror who receives an innocent signal and believes that the other jurors are voting informatively *must* believe that the probability the defendant is guilty is exactly $\beta(n-1, n)$. But since $\beta(n-1, n) > q$, the juror will ignore her private signal and vote to convict. Therefore, if (1) holds, then informative voting cannot be a Nash equilibrium.

THE PROBABILITY A CONVICTED DEFENDANT IS INNOCENT

In contrast to the results under informative voting, strategic voting under the unanimity rule imposes a *lower* bound on the probability that a convicted defendant is innocent. Proposition 1 provides this bound for *any* Nash equilibrium and shows that the bound is independent of the size of the jury.

PROPOSITION 1. *Consider any Nash equilibrium in which the defendant is convicted with strictly positive probability. Then the probability that a convicted defendant is innocent is bounded below by*

$$\min\left\{1/2, \frac{1-q}{1+q\dfrac{2p-1}{(1-p)^2}}\right\}$$

Proof. See Appendix A. *Q.E.D.*

It is straightforward to extend proposition 1 to the case in which jurors have different thresholds of reasonable doubt. If we let q denote the maximal threshold of reasonable doubt among the jurors, then the bound given in proposition 1 continues to hold in the more general case in which each juror has a different threshold, q_j, $j = 1, \ldots, n$.

To understand the intuition behind proposition 1, it is useful to distinguish two cases. First consider the (trivial) case in which the defendant is always convicted. Then the probability that a convicted defendant is innocent is simply 1/2, that is, the prior probability that the defendant is innocent.

Second, consider the case in which the defendant sometimes is acquitted. In this case at least one juror votes to acquit with strictly positive probability when he receives the signal i. A juror will only vote to acquit if the fact that a vote is pivotal is not overwhelming evidence of guilt. To put this formally, let piv_j denote the event that juror j's vote is pivotal. Upon observing signal i, j can only vote to acquit in any Nash equilibrium if

$$\Pr(G|piv_j, i) \leq q.$$

In other words, the probability the defendant is guilty conditional on j's vote being pivotal and on her private signal i must be less than or equal to her threshold of reasonable doubt q.

Now observe that the only difference between the event a vote is pivotal and the event the defendant is convicted is one vote. The most information that one additional vote can reveal occurs when a pivotal voter votes informatively. It follows that conditional on a conviction the defendant must be innocent with probability bounded strictly away from zero and that this lower bound is independent of the size of the jury. This outcome represents a stark contrast with the results under the assumption of informative voting, in which the fraction of convicted defendants who are innocent goes to zero as the size of the jury increases.

CONVICTING INNOCENT DEFENDANTS

In this section we explicitly compute an equilibrium under the unanimity rule. We then demonstrate that in this equilibrium innocent defendants are convicted with strictly positive probability even when the jury size is very large. This contrasts with the result under informative voting, in which the probability of convicting any defendant goes to zero as the size of the jury grows.

For the subsequent analysis we examine *symmetric*

[8] To see this, observe that $\beta(n-1, n) = p^{n-1}(1-p)/(p^{n-1}(1-p) + (1-p)^{n-1}p) = 1/(1 + (1-p/p)^{n-1}p)$. Since $p > 1/2$, it follows that $\lim_{n\to\infty} \beta(n-1, n) = 1$.

Nash equilibria, in which all jurors who receive the same signal take the same (possibly mixed) action. We therefore drop the subscript identifying particular jurors from our notation and write $\sigma(s)$ to denote the probability a juror votes to convict upon observing signal s. A symmetric strategy profile is given by $(\sigma(i), \sigma(g))$.

Under any voting rule there is a Nash equilibrium in which all jurors vote the same way independent of their signal; for example, under the unanimity rule all jurors may vote to acquit independent of their private signal. Since no juror can influence the outcome, this is always an equilibrium.[9]

In the following we focus on profiles in which jurors change their vote as a function of their private information with positive probability. We call such profiles responsive. In order to formalize our definition of responsive profiles we first define

$$\gamma_G = \sigma(g)p + (1-p)\sigma(i)$$

as the probability that a juror votes to convict if the defendant is guilty and

$$\gamma_I = (1-p)\sigma(g) + p\sigma(i)$$

as the probability that a juror votes to convict if the defendant is innocent. We say that the profile $(\sigma(g), \sigma(i))$ is responsive if $\gamma_G \neq \gamma_I$.

If condition 1 holds, then informative voting is not an equilibrium, and any responsive symmetric equilibrium must be in mixed strategies. More precisely, each juror must both vote to convict and vote to acquit with positive probability whenever she receives a signal i, that is, $\sigma(i) > 0$. When the juror receives the signal g, she votes to convict with probability 1, that is, $\sigma(g) = 1$.[10]

For a mixed strategy profile to be an equilibrium, a juror who receives an innocent signal must be indifferent between voting to acquit and voting to convict. This occurs when, conditional on $n-1$ others voting guilty and the juror receiving signal i, the probability that the defendant is guilty is exactly equal to q. By Bayes's law we get the following equilibrium condition for the unanimity rule:

$$\frac{(1-p)(\gamma_G)^{n-1}}{(1-p)(\gamma_G)^{n-1} + p(\gamma_I)^{n-1}} = q. \tag{2}$$

Therefore,

$$\sigma(i) = \frac{\left(\dfrac{(1-q)(1-p)}{qp}\right)^{1/(n-1)} p - (1-p)}{p - \left(\dfrac{(1-q)(1-p)}{qp}\right)^{1/(n-1)} (1-p)}. \tag{3}$$

Since $\sigma(i)$ is a mixed strategy, it must be that $\sigma(i) \leq 1$. Furthermore, in a responsive equilibrium it must be the case that $\sigma(i) < 1$; otherwise, each juror votes to convict with probability one irrespective of the signal. Examining equation 3 we see that $\sigma(i) < 1$ as long as $q > 1 - p$. If $q \leq 1 - p$, then there does not exist a responsive equilibrium. Instead, there is an equilibrium where $\sigma(i) = 1$, which implies that the probability of convicting an innocent defendant is one, and the probability of acquitting a guilty defendant is zero.[11]

To understand why there cannot be a responsive equilibrium if $q < 1 - p$, note that in equilibrium a guilty vote of some other juror can never be information in favor of the innocence of the defendant. If a juror receives no information from being pivotal, then he believes that the defendant is guilty with probability $1 - p$ when he receives the signal i. Hence, conditional on his vote being pivotal, each juror must believe the defendant to be guilty with probability at least $1 - p$, and each juror must vote to convict even if he receives the signal i.

Using equation 3 it is straightforward to compute the probability of making each type of error in equilibrium. When $q \geq 1 - p$, the probability that an innocent defendant is convicted is given by

$$l_I(p, q, n) = (\gamma_I)^n$$
$$= \left(\frac{(2p-1)\left(\dfrac{(1-q)(1-p)}{qp}\right)^{1/(n-1)}}{p - (1-p)\left(\dfrac{(1-q)(1-p)}{qp}\right)^{1/(n-1)}}\right)^n,$$

and the probability of acquitting a guilty defendant is

$$l_G(p, q, n) = 1 - (\gamma_G)^n$$
$$= 1 - \left(\frac{(2p-1)}{p - (1-p)\left(\dfrac{(1-q)(1-p)}{qp}\right)^{1/(n-1)}}\right)^n.$$

Proposition 2 summarizes our findings in this section and demonstrates that the probability of convicting an innocent defendant stays bounded away from zero for all n. Similarly, the probability of acquitting a guilty defendant also stays bounded away from zero.

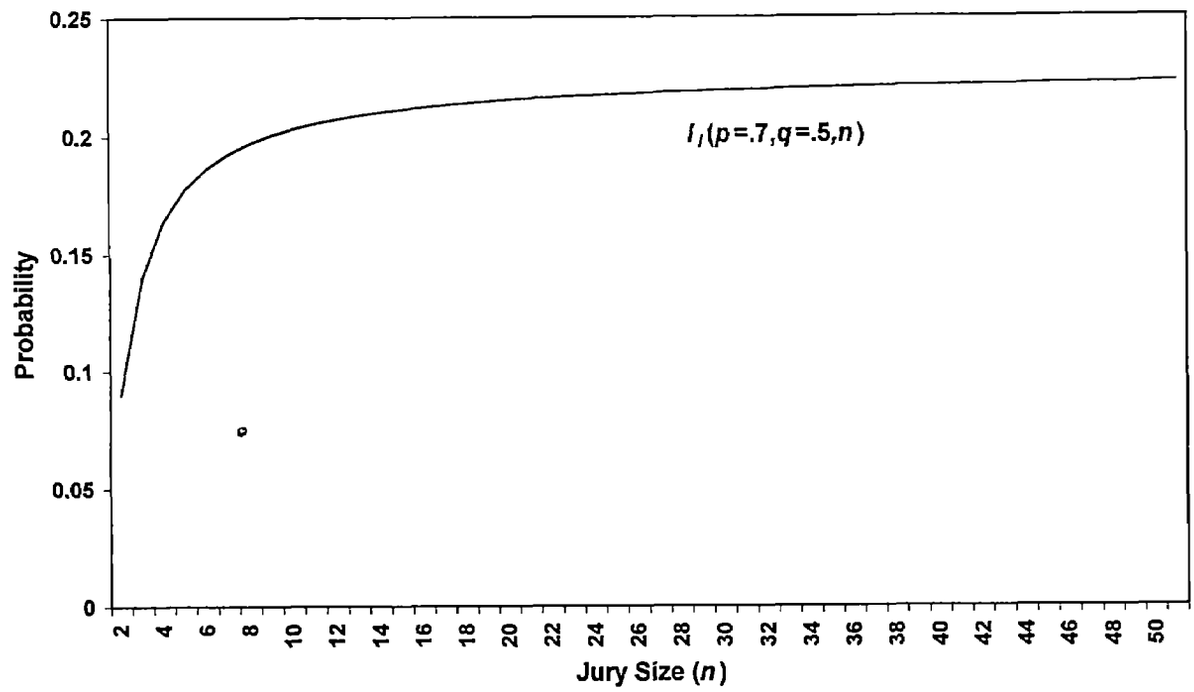
PROPOSITION 2. *Assume condition 1 holds and $q > 1 - p$. The strategy given by equation 3 is the unique responsive symmetric equilibrium for the unanimity rule. Moreover, $\sigma(i) \to 1$ as $n \to \infty$, and*

[9] Even when the unanimity rule is used the profile in which all jurors vote to acquit regardless of their signal is a symmetric equilibrium. Ruling out weakly dominated strategies does not eliminate this equilibrium.

[10] It is straightforward to verify that there are no mixed strategy equilibria in which jurors who receive signal g vote with positive probability to acquit. Support for such a mixed strategy equilibrium requires that those who receive a guilty signal are indifferent between convicting and acquitting, given their vote is pivotal. In such a case those who receive an innocent signal strictly prefer to acquit. But then the only jurors who vote to convict are those who receive the guilty signal. Under the unanimity rule, a vote is only pivotal when all others vote guilty. Thus, such a strategy profile is not a Nash equilibrium, by the same argument used to show that informative voting is not an equilibrium.

[11] Clearly, there also exist equilibria where the defendant is never convicted. For example, if all jurors vote to acquit independent of their signal, then we have a Nash equilibrium, since no juror can influence the outcome.

FIGURE 1. The Probability an Innocent Defendant Is Convicted as a Function of Jury Size



$$\lim_{n\to\infty} l_I(p, q, n) = \left(\frac{(1-q)(1-p)}{qp}\right)^{p/(2p-1)},$$

$$\lim_{n\to\infty} l_G(p, q, n) = 1 - \left(\frac{(1-q)(1-p)}{qp}\right)^{1-p/(2p-1)}.$$

If $q \leq 1 - p$, then there is no responsive equilibrium. In this case $\sigma(i) = 1$ is an equilibrium, and $l_I(p, q, n) = 1$, $l_G(p, q, n) = 0$.

Proof. For $q > 1 - p$ we demonstrated in the text above that the unique responsive voting equilibrium under the unanimity rule is given by equation 3. In Appendix A we show that

$$\lim_{n\to\infty} l_I(p, q, n)$$

$$= \lim_{n\to\infty}\left(\frac{(2p-1)\left(\frac{(1-q)(1-p)}{qp}\right)^{1/(n-1)}}{p-(1-p)\left(\frac{(1-q)(1-p)}{qp}\right)^{1/(n-1)}}\right)^n$$

$$= \left(\frac{(1-q)(1-p)}{qp}\right)^{p/(2p-1)}.$$

The proof that $\lim_{n\to\infty} l_G(p, q, n) = 1 - ((1 - q)(1 - p)/qp)^{(1-p)/(2p-1)}$ is analogous.

If $q \geq 1 - p$, then the argument given in the text shows that there is no responsive equilibrium. *Q.E.D.*

Proposition 2 also implies that the probability of a guilty defendant being convicted $(1 - l_G)$ is bounded away from zero for all n. This is again in contrast to the case of informative voting, in which the probability of conviction converges to zero as $n \to 0$, independent of whether the defendant is guilty or innocent. Thus, a second implication of strategic voting is that the probability of a guilty verdict may be much larger than under informative voting.

To provide an intuition for proposition 2, first observe that equation 3 implies $\sigma(i) \to 1$ as $n \to \infty$. As a consequence, γ_G (the probability that a juror votes to convict if the defendant is guilty) and γ_I (the probability that a juror votes to convict if the defendant is innocent) both converge to one. This is not enough to show that the probability of convicting an innocent defendant, $(\gamma_I)^n$, stays bounded away from zero. In Appendix A we demonstrate that for large n, γ_I can be approximated by

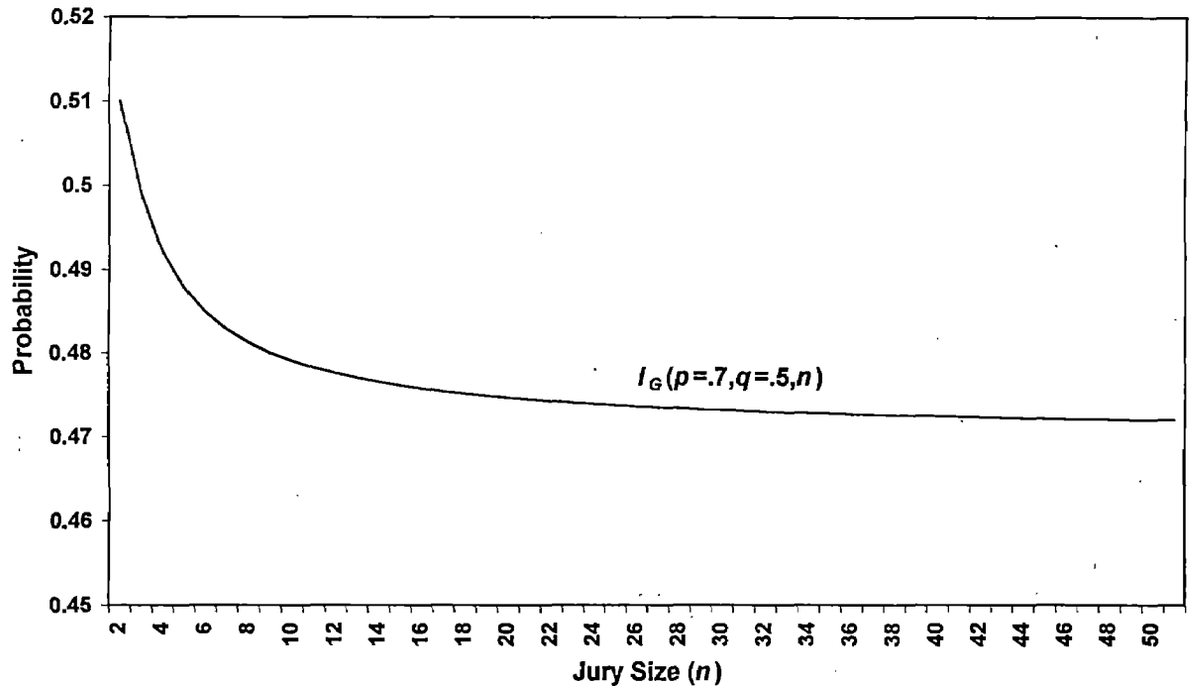
$$1 + \frac{1}{n-1}\left(\frac{p}{2p-1}\ln f\right),$$

where $f = (1 - q)(1 - p)/qp$, and hence $(\gamma_I)^n$ converges to $f^{p/(2p-1)}$, which is the bound given in proposition 2.[12]

The convergence to the bounds given in proposition 2 is fast, and hence the limit formula allows us to approximate the probabilities of each kind of error even for small juries. Figure 1 illustrates the convergence of $l_I(p, q, n)$ for the values $p = 0.7$, $q = 0.5$. The figure is startling for several reasons. First, the limit probability of convicting an innocent defendant is quite large—22%. Second, when there are only 12 jurors the probability of convicting an innocent is 21%.

[12] Recall that $e^x = \lim_{n\to\infty}(1 + x/n)^n$.

FIGURE 2. The Probability a Guilty Defendant Is Acquitted as a Function of Jury Size



There is a difference of only 1% between the probability of convicting an innocent defendant with a jury size of $n \geq 12$ and the limit probability. Third, the probability of convicting an innocent defendant *increases* with the size of the jury.

Figure 2 shows the probability of acquitting a guilty defendant for the values $p = 0.7$, $q = 0.5$. The limit probability of acquitting a guilty defendant is 47%. Once again, the limit probability is a very good estimate of the actual probability of this type of mistake, even for small juries. Note that the probability of acquitting a guilty defendant actually *decreases* with the size of the jury.

Figure 3 shows the limit errors $l_I(p, q) = \lim_{n\to\infty} l_I(p, q, n)$ and $l_G(p, q) = \lim_{n\to\infty} l_G(p, q, n)$ for the value $p = 0.7$ as a function of q. The figure demonstrates that for large juries the probability of convicting an innocent decreases in q, while the probability of acquitting a guilty defendant increases in q. Thus, the unanimity rule does a poor job of protecting innocent defendants from unreasonable juries. Yet, if the jury is responsible (e.g., $q = .9$), the innocent defendant is protected at the cost of a high probability of acquitting the guilty. In the next section we show that the unanimity rule is a uniquely bad voting rule in terms of the probabilities of both kinds of error it induces.

Using the equilibrium strategies calculated in this section, we may also compute the probability that a convicted defendant is innocent and compare it to the bound given in proposition 1. A straightforward calculation demonstrates that

$$\lim_{n\to\infty} \Pr(G|C) = \frac{1-q}{1+q\dfrac{2p-1}{1-p}} > \frac{1-q}{1+q\dfrac{2p-1}{(1-p)^2}},$$

where the right-hand side of the above inequality is the bound given in proposition 1.

NONUNANIMOUS RULES

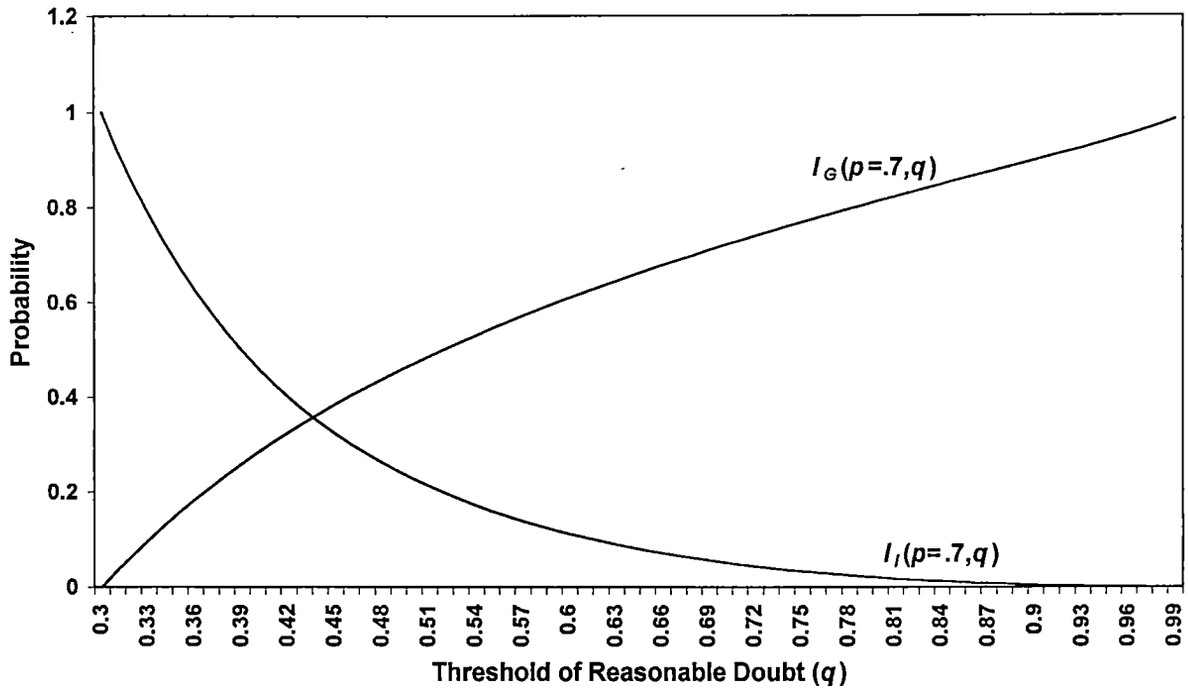
We now analyze the probability of making the wrong decision under nonunanimous voting rules.[13] Suppose $\hat{k} = \alpha n$ for some α with $0 < \alpha < 1$, and assume that αn is an integer. Thus, a defendant will be convicted if and only if at least an α-fraction of the jury votes to convict. For a fixed α, consider a sequence of symmetric responsive equilibria corresponding to an increasing jury size. In the following proposition we show that as $n \to \infty$ the probability of making either of the two kinds of error converges to zero.

PROPOSITION 3. *Fix any α with $0 < \alpha < 1$. (1) There is a n' such that for $n > n'$ there is a symmetric responsive equilibrium. (2) For any sequence of symmetric responsive equilibria the probability of convicting an innocent defendant and the probability of acquitting a guilty defendant both converge to zero.*

Proof. See Appendix B. *Q.E.D.*

[13] See Appendix B for a computation of the (unique) symmetric responsive equilibrium for general $\hat{k}$.

FIGURE 3. Limit Error Probabilities as a Function of the Threshold of Reasonable Doubt



Proposition 3 shows that for any $\alpha \in (0, 1)$ the probability of convicting an innocent defendant and the probability of acquitting a guilty defendant both converge to zero as the jury grows large.[14] This is in sharp contrast to the results of propositions 1 and 2, which showed that both types of mistakes stay bounded away from zero for the unanimity rule.

To provide an intuition for proposition 3, recall that γ_G is the probability a juror votes to convict if the defendant is guilty and γ_I is the corresponding probability if the defendant is innocent. As we noted above, in any responsive profile it must be the case that $1 > \gamma_G > \gamma_I > 0$. Suppose the actual fraction of guilty votes is a. Then probability that the defendant is guilty is given by

$$\frac{(\gamma_G)^{an}(1-\gamma_G)^{(1-a)n}}{(\gamma_G)^{an}(1-\gamma_G)^{(1-a)n} + (\gamma_I)^{an}(1-\gamma_I)^{(1-a)n}}.$$

It follows that if

$$\frac{(\gamma_I)^{a}(1-\gamma_I)^{1-a}}{(\gamma_G)^{a}(1-\gamma_G)^{1-a}} \neq 1,$$

then for large n the defendant is either guilty with probability close to one (if the above fraction is less than one) or innocent with probability close to one (if the above fraction is greater than one).

For any responsive profile there is a unique a^*, such that

$$\frac{(\gamma_I)^{a^*}(1-\gamma_I)^{1-a^*}}{(\gamma_G)^{a^*}(1-\gamma_G)^{1-a^*}} = 1,$$

and $\gamma_G > a^* > \gamma_I$.[15] If the actual fraction of guilty votes is $a < a^*$, then for large n the defendant is innocent with probability close to one; if $a > a^*$, then the defendant is guilty with probability close to one.

In any responsive equilibrium it must be the case that the event a vote is pivotal, that is, an α fraction votes to convict, is not overwhelming evidence of either guilt or innocence.[16] This in turn implies that the a^* implied by a responsive equilibrium must be arbitrarily close to α if n is sufficiently large.

Now recall that a responsive equilibrium may take two possible forms. One is that jurors vote to acquit when they observe signal i and randomize when they observe signal g (i.e., $\sigma(i) = 0$ and $0 \leq \sigma(g) \leq 1$). The other is that jurors vote to convict when they observe signal g and randomize when they observe signal i (i.e., $0 \leq \sigma(i) \leq 1$ and $\sigma(g) = 1$). Figure 4 depicts γ_I, γ_G, and a^* as a function of the strategy profile for $p = 0.7$. It is convenient to represent the strategy by the variable $x \in [0, 2]$. For $x \leq 1$ the strategy is $\sigma(i) = 0$, $\sigma(g) = x$; for $x \geq 1$ the strategy is $\sigma(g) = 1$, $\sigma(i) = x - 1$.

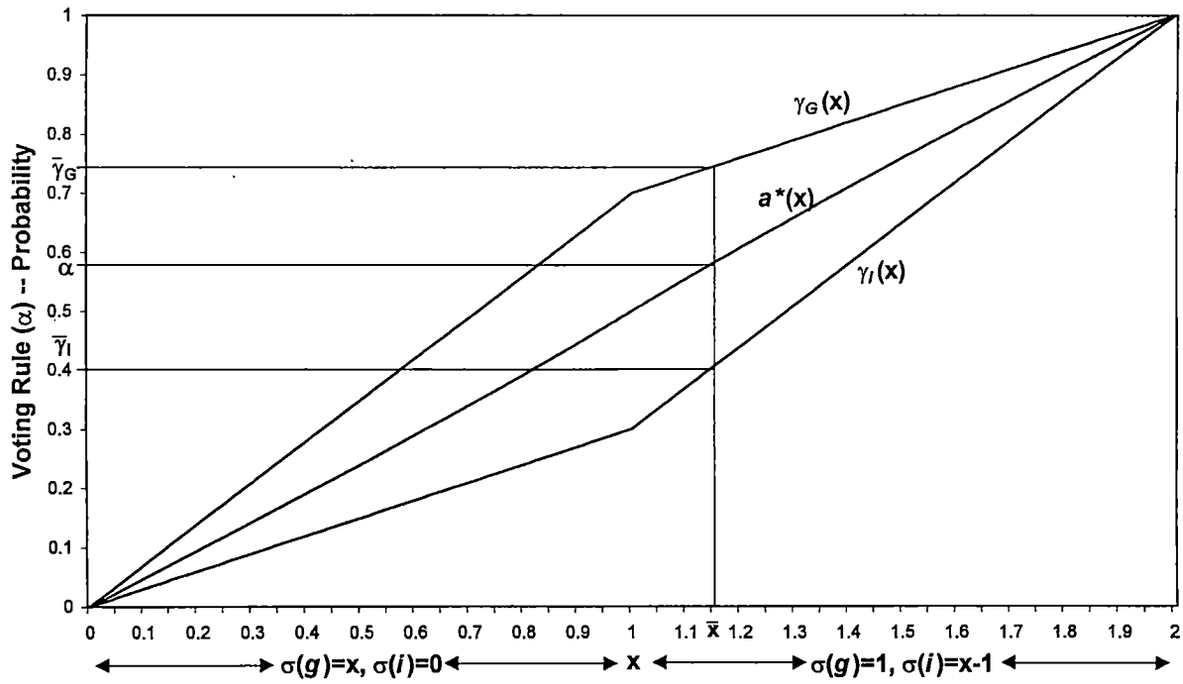
Figure 4 allows us to find the unique symmetric responsive equilibrium for large juries as a function of the voting rule α. Suppose, for example, that α is as indicated in the figure. Since a^* in a large jury is close to α, it must be that the equilibrium strategy profile is

[14] Proposition 3 holds in much more general environments. Feddersen and Pesendorfer (1997a) prove the analogous result for an environment that includes preference diversity and a much broader range of information environments. Myerson (n.d.) proves a similar result for the case of simple majority rule.

[15] It is easy to see that $(\gamma_I)^{a^*}(1-\gamma_I)^{1-a^*}/(\gamma_G)^{a^*}(1-\gamma_G)^{1-a^*} = 1$ implies $a^* = (\ln(1-\gamma_G) - \ln(1-\gamma_I))/(\ln\gamma_I - \ln\gamma_G + \ln(1-\gamma_G) - \ln(1-\gamma_I))$.

[16] Formally, it must be the case in any responsive equilibrium that $\Pr(G|piv, g) \geq q \geq \Pr(G|piv, i)$.

FIGURE 4. Limit Equilibrium Strategies as a Function of the Voting Rule



Note: Calculations assume $p = .7$ and $0 < q < 1$.
The actual functions shown are:

$$\gamma_G(x)=\begin{cases} .7+.3(x-1) & \text{if } x\geq 1 \\ .7x & \text{if } x<1 \end{cases}$$

$$\gamma_I(x)=\begin{cases} .3+.7(x-1) & \text{if } x\geq 1 \\ .3x & \text{if } x<1 \end{cases}$$

$$a^*(x)=\frac{\ln(1-\gamma_G(x))-\ln(1-\gamma_I(x))}{\ln \gamma_I(x)-\ln \gamma_G(x)+\ln(1-\gamma_G(x))-\ln(1-\gamma_I(x))}$$

close to $\bar{x}$ (which corresponds to $\sigma(g) = 1$ and $\sigma(i) = \bar{x} - 1$). Figure 4 also illustrates why large juries rarely make mistakes when $0 < \alpha < 1$. Observe that for large n the equilibrium pair (γ_G, γ_I) is very close to $(\bar{\gamma}_G, \bar{\gamma}_I)$, and $\bar{\gamma}_G > \alpha > \bar{\gamma}_I$. Furthermore, as the size of the jury grows the actual vote share converges in probability to the expected vote share. Therefore, when the defendant is guilty, the actual fraction of guilty votes will be close to $\bar{\gamma}_G$, with probability close to one, and the defendant will be convicted. Conversely, when the defendant is innocent, the actual fraction of guilty votes will be close to $\bar{\gamma}_I$, with probability close to one, and the defendant will be acquitted. By contrast, consider the unanimity rule ($\alpha = 1$). In order for a^* to be close to one it is necessary that both γ_G and γ_I be close to one, and therefore the above argument fails.

Propositions 1, 2, and 3 imply that the unanimity rule is *uniquely bad* for large juries. A second conclusion that can be drawn from proposition 3 is that under nonunanimous rules the size of a jury is more important in determining the probability of making a mistake in the verdict than is the voting rule. Therefore, if the probability an innocent defendant is convicted is considered to be too large under a nonunanimous rule, then the remedy is not to change the rule but to increase the size of the jury.

In the next section we provide an example which suggests that convergence is fast, and hence our convergence results are indeed relevant for relatively small juries.

EXAMPLE

We consider a 12-person jury, $n = 12$. We set the parameter for reasonable doubt at $q = 0.9$, that is, jurors need to believe that the defendant is guilty with probability 0.9 in order to convict. We assume that $p = 0.8$, that is, the probability of receiving a guilty signal if the defendant is guilty is 0.8.[17]

The probability of convicting an innocent defendant, $l_I(\hat{k})$, is given by

$$l_I(\hat{k}) = \sum_{j=\hat{k}}^{n} \binom{n}{j} (\gamma_I(\hat{k}))^j (1 - \gamma_I(k))^{n-j},$$

where

$$\gamma_I(\hat{k}) = (1 - p)\sigma(g)(\hat{k}) + p\sigma(i)(\hat{k})$$

is the probability that any juror votes for conviction if the defendant is innocent.

[17] It follows that a jury of one would never convict, since $\beta(1, 1) = .8 < .9 = q$.

TABLE 1. An Example of Error Probabilities and Equilibrium Strategies in a 12-Person Jury When $\hat{k} \geq 7$

Voting Rule $\hat{k}$	7	8	9	10	11	12
l_I	.004	.0011	.0025	.0045	.0066	.0069
l_G	.019	.0660	.1350	.2450	.4200	.6540
$\sigma(i)$	0	.0230	.1430	.2770	.4230	.5750

Note: We assume that $p = .8$ and $q = .9$. Note that l_I is the probability an innocent defendant is convicted and l_G is the probability a guilty defendant is acquitted. The full equilibrium profile is $\sigma(g) = 1$ and $\sigma(i)$ as specified in the table.

Similarly, the probability of acquitting a guilty defendant, $l_G(\hat{k})$, is given by

$$l_I(\hat{k}) = 1 - \sum_{j=\hat{k}}^{n} \binom{n}{j} (\gamma_G(\hat{k}))^j (1 - \gamma_G(k))^{n-j},$$

where

$$\gamma_G(\hat{k}) = p\sigma(g)(\hat{k}) + (1 - p)\sigma(i)(\hat{k}).$$

Table 1 gives the probability of making mistakes as a function of the rule $\hat{k}$ when $\hat{k} \geq 7$. At $\hat{k} = 7$, informative voting is the equilibrium. Thus, every juror who receives a signal g votes to convict, and every juror who receives a signal i votes to acquit. For all $\hat{k} > 7$, the jurors who receive the signal i mix between voting to convict and voting to acquit, while those who receive signal g always vote to convict. As the table shows, the unanimity rule has the highest probability of convicting an innocent defendant when $\hat{k} \geq 7$. In addition, all rules with $\hat{k} \geq 7$ have the property that they lead to a lower probability of acquitting a guilty defendant than the unanimity rule.

Table 2 shows the probability of making an error when $\hat{k} < 7$. For $\hat{k} \leq 6$ the equilibrium strategies are such that a juror who receives the signal i always votes to acquit, while a juror who receives the signal g mixes between voting to convict and voting to acquit. For $\hat{k} = 1$ the defendant is never convicted, that is, $l_G = 1$. To see why this is the case, suppose that no juror ever votes to convict. In this case, each juror is always pivotal, since one guilty vote is enough for a conviction. But this implies that the only information the juror has conditional on his vote being pivotal is his own signal. Conditional on having received signal g, the juror believes that the defendant is guilty with probability 0.8. Since the reasonable doubt threshold is 0.9, each juror votes to acquit.

TABLE 2. Error Probabilities and Equilibrium Strategies in a 12-Person Jury When $\hat{k} < 7$

Voting Rule	1	2	3	4	5	6
l_I	0	.0159	.034	.027	.019	.010
l_G	1	.780	.410	.210	.095	.036
$\sigma(g)$	0	.091	.306	.512	.704	.879

Note: The full equilibrium profile is $\sigma(i) = 0$ and $\sigma(g)$ as specified in the table.

The probability of convicting an innocent defendant reaches a maximum at $\hat{k} = 3$, while the probability of acquitting a guilty defendant is monotonically decreasing for $\hat{k} \leq 6$, and the probability of convicting an innocent defendant is zero at $\hat{k} = 1$.[18]

CONCLUSION

We demonstrated that strategic behavior dramatically alters our intuitions about the consequences of jury voting rules. When jurors vote strategically, the unanimity rule results in a strictly positive probability both of acquitting the guilty and convicting the innocent, even for very large juries. Increasing the size of the jury does not help and actually may increase the probability of convicting an innocent defendant. Finally, in large juries the unanimity rule is inferior to a variety of other rules.

It is appropriate to conclude with a note of caution. Jury reform is not an abstract proposition. A group called Citizens for a Safer California proposed the Public Safety Protection Act of 1996. It would eliminate the unanimous verdict in all but capital murder cases and replace it with a rule requiring only 10 of 12 jurors to convict.[19] Our results lend some support to such an initiative. Yet, retaining the unanimity rule in capital cases is exactly the wrong thing to do. Presumably, the motive for retaining it is to protect against the terrible consequences of convicting an innocent. Our results suggest that it would be better to combine a supermajority rule with a larger jury for cases in which it is desirable to reduce the probability of convicting an innocent.

More important, our results depend on the assumptions of private information and strategic voting. As noted above, there is some experimental evidence that strategic voting will occur in the presence of private information and common values (Ladha, Miller, and Oppenheimer 1996). There is also some empirical support for the assumption that deliberations fail to eliminate private information, and hence jurors have private information at the final voting stage. Selvin and Picus (1987, 24) conducted interviews with jurors after the verdict and found significant differences in their information about the facts of the trial. The degree to which strategic voting and private information characterize actual juries is ultimately an empirical question and beyond the scope of this article.

The final caveat is that criminal trials in the United States have at least three possible outcomes: convic-

[18] In this case, conditional on a vote being pivotal, a juror knows that all other jurors must have voted to acquit. In equilibrium this can never be information in favor of the guilt of the defendant. If the juror receives a guilty signal, he therefore believes the defendant to be guilty with probability at most p. Since $p < q$, the juror votes to acquit.

[19] The group claims that a "broad coalition of crime victims, law enforcement and concerned citizens" support the proposed legislation. It also claims that an independent poll shows 71% of Californians support 10:2 jury verdicts. This information was downloaded from the Internet at http://taren.ns.net/cdaa.htm, October 1996.

tion, acquittal, or mistrial. A unanimous vote is required for either conviction or acquittal. The effect of strategic voting and private information in this setting has been examined by Coughlan (1997). He shows that requiring juries to reach a unanimous verdict minimizes the probability of both types of error when a hung jury always results in a retrial. As we showed above, however, the probability of errors can be made arbitrarily small when a single jury verdict is decisive by using a nonunanimous rule and increasing the size of the jury. Hence, if retrying the defendant is very costly, unanimity remains an inferior rule.

APPENDIX A

Proof of Proposition 1

Let $\sigma = \{(\sigma_j(i), \sigma_j(g)\}_{j=1}^n$ be a Nash equilibrium profile. There are two types of equilibria in which the defendant is convicted with positive probability. The first type occurs when $1 - p > q$ and all jurors vote to convict independent of their private signal. In this case for any juror j it follows that $\Pr(G|piv_j, i) = 1 - p \geq q$. The defendant is always convicted, and the probability that a convicted defendant is innocent is equal to 1/2.

The second type of equilibrium occurs when there is some juror who votes to acquit with positive probability. Suppose j upon receiving signal s votes to acquit with positive probability, that is, $\sigma_j(s) < 1$. Also note that since the defendant is convicted with strictly positive probability, it follows that each juror is pivotal with strictly positive probability. Since $\sigma_j(s) < 1$, it follows that

$$q \geq \Pr(G|piv_j, s)$$

$$= \frac{\Pr(piv_j|G)\Pr(s|G)(1/2)}{\Pr(piv_j|G)\Pr(s|G)(1/2) + \Pr(piv_j|I)\Pr(s|I)(1/2)}$$

$$= \frac{\dfrac{\Pr(piv_j|G)}{\Pr(piv_j|G) + \Pr(piv_j|I)}\Pr(s|G)}{\left[\dfrac{\Pr(piv_j|G)}{\Pr(piv_j|G) + \Pr(piv_j|I)}\Pr(s|G) + \dfrac{\Pr(piv_j|I)}{\Pr(piv_j|G) + \Pr(piv_j|I)}\Pr(s|I)\right]}$$

$$= \frac{\Pr(G|piv_j)\Pr(s|G)}{\Pr(G|piv_j)\Pr(s|G) + (1 - \Pr(G|piv_j))\Pr(s|I)}.$$

Thus,

$$\Pr(G|piv_j) \leq \frac{qp}{(1-q)(1-p) + qp}. \tag{4}$$

Recall that C denotes the event that the defendant is convicted, and let $\gamma_{Gj} = p\sigma_j(g) + (1-p)\sigma_j(i)$ and $\gamma_{Ij} = p\sigma_j(i) + (1-p)\sigma_j(g)$. Since the defendant is convicted with strictly positive probability, it follows that $\sigma_j(g) > 0$ or $\sigma_j(i) > 0$; therefore, both $\gamma_{Gj} > 0$ and $\gamma_{Ij} > 0$. Hence we may write

$$\Pr(G|C) = \frac{\Pr(G|piv_j)\gamma_{Gj}}{\Pr(G|piv_j)\gamma_{Gj} + (1 - \Pr(G|piv_j))\gamma_{Ij}}.$$

Since

$$\frac{\gamma_{Ij}}{\gamma_{Gj}} \geq \frac{1-p}{p},$$

it follows from 4 and some algebra that

$$\Pr(G|C) \leq \frac{qp^2}{1 - 2p + p^2 - q + 2qp},$$

which gives the result

$$\Pr(I|C) = 1 - \Pr(G|C) \geq \frac{(1-p)^2(1-q)}{1 - 2p + p^2 - q + 2qp}.$$

Q.E.D.

Proof of Proposition 2

We now show that

$$\lim_{n\to\infty}\left(\frac{1}{\dfrac{p}{2p-1}\left(\dfrac{(1-q)(1-p)}{qp}\right)^{-1/n-1} - \dfrac{(1-p)}{2p-1}}\right)^n = \left(\frac{(1-q)(1-p)}{qp}\right)^{p/(2p-1)}.$$

Let

$$h = \frac{p}{2p-1}\left(\frac{(1-q)(1-p)}{qp}\right)^{-1/(n-1)} - \frac{(1-p)}{2p-1}.$$

Suppose $\infty > \lim_{n\to\infty} h^n > 0$, then $\lim_{n\to\infty} h^{-n} = 1/\lim_{n\to\infty} h^n$. Now let

$$f = \frac{(1-q)(1-p)}{qp}.$$

We use the following facts:

$$\lim_{n\to\infty}\left(1 + \frac{z}{n}\right)^n = e^z, \tag{5}$$

and, given $f \in (0, 1)$,

$$1 + \frac{-1}{n}\ln f \geq f^{-1/n-1} \geq 1 + \frac{-1}{n-1}\ln f. \tag{6}$$

From 6 we know

$$h \geq \frac{p}{2p-1}\left(1 + \frac{-1}{n-1}\ln f\right) - \frac{(1-p)}{2p-1}.$$

Some simple algebra shows that

$$\frac{p}{2p-1}\left(1 + \frac{-1}{n-1}\ln f\right) - \frac{(1-p)}{2p-1} = 1 + \frac{1}{n-1}\frac{-p}{2p-1}\ln f.$$

Thus,

$$\lim_{n\to\infty}(h)^n \geq \lim_{n\to\infty}\left(1 + \frac{1}{n-1}\frac{-p}{2p-1}\ln f\right)^n,$$

and from 5 we get

$$\lim_{n\to\infty}\left(1 + \frac{1}{n-1}\frac{-p}{2p-1}\ln f\right)^n = \left(\frac{(1-q)(1-p)}{qp}\right)^{-p/(2p-1)}.$$

We can make an identical argument using 6 to establish that

$$\lim_{n\to\infty} (h)^n \leq \lim_{n\to\infty} \left(1 + \frac{1}{n}\frac{-p}{2p-1}\ln f\right)^n$$
$$= \left(\frac{(1-q)(1-p)}{qp}\right)^{-p/(2p-1)}. \qquad Q.E.D.$$

APPENDIX B

First, we compute the equilibrium for a general $\hat{k}$. Denote the probability that a juror votes to convict in state I as

$$\gamma_I = (1-p)\sigma(g) + p\sigma(i)$$

and the probability that a juror votes to convict in state G as

$$\gamma_G = p\sigma(g) + (1-p)\sigma(i).$$

When $1 > \sigma(i)$, we must have

$$\frac{1}{1 + \dfrac{p(\gamma_I)^{\hat{k}-1}(1-\gamma_I)^{n-\hat{k}}}{(1-p)(\gamma_G)^{\hat{k}-1}(1-\gamma_G)^{n-\hat{k}}}} \leq q, \tag{7}$$

with equality holding if $1 > \sigma(i) > 0$. Similarly, when $\sigma(g) > 0$ it must be true that

$$\frac{1}{1 + \dfrac{(1-p)(\gamma_I)^{\hat{k}-1}(1-\gamma_I)^{n-\hat{k}}}{p(\gamma_G)^{\hat{k}-1}(1-\gamma_G)^{n-\hat{k}}}} \geq q, \tag{8}$$

with equality holding if $1 > \sigma(g) > 0$.

Second, we show that $1 > \sigma(g) > 0$ implies $\sigma(i) = 0$ (an identical exercise shows that $1 > \sigma(i) > 0$ implies $\sigma(g) = 1$). Suppose $1 > \sigma(g) > 0$; 8 then implies

$$\frac{(1-q)p}{q(1-p)} = \frac{(\gamma_I)^{\hat{k}-1}(1-\gamma_I)^{n-\hat{k}}}{(\gamma_G)^{\hat{k}-1}(1-\gamma_G)^{n-\hat{k}}}.$$

Since $p > 1/2$ we can rewrite 7 as

$$\frac{1}{1 + \dfrac{(1-q)}{q}\left(\dfrac{p}{1-p}\right)^2} = q\,\frac{p^2 - (2p-1)}{p^2 - q(2p-1)} < q,$$

which implies $\sigma(i) = 0$.

Thus, in any responsive equilibrium we must have either $\sigma(i) = 0$ and $\sigma(g) > 0$ or $\sigma(i) < 1$ and $\sigma(g) = 1$. If

$$\frac{(1-p)(p)^{\hat{k}-1}(1-p)^{n-\hat{k}}}{p(1-p)^{\hat{k}-1}p^{n-\hat{k}} + (1-p)p^{\hat{k}-1}(1-p)^{n-\hat{k}}}$$
$$= \frac{1}{1 + \dfrac{p(p)^{\hat{k}-1}(1-p)^{n-\hat{k}}}{(1-p)(p)^{\hat{k}-1}(1-p)^{n-\hat{k}}}} \leq q \tag{9}$$

and

$$\frac{(p)^{\hat{k}}(1-p)^{n-\hat{k}}}{(1-p)^{\hat{k}}p^{n-\hat{k}} + p^{\hat{k}}(1-p)^{n-\hat{k}}}$$
$$= \frac{1}{1 + \dfrac{(1-p)(p)^{\hat{k}-1}(1-p)^{n-\hat{k}}}{p(p)^{\hat{k}-1}(1-p)^{n-\hat{k}}}} \geq q, \tag{10}$$

then the unique responsive voting equilibrium is $\sigma(i) = 0$ and $\sigma(g) = 1$. (Recall that a voting equilibrium is a symmetric Nash equilibrium.)

To see why this is the unique responsive voting equilibrium, observe that the left-hand side of 7 is strictly decreasing in $\sigma(i)$. Together with 9 this implies that whenever $\sigma(i) > 0$ (and $\sigma(g) = 1$) every juror has a strict incentive to vote to acquit. Similarly, the left-hand side of 8 is strictly decreasing in $\sigma(g)$. This together with 10 implies that whenever $\sigma(g) < 1$ (and $\sigma(i) = 0$) every juror has a strict incentive to vote to convict.

If either 9 or 10 does not hold, then there are two cases to consider.

Case 1

Suppose

$$\frac{(p)^{\hat{k}}(1-p)^{n-\hat{k}}}{(1-p)^{\hat{k}}p^{n-\hat{k}} + p^{\hat{k}}(1-p)^{n-\hat{k}}} < q.$$

Then $\sigma(i) = 0$ is the equilibrium, and the equilibrium condition for $\sigma(g)$ is defined by 8 with equality holding. This yields

$$\frac{1}{1 + \dfrac{(1-p)(\gamma_I)^{k-1}(1-\gamma_I)^{n-k}}{p(\gamma_G)^{k-1}(1-\gamma_G)^{n-k}}} = q,$$

which we can rewrite as

$$\frac{(1-q)p}{q(1-p)} = \frac{(\gamma_I)^{k-1}(1-\gamma_I)^{n-k}}{(\gamma_G)^{k-1}(1-\gamma_G)^{n-k}}$$
$$= \frac{((1-p)\sigma)^{k_n-1}(1-(1-p)\sigma)^{n-k_n}}{(p\sigma)^{k_n-1}(1-p\sigma))^{n-k_n}}$$
$$= \left(\frac{1-p}{p}\right)^{k-1}\left(\frac{1-(1-p)\sigma}{1-p\sigma}\right)^{n-k}.$$

Therefore, we get

$$\frac{(1-q)}{q}\left(\frac{p}{1-p}\right)^k = \left(\frac{1-(1-p)\sigma}{1-p\sigma}\right)^{n-k}$$
$$\frac{(1-\sigma+p\sigma)}{(1-p\sigma)} = \left(\frac{(1-q)}{q}\left(\frac{p}{1-p}\right)^k\right)^{1/(n-k)}.$$

This yields

$$\sigma(g) = \frac{h-1}{p(h+1)-1},$$

where

$$h = \left(\frac{(1-q)}{q}\left(\frac{p}{1-p}\right)^k\right)^{1/(n-k)}.$$

Clearly, since $\sigma(g)$ is the unique solution of 8 in this case, there is a unique responsive voting equilibrium in this case.

Case 2

Suppose

$$\frac{(1-p)(p)^{\hat{k}-1}(1-p)^{n-\hat{k}}}{p(1-p)^{\hat{k}-1}p^{n-\hat{k}} + (1-p)p^{\hat{k}-1}(1-p)^{n-\hat{k}}} > q.$$

In this case $\sigma(g) = 1$, and the equilibrium condition is given by:

$$\frac{(1-p)(\gamma_G)^{\hat{k}-1}(1-\gamma_G)^{n-\hat{k}}}{p(\gamma_I)^{\hat{k}-1}(1-\gamma_I)^{n-\hat{k}} + (1-p)(\gamma_G)^{\hat{k}-1}(1-\gamma_G)^{n-\hat{k}}} \leq q,$$

with equality holding whenever $\sigma(i) \in (0, 1)$.

A straightforward calculation shows that for an interior solution in this case

$$\sigma(i) = \frac{p(1+f) - 1}{p - f(1-p)},$$

where

$$f = \left(\frac{(1-q)}{q}\left(\frac{(1-p)}{p}\right)^{n-k+1}\right)^{1/(k-1)}.$$

Again, since $\sigma(i)$ is the unique solution of 7 in this case, there is a unique responsive voting equilibrium in this case.

Whenever $\sigma(i)$ as defined by the previous two equations is less than zero, then $\sigma(i) = 0$; whenever $\sigma(i)$ as defined by the previous two equations is larger than 1, then $\sigma(i) = 1$.

Proof of Proposition 2

For sufficiently large n there exists a responsive voting equilibrium. To see this we first compute the limit equilibrium as $n \to \infty$ for the case in which $k/n = \alpha$. In case 2 we have

$$\sigma(i) = \frac{p(1+f) - 1}{p - f(1-p)},$$

where

$$f = \lim_{n\to\infty}\left(\frac{(1-q)}{q}\left(\frac{(1-p)}{p}\right)^{n-\alpha n+1}\right)^{1/(\alpha n-1)} = \left(\frac{1-p}{p}\right)^{1-\alpha/\alpha},$$

and therefore we have

$$\sigma(i) = \frac{p(1+f) - 1}{p - f(1-p)} = \frac{p\left(1 + \left(\frac{1-p}{p}\right)^{(1-\alpha)/\alpha}\right) - 1}{p - \left(\frac{1-p}{p}\right)^{(1-\alpha)/\alpha}(1-p)}.$$

It is easily checked that $1 > \sigma(i) \geq 0$ for $1 > \alpha \geq 1/2$ with $\sigma(i) \to 1$ as $\alpha \to 1$. Similarly, in case 1 we have

$$\sigma(g) = \frac{\left(\frac{p}{1-p}\right)^{\alpha/(1-\alpha)} - 1}{p\left(\left(\frac{p}{1-p}\right)^{\alpha/(1-\alpha)} + 1\right) - 1},$$

and again it can easily be checked that $0 < \sigma(g) \leq 1$ for $0 < \alpha \leq 1/2$ with $\sigma(g) \to 0$ as $\alpha \to 0$.

Together this implies that for any $0 < \alpha < 1$ there is a responsive limit equilibrium. Now a simple continuity argument implies that for sufficiently large n the solution to equations 7 and 8 must be arbitrarily close to the limit solution, and hence it follows that for sufficiently large n there is a responsive voting equilibrium.

To prove part (2) of proposition 3, observe that in the limit as $n \to \infty$ we have

$$\gamma_I = 1 - p + p\,\frac{p\left(1 + \left(\frac{1-p}{p}\right)^{(1-\alpha)/\alpha}\right) - 1}{p - \left(\frac{1-p}{p}\right)^{(1-\alpha)/\alpha}(1-p)}$$

and

$$\gamma_G = p + (1-p)\,\frac{p\left(1 + \left(\frac{1-p}{p}\right)^{(1-\alpha)/\alpha}\right) - 1}{p - \left(\frac{1-p}{p}\right)^{(1-\alpha)/\alpha}(1-p)}.$$

Note that for $0 < \alpha < 1$ this implies that

$$\gamma_G > \gamma_I - \varepsilon,$$

for some $\varepsilon > 0$ which depends on α.

Next we show that

$$\gamma_I < \alpha < \gamma_G. \tag{11}$$

To see why this is sufficient to prove proposition 3, note that by the law of large numbers the actual share of guilty votes converges to the expected share of guilty votes in each state. Hence, the share of guilty votes if the defendant is innocent converges to $\gamma_I < \alpha$ in probability, and the defendant is acquitted with probability close to one for large n. Similarly, if the defendant is guilty, the share of guilty votes converges to $\gamma_G > \alpha$ in probability, and hence the defendant is convicted with probability close to one for large n.

Suppose 11 is violated, and $\gamma_G > \gamma_I \geq \alpha$. From the equilibrium conditions we know that for all n

$$\frac{1}{1 + \frac{(1-p)(g_{ni})^{\alpha n-1}(1-g_{ni})^{(1-\alpha)n}}{p(g^n_{ng})^{\alpha n-1}(1-g^n_{ng})^{(1-\alpha)n}}} \geq q \tag{12}$$

(where g_{ns} denotes the equilibrium probability of a guilty vote in state s with n jurors). Note that $x^\alpha(1-x)^{(1-\alpha)}$ is a single peaked function for $x \in [0, 1]$ with a maximum at $x = \alpha$. If $\gamma_G > \gamma_I \geq \alpha$, then $(\gamma_I)^\alpha(1-\gamma_I)^{(1-\alpha)}/(\gamma_G)^\alpha(1-\gamma_G)^{(1-\alpha)} > 1$, and hence

$$\left(\frac{(\gamma_I)^\alpha(1-\gamma_I)^{(1-\alpha)}}{(\gamma_G)^\alpha(1-\gamma_G)^{(1-\alpha)}}\right)^n \to \infty.$$

The left-hand side of 12 must converge to zero as $n \to \infty$, and hence inequality 12 cannot hold. An analogous argument can be made if $\alpha \geq \gamma_G > \gamma_I$. *Q.E.D.*

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Rational Choice and the Dynamics of Collective Political Action: Evaluating Alternative Models with Panel Data

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Causal inference in research testing rational choice models of unconventional political behavior has been hampered by the inability to use perceptions of the costs and benefits of participation at a given time to predict behavior that necessarily occurred in the past and by ambiguities associated with analyzing behavioral intentions *instead of actual participation. Using panel data collected on a national sample in West Germany between 1987 and 1989, we show that variables from a "collective interest" model measured in 1987—individuals' dissatisfaction with the provision of collective goods, beliefs that group actions can be successful, and beliefs in the importance of their own participation—predict subsequent participation in collective protest activities. Variables corresponding to the private "selective incentives" associated with protest are found to be less relevant. Furthermore, we find that engaging in protest changes many of the perceptions that influence future participation. We discuss the implications for theories of political mobilization.*

Perhaps no area in mass political behavior research is beset with such serious methodological difficulties as the study of participation in political protest and other unconventional activities. Some of the problems, such as finding and eliciting truthful responses from the limited number of individuals who engage in these types of behaviors, can and have been remedied through better sampling and more sensitive interviewing methods in national and local probability surveys. A more fundamental problem, however, is that unlike voting or campaign participation, unconventional behavior does not occur during fixed periods, and hence researchers typically cannot plan a study in advance of the specific activities they may wish to analyze. This means that almost all previous survey-based research on protest and unconventional political action is prone to what Green and Shapiro (1994, 85) term the "pedestrian methodological defect" of predicting *past* behavior from information gathered at the survey's current time.[1]

This error would be less consequential for causal inference if, as was common until recently, the principal explanatory variables of interest were such stable sociodemographic variables as race, gender, educational attainment, or social class or enduring attitudinal orientations, such as ideological or partisan identification (Barnes and Kaase 1979, Marsh 1977). In research of this type, it may be legitimate to assume that the explanatory factors did not change significantly since protest or other behaviors took place, and hence cross-sectional data can be useful in exploring causal hypotheses. More recent work in the field, however, has developed and tested models of unconventional behavior based on rational choice theory, and here the use of cross-sectional data is more problematic. Such models, adapted from the framework set forth by Olson (1965) in the well-known *Logic of Collective Action*, posit that individuals calculate the expected costs and benefits associated with various alternatives before deciding whether to participate in or abstain from collective activities (see the recent reviews in Leighley 1995, Whiteley 1995). Numerous studies drawing on the Olsonian *Logic* have appeared in the literature. Some identify the private payoffs, usually of a social or psychological nature, that allegedly motivate unconventional action (Chong 1991, Klandermans 1984, Opp 1989). Others show that individual preferences for public goods, coupled with estimates of the likelihood of group success in providing the goods through collective behavior and perceptions of the importance of personal participation, are the critical explanatory variables (Finkel et al. 1989, Muller and Opp 1986, Muller et al. 1991). All this work, however, must be viewed as incomplete in light of the fact that decisive tests of rational choice theories cannot be conducted with cross-sectional data using individuals' reports of past behavior.

Most obviously, an individual's "expected" utility from participation, by definition, refers to some future state, rendering reports of past participation irrelevant

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The data analyzed here were collected by Karl-Dieter Opp of the Universitaet Leipzig with the support of the Volkswagenstiftung. Finkel wishes to thank Paul Kingston, Paul Freedman, Benjamin Highton, Steven Schier, Paula McClain, several anonymous *APSR* referees, and especially Karl-Dieter Opp for valuable criticisms and comments. He is also grateful for the support of the University of Virginia's Center for Advanced Studies, where he was a Sesquicentennial Associate during the fall 1994 semester, when this work began, and in fall 1997, when it was completed. Finally, he wishes to acknowledge all the efforts of the University of Arizona's Department of Political Science in honoring Ned Muller's memory since his tragic death in summer 1995.

[1] Throughout the rest of the paper, the term "protest" connotes participation in a variety of legal and illegal collective activities, such as collecting signatures for petitions, taking part in a permitted demonstration, or taking part in public disorders. Some of the behaviors we analyze, such as working with a citizen's action group or signing a petition, are not unconventional in the usual sense, but we use the term "protest" to describe them, as all involve some form of organized demonstration of disapproval of governmental policy. The theoretical discussion is applicable to all forms of collective political action.

from the point of view of rational choice theory. More practically, the variables critical to these theories, such as an individual's perceptions of the likelihood of group success or perceptions of material or social costs associated with participation in collective action, may not remain stable over time; indeed, to the extent that they are relevant predictors of participation, they should be precisely those attitudes and perceptions that protest groups and the government attempt to manipulate in order to encourage or discourage these forms of behavior (Lichbach 1995, chapter 3 and 8–10). Therefore, it cannot be assumed that the values of these variables at the time of a survey were similar to their values before the individuals actually took part in (or abstained from) protest or other unconventional activities (Pierce and Converse 1990). Moreover, individuals who have participated in protest may change their attitudes as a result of their past participation, either by absorbing new information about a given situation and their own capabilities or by rationalizing to themselves or the survey interviewer that their actions "must have" been due to their high levels of alienation, their strong belief that the protest group could be successful, and the like. For these reasons, cross-sectional data cannot show conclusively that current perceptions relevant to rational choice theories are the cause, and not the result, of the reports of individual behaviors that are elicited in a survey interview.[2]

One solution to this problem is to alter the dependent variable to represent the individuals' future *willingness* or *intention* to participate in protest activities. Much research in the field does make use of behavioral intentions as the dependent variable, sometimes weighting these intentions with reports of past behavior in order to make the intentions variable "more realistic" (following the procedures outlined by Muller 1979). Although these models help overcome the temporal ordering problem, they are unsatisfactory to the extent that, as is often the case, the relationship between behavioral intentions and actual behavior is weak. One reason is that there may be few collective protest opportunities in a given period, and so even highly "willing" individuals will not engage in actual behavior. Another is that we know that the relationship between voting intentions and voting turnout is less than perfect (Silver et al. 1986, Granberg and Holmberg 1988), and there is little reason to expect a *greater* degree of consistency between intentions and behavior in more irregular and sometimes illegal forms of political participation. Finally, the relationship between attitudinal independent variables and willingness to participate may be subject to rationalization effects, in that individuals who claim, for example, to be dissatisfied with government and who perceive that protest can be successful may reason that, with this constellation of attitudes, they "must be" willing to participate in protest behaviors. Alternatively, individuals who intend to protest may deduce that they "must have" attitudes commensurate with this behavioral intention. As Aldrich (1976, 322) notes in regard to voting and campaign participation, "it may well be that a citizen responds that he is greatly concerned about the election, that he believes the election to be very close . . . because he has already decided—perhaps, for quite different reasons than his responses might indicate—to go to the polls." For these reasons, the use of behavioral intentions in survey-based research on protest participation may be highly problematic.

To overcome these "pedestrian methodological defects," survey data are needed that provide individual reports of actual participation in protest activities over some finite period, as well as attitudes pertaining to rational choice theories that are measured *before* the behavioral time interval of interest. Clearly, the only type of study that provides these necessary data is a panel survey, in which interviews are conducted with the same individuals over time. *Yet, no panel study testing a model of this type exists in the literature.* Although there has been one large-scale cross-national panel study of protest in the late 1970s and early 1980s, along with several analyses of panel surveys of activist groups, or university and local area samples (Finkel 1987, Jennings et al. 1990, Klandermans 1984, Oegema and Klandermans 1994, Opp n.d., Opp and Ruehl 1990), none of this research contains the most basic test that should be conducted with panel data: whether attitudes relevant to rational choice theories that are measured at a given time are independent causes of the individual's *subsequent* behavior. By specifying that attitudes must exert influence on behavior measured at some future time, such a panel model would represent a more conservative test of whether rational choice variables are related to political participation. Furthermore, few studies exploit panel data to determine the extent to which theoretically relevant independent variables may themselves be affected by protest participation, producing either a reciprocal causal relationship with behavior or a unidirectional relationship in which the causal effect runs only from behavior to attitude (Finkel 1987, Opp n.d.).

In this paper, we attempt to resolve these controversies by testing rational choice theories of participation with national survey panel data on political protest collected over time in the Federal Republic of Germany. Using individuals' reports of their actual participation in legal and illegal protest, and attitudes and perceptions measured before the behaviors in question, we test models of the causes and effects of individual protest participation on variables associated with several variants of rational choice theory. Our findings suggest that individual participation in protest is determined primarily by variables from what we term

[2] Some cross-sectional work on political participation (e.g., Verba et al. 1995) attempts to circumvent the simultaneity problem by using two-stage least squares (TSLS) or some related statistical procedure. This is a clear improvement over the use of ordinary least-squares (OLS) regression, but some difficulties remain in applying this technique. It is still problematic theoretically to assume that an attitude at a given time (even if purged of its association with participation's error term) has a causal effect on reported behavior that occurred in the past. Moreover, TSLS depends on specifying outside instrumental variables that have a relatively strong effect on either participation or the attitude in question (but not both), and such variables are notoriously difficult to find.

a *collective interest* model: individual preferences for public goods, perceptions of the importance of personal participation in collective efforts to achieve the good, and perceptions of the likelihood of group success. Variables associated with private material, social, or psychological "selective incentives" such as monetary payoffs, social pressure from significant others, and the expressive benefits of participation, are found to be less relevant as determinants of protest behavior. Moreover, several of the variables in the collective interest model are found to have a reciprocal relationship with protest behavior, as protest groups attempt to influence these perceptions and attitudes through the mobilization process itself. By contrast, the links between the selective incentives and participation, when they exist at all, are primarily unidirectional, as individuals adjust their current expectations of private rewards and costs in response to past participation in (or abstention from) collective protest activities. These findings have considerable implications for understanding both the dynamics of protest participation and the explanatory power of alternative versions of rational choice theories of collective behavior.[3]

TESTING RATIONAL CHOICE MODELS OF PROTEST: PROBLEMS OF CAUSAL INFERENCE

The logic of individual participation in collective action, derived from Olson's (1965) seminal exposition, is well known. In large groups, each individual reasons first that his or her participation will have little marginal effect on the likelihood that collective action will be successful. Furthermore, individuals who do not participate cannot normally be denied the benefits of the "public goods," for example, a change in regime or public policy, that successful collective action can provide. As a result, rational individuals will prefer to abstain, or "free-ride" on the efforts of others, reasoning that abstention will bring the same expected benefits as participation without any of the potentially severe personal, legal, and opportunity costs involved in protest behavior.

How, then, do we account for the fact that individuals do take part in collective protest activities? Although Lichbach (1995) identifies about two dozen different potential "solutions" to the free-rider problem, scholars in the survey research tradition generally have focused their efforts in two directions: (1) measuring individual demand for public goods and perceptions of individual and group efficacy to determine whether the combination of these *collective interest* variables yields, *contra* Olson, a nonzero expected benefit from participation, and (2) specifying the private payoffs or *selective incentives* that are available only to collective action participants and hence may produce greater expected benefits to the individual from participation than abstention. Empirical research has shown support for these efforts in tests conducted with cross-sectional data in a variety of national and local settings (Finkel et al. 1989; Finkel and Opp 1991; Gibson 1997; Klandermans 1984; Klandermans and Oegema 1987; Muller and Opp 1986; Muller et al. 1991; Opp 1988, 1989; Walsh and Warland 1983).[4] Although the presumption in such studies is that these "expected utility" incentives provide motivation for individual participation in collective political action, there are several reasons to suspect that the effect of past behavior (or current behavioral intention) on current attitudes accounts for at least part of the cross-sectional covariation between these variables.

Consider first the collective interest model, elaborated most recently in Finkel et al. (1989) and Muller et al. (1991). This model posits that individuals will participate in protest activities to the extent that (1) they have high levels of discontent with the current provision of public goods by the government or regime, (2) they believe that collective efforts can be successful in providing desired public goods; and (3) they believe that their own participation will enhance the likelihood of the collective effort's success (see also Klandermans 1984). The positive expected utility of participation over abstention in this model follows from the fact that, in contrast to the Olsonian version of conventional rational choice theory, individuals will not necessarily perceive that the importance of their own participation in the collective effort is negligible. The perceived importance of personal participation may stem either from an individual's general sense of political resourcefulness or subjective efficacy (cf. Verba et al. 1995) or from the individual's adherence to strategic beliefs, such as "united we stand, divided we fall," that promote what Finkel et al. (1989) termed "collective rationality." In either case, discontented individuals will be motivated to participate so long as the overall likelihood of group success is reasonably high; if the cause is hopeless, then even subjectively resourceful individuals will realize that there are too few participants to make a difference, and appeals to group unity will have little effect. The model thus posits that individuals will attempt to achieve collective goods through political participation, but that rational individuals will do so only when the collective chances of success and their own personal influence are high.

Although the collective interest model has received strong support in these cross-sectional studies, several alternative causal processes cannot be ruled out in the tests conducted to date. The problems stem from the

[3] We note that the paper is limited to evaluating alternative models of collective action derived from rational choice theory, not all extant theories of political participation, such as relative deprivation, the SES and resource-based models found in Verba et al. (1995), and the "opportunity structure" models developed by Tarrow (1995) and others. Future research should explore the extent to which variables from these theories represent antecedents to the "inside the head" subjective expected utility judgments that, from the perspective of rational choice theory, are the proximate causes of participation in collective political action.

[4] This is not to suggest that these "solutions" may be found *only* in the survey-based literature. Chong (1991), Lohmann (1994), and Oberschall (1980, 1994), for example, use them to explain past social movements, such as the American civil rights movement and the East German revolution of 1989.

use of behavioral intentions and past participation as dependent variables, both of which may be the cause, and not the effect, of the model's independent variables. One possibility is that respondents are bringing their attitudes in line with their behavioral intentions through a rationalization process: People at a given time may be willing to take part in protest activities for reasons unrelated to the collective interest model and then report a high degree of political discontent or perceptions of the likelihood of group success as a result. Green and Shapiro (1994, 85–6) seem to have this possibility in mind when they suggest that individuals' (mis)perceptions of personal and group influence are part of the "ideology of activism," by which individuals who are otherwise oriented to protest may be inclined to claim that they and the groups to which they belong have much influence on the potential provision of public goods.

The problem of reciprocal causal influence is perhaps even more serious in analyses in which the dependent variable is past participation, or behavioral intentions *weighted* by past participation. Previous research with panel data has suggested that participation in collective action may have an effect on both preferences for public goods and perceptions of personal influence (Finkel 1987, Opp n.d.). More generally, much recent scholarship supports the notion that the beliefs and values relevant for protest are "temporally variable and can be modified during the course of actual participation and by the micromobilization efforts of social movement organizations" and protest groups (Snow et al. 1986, 471). Groups attempt to mobilize individuals by "identifying a problematic condition and defining it as unjust, intolerable, and deserving of corrective action," that is, changing individuals' preferences for public goods (Snow and Oliver 1993, 31–3); by "amplify[ing] . . . beliefs regarding the efficacy of their campaigns" (Snow et al. 1986, 471); and by stimulating a sense of group unity and necessity of each individual's participation (Gamson 1992, Schwartz and Paul 1992). To the extent that these processes occur, then past behavior and participation in mobilization efforts will have significant effects on current levels of preferences for public goods and individual and group efficacy, thus biasing existing cross-sectional analyses that include past behavior in the construction of the dependent variable.

Similar problems pervade research that, in contrast to the collective interest model, emphasizes the private payoffs or selective incentives that may overcome the pressure for individuals to free ride in collective action situations. Previous research has suggested that social and psychological incentives are far more relevant for protest behavior than material rewards and costs (Klandermans 1984, Muller et al. 1991, Opp et al. 1995; but see Lichbach 1994). Yet, for these types of variables, there are strong possibilities of attitude rationalization and reciprocal causal relationships that may contaminate causal inference in cross-sectional studies. Following "resource mobilization" theories of collective action, much research has hypothesized that individuals respond to the norms and expectations of other people within their social network and hence derive benefits from adhering to the behavioral norms of individuals and groups with whom they identify (Klandermans 1984, McAdam and Paulsen 1993, Opp 1989). Friedman and McAdam (1992, 161) argue explicitly that social networks draw individuals into collective action by providing incentives, such as friendship or social honor, that may be obtained by "responding to the call of network partners," and Morton (1991) and Uhlaner (1989) express similar sentiments in their work showing how group memberships and strategic group interactions may overcome pressures for individuals to free ride. Yet, certainly individuals' reports of expected social pressures in a survey situation are subject to rationalization effects, and it is equally plausible that integration into groups or association with significant others who encourage protest is the result of past activities and not necessarily an independent influence on current behavioral intentions or future behavior.

These alternative possibilities may be even more pervasive in the case of the theoretically controversial "psychic," "expressive," or "in-process" rewards that have been put forward as possible motivations for participation. Recent studies have hypothesized that a variety of these "soft incentives" may motivate participation: Individuals may derive psychic satisfaction or suffer guilt from adhering or not adhering to personal norms about taking part in illegal political actions; they may participate in order to "stand up for what they believe in," to gain knowledge about politics, or simply because of the sheer entertainment value involved in group political activities (Finkel and Opp 1991, Muller et al. 1991, Opp 1989, Whiteley et al. 1994). But there is also the possibility that individuals' past behavior and current behavioral intentions have strong effects on responses to survey questions regarding these types of rewards. Elster (1989, 214) speculates that internalized norms of behavior "may be mere rationalizations with no independent motivating power," and it is easy to imagine that individuals who have participated in the past would claim to expect some psychic reward from "standing up" for their political beliefs through collective protest. Thus, the possibilities of attitude rationalization or reciprocal causal influence between attitudes and behavior means that cross-sectional tests of the effects of selective incentives on protest, as well as the effects of the collective interest variables, must be viewed as inconclusive.

DATA AND METHODS

These difficulties can be overcome to a large extent through the analysis of panel data, in which attitudes, cognitions, and behaviors are measured over at least two points in time. Panel data offer several key advantages for our current concerns.[5] First, they enable us to model the effects of expected utility variables measured at one point in time on an individual's self-reported

[5] Kessler and Greenberg (1981) and Finkel (1995) discuss the advantages of panel data in more detail.

behavior at a later date. The analysis of actual behavior is a clear improvement over the reliance of previous research on behavioral intentions or some hybrid dependent variable; equally important, by using wave 1 attitudes to predict behavior during the wave 1 to wave 2 interval, the potential simultaneity biases that exist in cross-sectional designs are eliminated. In addition, the inclusion of prior participation measured in wave 1 as a predictor of wave 1–2 behavior serves partially to control for omitted variables that influence behavior at both periods. Thus the panel design will provide a more rigorous and conservative test of the effects of collective interest variables and selective incentives on the individual's subsequent decisions to participate in collective protest behaviors, controlling for past participation.

Second, the analysis of panel data allows us to take into account the possibility of dynamic reciprocal relationships between all independent variables and participation. That is, in addition to predicting subsequent participation from wave 1 attitudes, we may also use reports of participation in the first wave to predict the collective interest and selective incentive variables measured in wave 2. The results of all these tests will enable us to assess whether the collective interest and selective incentives are reciprocally related to protest; whether the attitudes cause, but are not caused by, participation in collective protest; or whether the sole causal effect between the variables runs from participation to attitudes and not the reverse. In the latter case it may be concluded that individuals are adjusting their perceptions of costs and benefits to rationalize prior behavior, which was caused by other factors altogether.

We test these models with panel data from a representative survey ($N = 714$) of adults (age eighteen and older) conducted in West Germany between November 1987 and January 1988.[6] Reinterviews with 377 respondents were conducted between October 1989 and January 1990. We analyzed the differences between the respondents who were reinterviewed in the second wave ($N = 377$) and those who were not reinterviewed ($N = 337$) by comparing the means of all independent variables and the participation scales for the first wave. The only significant difference to emerge between the panel participants and other respondents was for the former to have slightly lower scores (2.2 to 2.3 on a five-point scale, eta = .09) on an "internal behavioral norm" that measures the extent to which the respondent believes political protest and violence are morally justified (the index is described in more detail below). We thus conclude that the panel respondents are an unbiased sample of first-wave respondents.

The main drawback to this analytic approach, however, is that because collective protest participation is relatively uncommon, our sample of 377 panel respondents did not yield a large number of actual participants. To maximize the variability on the dependent variable, we chose to combine a number of acts of unconventional participation into one scale. We constructed a variable representing the individual's participation in each panel wave by counting the number of the following activities in which the respondent had engaged "within the past two years": took part in a permitted demonstration; signed a petition; worked with a citizen's action group; participated in an organized effort to solve a neighborhood problem; wore a button or a sticker for a political cause; collected signatures for a petition; took part in a demonstration that broke the law; seized buildings, such as factories or government or university offices; participated in confrontations with police or other governmental authorities; participated in political activities that resulted in property damage; participated in protest activities at the workplace which were against the law; participated in confrontations with other political groups or individuals; and took part in public disorders. These activities vary widely in their nature and severity; nevertheless, they constitute a reasonably reliable scale in both panel waves, with coefficient alpha being .78 in the first wave and .76 in the second. Table 1 shows the distribution on the participation scale, and it can be seen that there is reasonable variation in the number of reported activities within each year. The table indicates that, as expected, the large majority of respondents either were completely inactive or participated in one or two of these behaviors, while about 10% report engaging in five or more activities in both waves.

We measured all independent variables in similar ways as in previous research, with the proviso that incentives for *legal* and for *illegal* behaviors were combined into an additive scale to correspond to the combined nature of the dependent variable. The question wordings (in English) can be found in the Appendix.

Measurement of Collective Interest Variables

Public Goods Dissatisfaction was the average of an individual's score on five measures of policy dissatisfaction: the extent of unemployment; the differences between rich and poor; environmental pollution; nuclear power stations; and the deployment of missiles. Respondents were asked to indicate their concern about each issue (0–4 scale) and then rate the performance of the government in each area (0–5 scale). The concern scores were multiplied by the performance scores, and the variable was then converted to a 0–1 scale, with higher numbers representing higher levels of overall dissatisfaction with the provision of public goods (cf. Finkel et al. 1989).

[6] The data were collected by the Gfm-Getas Survey Research Institute in Hamburg, a firm with expertise in designing and implementing surveys on protest and political participation. Each survey was a probability sample drawn according to the design of the working group of German market research institutes. In this procedure the first step is to select sample points (voting districts). Then the interviewer looks for households according to a random route procedure. Finally, a member of the household is randomly selected to be interviewed. More detailed information about this data set and the broader research project from which it is taken can be found in Finkel et al. 1989, Finkel and Opp 1991, Muller et al. 1991, and Opp et al. 1995.

TABLE 1. Distribution of Sample on Protest Participation, 1987 and 1989 (*N* = 377)

Number of Activities	1987	1989
0	50.9%	44.0%
1	14.1	18.0
2	9.3	11.4
3	8.0	8.8
4	9.0	6.4
5	4.2	5.0
6	1.9	4.5
7	1.9	1.9
8	.8	0.0

Likelihood of Group Success is the product of (1) the extent to which respondents believe that groups have helped their cause in the past through legal and illegal protest and (2) their perception that other people with views similar to theirs are likely to participate in either legal or illegal collective political action. The scores were created by multiplying the "past group success" value by the "willingness of others" value. We then averaged the legal and illegal scores and converted the variable to a 1–5 scale corresponding to the respondent's perception of the likelihood of group success in providing public goods through collective action.

Perceived Personal Influence was the combination of the individual's perceptions of the extent to which s/he "personally could have an influence on politics if [s/he] participated in each of five legal or illegal behaviors, and two questions measuring an individual's general level of perceived influence: "There is no point in getting involved in politics because I would have no influence anyway," and "If I were more involved in politics, I would have more influence on what happens." The variable was converted to a 1–5 scale, with higher values representing higher levels of perceived personal influence.

Necessity of Group Unity was composed of responses to two questions: "Every individual member is necessary for the success of a political group, no matter how large it is," and "For groups to have a reasonable chance of success by means of political action, everyone must contribute a small part." The variable was scored from 1 to 5, with higher values representing a stronger belief in the necessity of group unity.

Measurement of Costs

We included three measures of the costs of participating in collective political action. *Trouble with Police* was measured on a 1–4 scale corresponding to the perceived likelihood that the individual "would get into trouble with the police or the courts" if s/he participated in legal or illegal political action. *Could Get Hurt* used a 1–4 scale measuring the perceived likelihood of physical harm from legal or illegal action. *Take Too Much Time* used a 1–4 scale measuring the perceived time constraints faced by the individual who engages in legal or illegal protest.

Measurement of Selective Incentives

We measured seven selective incentives that individuals may perceive as material, social, or psychological "payoffs" from collective political action. *Financial Gain* was a 1–4 scale corresponding to the likelihood that the individual would "gain financially" if s/he participated in legal or illegal action. *Expectations of Others* was a 1–5 scale corresponding to individuals' perception of the extent to which people who are important to them (spouse, friends, colleagues, or others) would think "very positively" (5) to "very negatively" of them if they participated in legal or illegal protest. *Group Encouragement* was a count of the number of groups to which individuals belonged that encouraged either legal or illegal protest from its members. *Gain Knowledge* was a 1–4 scale measuring the extent to which individuals "would understand politics better" if they participated in either legal or illegal protest. *Stand Up for Beliefs* was a 1–4 scale measuring the extent to which the individual would "feel good because I had stood up for something I believe in" through legal or illegal protest. *Entertainment* was a 1–5 scale measuring the extent to which "being involved in politics is a very enjoyable experience." *Internal Behavioral Norms* was a 1–5 scale measuring the extent to which individuals believe that violating the law and engaging in political violence in the pursuit of certain political goals is morally justifiable; politics should not be left to elected officials; and dissatisfied citizens have a "duty" to do something about it. All selective incentives variables except for internal behavioral norms were coded so that higher values mean a greater perceived benefit from participation; higher values on the norms index indicate a greater psychological cost from taking part in morally unjustifiable protest behaviors.

We first estimate regression equations that correspond to the tests of the collective interest model in previous research, using independent variables measured in 1987 to predict reported participation in the 1987–89 interval (as measured in the second wave in 1989). We then include the selective incentives variables and assess the explanatory power of a full model predicting 1987–89 collective political action. In equation form:

$$P(1987\text{–}89) = \beta_0 + \beta_1 P(1985\text{–}87) + \Sigma\beta_k CI(1987)_k + \Sigma\beta_j SI(1987)_j + \Sigma\beta_i C(1987)_i + U_1, \quad (1)$$

where $P(1987\text{–}89)$ represents the participation scale measured in wave 2, $P(1985\text{–}87)$ represents the participation scale measured in wave 1, $CI(1987)_k$ represents each of the k 1987 collective interest variables, $SI(1987)_j$ represents each of the j 1987 selective incentives, and $C(1987)_I$ represents each of the I 1987 perceived costs of participation.[7]

[7] It should be noted that equation 1 contains no instantaneous effect from the collective interest variables in wave 2 on wave 2 participation. According to the temporal logic developed in the paper thus far, we rule out the possibility that participation that took place in the 1987–89 interval could be caused by any 1989 attitudinal variable.

TABLE 2. Collective Interest and Selective Incentive Models of Protest Behavior, 1987–89

	(1)		(2)	
Variable	Unstandardized	Standardized	Unstandardized	Standardized
1985–87 Behavior	.27*** (.05)	.27	.23*** (.06)	.23
Public Goods Dissatisfaction (Logged)	.53** (.23)	.11	.47* (.24)	.10
Perceived Likelihood of Group Success (Logged)	.15* (.08)	.10	.15* (.08)	.10
Perceived Personal Influence (Logged)	.28** (.11)	.13	.24* (.12)	.11
Belief in Necessity of Group Unity (Logged)	.11 (.11)	.05	.10 (.12)	.04
Trouble with Police	−.04 (.03)	.05	−.04 (.03)	−.08
Could Get Hurt	.00 (.02)	.01	.01 (.02)	.01
Take Too Much Time	−.03* (.02)	−.08	−.04 (.03)	−.05
Group Encouragement	—	—	.03* (.01)	.10
Expectations of Others	—	—	−.01 (.02)	−.03
Financial Gain	—	—	.04 (.04)	.05
Gain Knowledge	—	—	−.01 (.02)	−.03
Stand Up for Beliefs	—	—	.01 (.02)	.00
Entertainment	—	—	.01 (.01)	.03
Internal Behavioral Norms	—	—	−.03 (−.03)	−.07
Adjusted R^2	.20	—	.21	—

*$p < .05$, **$p < .01$, ***$p < .001$ (one-tailed). $N = 377$. Standard errors in parentheses.

We then examine the reciprocal linkages between behavior and the variables in both the collective interest and selective incentives variables by specifying equations predicting the wave 2 values of each incentive in 1989 from its prior value and from prior participation reported in wave 1 between 1985 and 1987. In equation form:

$$SI(1989)_j = \beta_0 + \beta_1 SI(1987)_j + \beta_2 P(1985\text{–}87) + U_j, \quad (2)$$

and

$$CI(1989)_k = \beta_0 + \beta_1 CI(1987)_k + \beta_2 P(1985\text{–}87) + U_k, \quad (3)$$

where $SI(1989)_j$ and $CI(1989)_k$ represent the 1989 (wave 2) values of the j and k selective incentives and collective interest variables, respectively.[8] Equations 2 and 3 are estimated separately for each collective interest and selective incentive, as will be seen in Table 4.

In combination, the two sets of analyses represent a variant of the basic two-wave cross-lagged panel model, which alleviates many of the ambiguities of the temporal ordering and simultaneity biases that have marred previous empirical tests of these relationships. Despite this attention to the problems of causal order, however, equations 1–3 still may have omitted relevant variables, and in later analyses we include education, age, and social class to control for these variables' own causal effects as well as their effects as possible proxies for other factors that may influence both participation and the rational choice variables.

RESULTS

We examine first the collective interest model, which predicts protest from the individual's dissatisfaction with the provision of public goods, perceptions of the likelihood of group success, and perceived importance of personal participation. Model 1 in Table 2 shows the results of an equation predicting reported (logged) protest participation between 1987 and 1989 with the collective interest variables, controlling for reported 1985–87 behavior (logged) and several perceived costs of protest. All independent variables are taken from the 1987 (wave 1) survey, are measured as described in the previous section, and are logged to correspond to the logarithmic regression models conducted in previous research (Finkel et al. 1989, Muller et al. 1991). These log-log equations express in additive form the interaction between preferences for public goods and perceptions of group and individual efficacy that is posited by the collective interest model.

The first model in the table shows that three of the four variables from the collective interest model, measured in 1987, are all statistically significant predictors of future protest behavior. While the strongest influence on 1987–89 behavior is past behavior, these collective interest variables have standardized regression coefficients between .10 and .13, and taken together the model explains about one-fifth of the variance in reported 1987–89 protest behavior. Considering the two-year lag between measurements and the summary nature of the dependent variable, this is strong evidence supporting the hypothesis that the collective interest variables exert influence on future protest behavior, independent of possible reciprocal

[8] It is also possible that wave 2 participation (which measures behavior undertaken in 1987–89) influences wave 2 (1989) incentives. If that is the case, then the results reported in Table 4 below may *underestimate* the effect of participation on the rational choice variables. But given that such a specification would directly contradict virtually all previous cross-sectional research in the field of political participation, we decided on the more conservative formulation, whereby we predict wave 2 incentives only from wave 1 (1985–87) participation.

relationships between these variables and independent of the contaminating effects of attitude rationalization.[9]

Interestingly, the cost variables also influence protest in the expected negative direction, with too much time achieving statistical significance at the .05 level and the effect of trouble with police having a t value of -1.6. This indicates that individuals are balancing the perceived benefits from their participation in protest with perceived opportunity costs and with potential costs related to legal or official sanctions. These results support the findings from the analysis of protest in Peru (Muller et al. 1991) but run counter to the finding of the so-called martyr effect among antinuclear activists in West Germany in the early 1980s (Muller and Opp 1986), where perceived costs were *positively* related to protest potential. It is possible that the latter effect, one that has puzzled scholars since its publication (cf. Klosko 1987, Opp and Ruehl 1988), was produced by individuals adjusting their perceptions of costs upward after having decided to participate in collective protest activities. The negative relationship between perceived costs and subsequent protest here supports a more traditional rational choice interpretation.

The second model from Table 2 represents the full participation model specific to equation 1 above and includes a series of material, social, and psychological selective incentives into the predictive equation: expected financial and occupational gains or losses, conforming to the behavioral norms of others or groups to which the individual belongs, and the "in-process" rewards of gaining knowledge, entertaining oneself through protest, standing up for one's political views, or conforming to one's own internalized norms for or against violent political behavior. In contrast to the results of previous cross-sectional research, we find that the values of these variables in 1987 are relatively weak predictors of subsequent behavior. The selective incentives add little to the explanatory power of the entire equation, with adjusted R^2 reaching .21, and only one variable in the entire set, belonging to groups that encourage protest, is statistically different from zero at the .05 probability level. In comparison, the coefficients for public goods dissatisfaction, perceived likelihood of group success, and perceived personal influence all remain statistically significant, with little change in their substantive magnitude from equation 1.[10]

It is possible, however, that the results in Table 2 underestimate the effects of selective incentives and material costs, as the ten individual variables may exert stronger influence when combined into more reliable summary indices. We therefore conducted a principal components factor analysis of the selective incentives and costs variables and uncovered a two-factor solution. Summary factors were created from the factor scores associated with each variable. One factor includes the variables too much time, trouble with police, and could get hurt, along with a (negatively loading) financial gain, and we label this factor "Material Costs." The other factor includes all other selective incentives in Table 2 except for group encouragement, which loaded on neither factor. We show in Table 3 the results of a model predicting protest behavior with these two factors, group encouragement, a summary multiplicative measure of the collective interest variables, and past behavior. We also introduce the demographic controls of education (coded from 1 for "left school without elementary diploma" to 5 for "completed Abitur or university entry diploma"), age (in years), and self-reported social class (coded from 1 for "lower class" to 5 for "upper class").

TABLE 3. Regression Model Predicting Protest Behavior, 1987–89, with Summary Measures and Demographic Controls

Variable	Unstandardized		Standardized
1985–87 Behavior	.22***	(.05)	.22
Collective Interest 1987	1.23**	(.45)	.16
Material Costs 1987	−.06**	(.02)	.12
Group Encouragement 1987	.02*	(.01)	.09
Selective Incentives 1987	.01	(.02)	.02
Education 1989	.03*	(.01)	.12
Age 1989	−.001	(.001)	−.07
Class 1989	.01	(.02)	.00
Adjusted R^2	.22		

*$p < .05$, **$p < .01$, ***$p < .001$ (one-tailed). $N = 377$. Standard errors in parentheses.

The results strongly confirm the conclusions reached above. The summary factor for selective incentives is irrelevant for protest participation, while the collective interest variable has the strongest standardized effect on 1987–89 behavior of any variable except for past behavior. Group encouragement also remains statistically significant, and the summary factor of material costs exerts a relatively strong and significant negative effect on participation. These effects, in addition, hold after controlling for standard demographic influences on behavior.

The panel analyses show clearly that the factors leading individuals to engage in political protest are those in the collective interest model, along with an independent effect of group mobilization. To some extent, these findings parallel those in previous research, in which the collective interest model as a whole typically outperformed models based on selec-

[9] We tested the hypothesized interaction effect of the variables in the collective interest model in another way by first creating a multiplicative variable comprised of public goods dissatisfaction, likelihood of group success, and perceptions of personal influence (the three significant variables in the model). We then tested whether the individual variables had a significant effect on participation independent of the multiplicative interactive variable. In each case the multiplicative variable was significant, and the individual variable was not.

[10] The results are stable even after taking into account the possibility that measurement error accounts for some of the observed change in participation over time (cf. Finkel 1995). Under the assumption that the reliability of the participation scale is .76 in wave 2 and .78 in wave 1 (see p. 12), we reestimated the model of Table 3 in LISREL and found that the magnitude of the coefficients and their statistical significance remained largely unchanged.

TABLE 4. Regression Models Predicting 1989 Selective Incentives and Collective Interest Variables

Dependent Variable	Lagged Dependent Variable 1987	Participation 1985–87	Education 1989	Age 1989	Class 1989	Adjusted R^2
Selective Incentives						
Group Encouragement	.55***	.10*	.10*	−.03	−.06	.40
Expectations of Others	.13**	.04	.13**	−.17**	.08*	.13
Stand Up for Beliefs	.13**	.11**	.09	−.18***	.10*	.13
Internal Behavioral Norms	.26***	−.06	−.11*	.10*	.02	.14
Entertainment	.09	.18***	.05	−.06	.13*	.06
Gain Knowledge	.23***	.07	−.07	−.13**	.04	.08
Summary Selective Incentives	.20***	.14**	.08	−.19***	.11*	.21
Collective Interest						
Likelihood of Group Success	.16**	.10*	.09*	−.09*	.06	.09
Personal Influence	.29***	.12**	.08	−.11*	.08	.21
Belief in Necessity of Group Unity	.15**	.03	.21**	−.01	.01	.08
Public Goods Dissatisfaction	.43***	.04	−.02	−.05	−.02	.20

Note: Entries are standardized regression coefficients.
*$p < .05$, **$p < .01$, ***$p < .001$ (one-tailed). $N = 377$.

tive incentive variables in explaining protest in several national settings (Muller et al. 1991, Muller and Opp 1986). Yet, in prior cross-sectional studies, several of the most important explanatory variables have been the individual's desire to conform to the behavioral norms of important others, to stand up for one's beliefs, and to adhere to one's own internalized norms against protest behavior. We suggest that the cross-sectional findings may have been produced by the effect of past behavior on the selective incentives, and this possibility is explored with the panel data below.

The top half of Table 4 shows the results of the estimation of equation 2 above, the model predicting each of the selective incentives, as measured in 1989 (wave 2), from its value in 1987 and 1985–87 reported protest participation, with education, age, and social class included as controls. As can be seen, participation has a statistically significant effect on three of these variables: standing up for one's beliefs, the entertainment value of protest, and belonging to groups that encourage protest. Past participation also has a significant influence on the overall selective incentives factor described above. In combination with the results of Table 3, these findings indicate that, except for group encouragement, the relationship between participation and selective incentives is unidirectional: Participation affects these attitudes, but the attitudes have no independent effect on subsequent participation. This is evidence of a rationalization process, whereby individuals take part in protest activities and then claim, for example, that they "would feel good" for standing up for what they believe, *without those attitudes being true causes of future participation.*

Table 4 also shows that protest participation influences the individual's propensity to join groups that encourage these forms of behavior. This finding, along with the results of tables 2 and 3, supports the notion that integration into protest groups is both a cause and effect of collective action. Individuals are mobilized by their membership in groups that encourage protest; at the same time, taking part in protest stimulates individuals to extend their memberships in like-minded organizations. These tables demonstrate clearly a key element in the mobilization process, the mutually reinforcing relationship between organizational activities and participation in collective action.

A similar pattern of reciprocal linkages between participation and attitudes can be seen for several of the collective interest variables in the bottom portion of Table 3. Participation has a significant effect on individual and group efficacy, indicating that prior behavior leads individuals to adjust their perceptions of both group and individual influence upward. These variables were shown in Table 2 to have significant effects on subsequent participation as well, indicating that two of the critical attitudes that predict protest are reinforced by taking part in protest behavior. Protest participation thus sets in motion a process of attitude adjustment and reinforcement of several of the collective interest variables, which then facilitate subsequent behavior. Interestingly, adding the 1987 value of group encouragement to these models (not displayed) shows that prior membership in protest-encouraging groups leads to marginally higher levels of group and individual efficacy in 1989 (t statistics of 1.5 and 1.6, respectively), supporting the notion that protest groups attempt to increase future participation by influencing individuals' perceptions of the likelihood of group success and their own importance of contributing to the collective cause (Klandermans 1992, Snow et al. 1986). The mobilization process in general is thus characterized by a series of mutually reinforcing relationships among prior participation, group memberships, and key variables in the collective interest model.[11]

[11] Taking measurement error into account under the assumption that the reliabilities of past participation and all other variables are .78, LISREL estimates of the models in Table 4 produce similar results. In particular, the standardized effects of past participation on perceptions of group success and personal influence are .11 and .12, respectively, and the effect of past participation on the summary

CONCLUSION

Previous research that has tested rational choice models of political protest with cross-sectional data has been unable to establish that the benefits perceived by individuals are the cause, and not the effect, of participation in collective action. Causal inference in the cross-sectional case has been hampered by the inability to use perceptions of the costs and benefits of participation at a given time to predict behavior that necessarily occurred in the past and by ambiguities associated with analyzing behavioral intentions instead of actual protest participation. We have argued that the analysis of panel data can overcome both of these difficulties. Using panel data collected on a national sample in West Germany between 1987 and 1989, we have shown that variables comprising what we have termed a collective interest model measured in 1987 do predict reported participation in protest activities during the subsequent two-year interval. Individuals participate in collective protest when they are dissatisfied with the current provision of public or collective goods, when they believe that group actions can be successful, and when they believe that their own participation is important for group success. Other variables related to rational choice—corresponding to the selective incentives or private material, social, and psychic payoffs associated with protest—are found to be weaker predictors of future behavior. Put simply, what matters for protest behavior are not private incentives but, rather, collective goals, collective chances for success, and the individual's estimates of his or her own importance for the collective outcome.

The findings have considerable implications for recent disputes surrounding the applicability of rational choice theory in explaining political participation (Green and Shapiro 1994, Whiteley 1995). Much of the controversy involves the legitimacy of including "soft selective incentives," such as social approval, entertainment, or standing up for one's beliefs, into rational choice models. We believe that these factors can be included *provided* that they are specified and measured in advance of the behaviors in question, thus overcoming the criticism that they represent post hoc embellishments to explain unanticipated behavior. But in so doing, the analyses here suggest that the "soft incentives" empirically are less important than previously found in cross-sectional research; moreover, the models in Table 4 show that these variables are largely rationalizations of prior participation, with weak independent explanatory effects.[12] To this extent, the results stand the traditional view of selective incentives on its head: Rather than participation being the "by-product" of private payoffs to the individual, it appears that expectations of social and psychological rewards are the by-products of past participation! Thus, the heated debate about the "rational choice" status of these types of variables in this instance has relatively little empirical relevance.

We disagree strongly, however, with the criticism that the rational choice approach must necessarily exclude individual (mis)perceptions of personal influence, and by extension individual preferences for public goods and perceptions of the likelihood of group success. Theoretically, the collective interest model is consistent with the subjective expected utility framework, as individuals attempt to achieve benefits (such as a change in government policy), subject to the constraints that the benefits are collectively possible to obtain, that their own participation is important for the benefit's provision, and that the material and opportunity costs of participation are not too high. Some, however, assert that an "objectively" false belief, such as a nonnegligible perception of personal influence on the provision of a public good, has no place in a rational choice model (Klosko 1987, Green and Shapiro 1994). Yet, there is much philosophical disagreement on this point; scholars such as Simon (1985) and Benn and Mortimore (1976) assert that acting efficiently on the basis of even imperfect information may be considered "procedurally" or "practically" rational. Elster (1985, 9) similarly claims that an action is rational if, "given the beliefs of the agent, the action was the best way for him to realize his plans and desires." And given the fact that perceptions of efficacy correlate strongly with indicators of political resources, such as education, income, and cognitive skills (Barnes and Kaase 1979, Verba et al. 1995), it is by no means clear that the individual's belief in his or her own political influence is "objectively" false (see also Lohmann 1994). Following these notions, the collective interest model falls squarely within the family of rational choice explanations.

Empirically, the results attest unequivocally to the importance of personal influence and the other variables in the collective interest model in motivating individual participation. Equally important, we have shown with panel data that perceptions of individual and group efficacy are not merely rationalizations of an individual's prior willingness to participate or measurement artifacts of the "ideology of activism" (Green and Shapiro 1994, 85–6). Instead, these perceptions are significantly related to future behavior, and indeed they serve as the key links between individuals' political discontent and their decision to participate in or abstain from collective political action. This is the essence of the collective interest model, and this study provides its strongest confirmation to date.

The results reported here also shed light on more general issues related to political mobilization. Much recent literature (e.g., Lichbach 1995, Snow et al. 1986), for example, describes how dissident groups attempt to mobilize participation by altering perceptions of both the "probability of winning" and the individual's estimate of the "probability of making a difference," precisely the terms shown to be relevant here in motivating individual behavior. Moreover, we found considerable evidence that individuals who en-

selective incentives index is .16. The effect of participation on group encouragement is, however, weakened to a standardized value of .07.

[12] It is possible, however, that the soft incentives influence participation over a shorter time lag than was measured in this study, and future research should address this possibility with panel data collected over shorter intervals.

gage in protest begin to perceive that their own actions and those of the group may be more successful, and indeed many individuals who protest at one point go on to join organizations that encourage future protest behavior. These processes support the view in social movement research which claims that "the cognitions relevant to collective action—be they preferences, values, interests or utilities, costs or benefits . . . are . . . dynamic and evolving entities" (Snow and Oliver 1993, 33), with individuals who protest at one time coming to accept more readily the "motivational frames" that make future behavior more likely. The reciprocal causal effects found here suggest that the act of engaging in collective protest changes precisely those perceptions and cognitions that influence subsequent participation.

Finally, the fact that protest participation influences individual perceptions and cognitions serves as a caution against attempts to test attitudinal models of political behavior with cross-sectional data. We agree that "one may be rightly skeptical of surveys that ask people to recount the reasons behind [their] actions, . . . particularly when the survey takes place long after the fact" (Green and Shapiro 1994, 85; Leighley 1995), and it was precisely this skepticism that motivated our use of a panel design in this study. It must be recognized that attitudes measured at one point can best be used to predict subsequent political participation, as a substantial portion of the cross-sectional covariation between attitudes and behaviors may be due to the effect of past behavior on current attitudes. This problem, moreover, is not limited to rational choice models of protest; it potentially will affect other models, such as those based on theories of relative deprivation or postmaterialism, that specify political and social attitudes as causes of behavior. Panel data are ideal for the estimation of such models, and such data provide the additional benefit of allowing for more complex analyses of the dynamic processes by which individuals come to participate in the pursuit of collective ends.

APPENDIX. QUESTION WORDINGS FROM THE 1987–89 GERMAN PANEL STUDY

Collective Interest Variables

Public Goods Dissatisfaction

Concern about Problems. Would you tell me now, how concerned you are about the following problems?

Response values: 0 for "not at all concerned" to 4 for "extremely concerned."

1. The extent of unemployment
2. The differences between rich and poor
3. Environmental pollution
4. Nuclear power stations
5. Deployment of missiles

Performance of Government. Please tell me for each issue how you would rate the overall performance of the government. If you think that any of these issues are not a proper concern for the government, please tell me.

Response values: 0 for not a proper concern, 1 for "excellent" to 5 for "very poor."

Likelihood of Group Success

Past Group Success. On this sheet we have indicated what groups have done to try to attain their political goals. Please tell me whether you think they have by and large hurt or helped the cause of the group, regardless of whether you approve of these actions or not.

Response values: 1 for "hurt a lot" to 5 for "helped a lot."

1. When groups have collected signatures
2. When groups have undertaken legal demonstrations
3. When groups have seized buildings
4. When groups have blocked streets or staged sit-ins
5. When groups have participated in confrontations with police or other government authorities

Willingness of Others. Now, thinking more generally about people in your area who have political views similar to your own, would you tell us how many people would be willing to get involved in the following activities?

Response values: 1 for "none" to 5 for "almost all."

1. Seizing buildings
2. Blocking streets or staging sit-ins
3. Collecting signatures for a petition
4. Working with a citizen's action committee

Perceived Personal Influence

Influence through Specific Activities. Here is a list of political activities. Please indicate the extent to which you *personally* could have an influence on politics if you participated in each one.

Response values: 1 for "I would have no influence" to 5 for "I would have great influence."

1. Collected signatures for a petition
2. Worked with a citizen's action committee
3. Participated in an organized effort to solve a neighborhood problem
4. Seized buildings
5. Blocked streets or participated in sit-ins

General Political Influence. I would like to ask you some more questions on general issues. To what extent do you agree or disagree with the following statements?

1. There is no point in getting involved in politics because I would have no influence anyway. Response values: 1 for "strongly agree" to 5 for "strongly disagree."
2. If I were more involved in politics, I would have more influence on what happens. Response values: 1 for "strongly disagree" to 5 for "strongly agree."

Necessity of Group Unity

Now I would like to ask some general questions about political activities. To what extent do you agree or disagree with the following statements?

Response values: 1 for "strongly disagree" to 5 for "strongly agree."

1. Every individual member is necessary for the success of a political group, no matter how large it is.
2. For groups to have a reasonable chance of success by means of political action, everyone must contribute a small part.

Costs of Participation

Trouble with Police, Could Get Hurt, Take Too Much Time

If you were to participate in political action that is *permitted*, e.g., working in an election campaign, collecting signatures for a petition, or taking part in a permitted demonstration, how likely is it that the following will happen?

Response values: 1 for "very unlikely" to 4 for "very likely."

1. I would get into trouble with the police or the courts.
2. I could get hurt.
3. It would take too much of my time.

If you were to participate in political action that *breaks the law*, e.g., blocking streets or damaging property, how likely is it that the following will happen?

Response values: 1 for "very unlikely" to 4 for "very likely."

1. I would get into trouble with the police or the courts.
2. I could get hurt.
3. It would take too much of my time.

Selective Incentives

Financial Gain, Gain Knowledge, Stand Up for Beliefs

If you were to participate in political action that is *permitted*, e.g., working in an election campaign, collecting signatures for a petition, or taking part in a permitted demonstration, how likely is it that the following will happen?

Response values: 1 for "very unlikely" to 4 for "very likely."

1. I would gain financially.
2. I would understand politics better.
3. I would feel good because I had stood up for something I believed in.

If you were to participate in political action that *breaks the law*, e.g., blocking streets or damaging property, how likely is it that the following will happen?

Response values: 1 for "very unlikely" to 4 for "very likely."

1. I would gain financially.
2. I would understand politics better.
3. I would feel good because I had stood up for something I believed in.

Expectations of Others

Think about those people whose opinion is especially important to you, for example, your spouse, friends, colleagues, or other people you know. If you were to participate in political action that *breaks the law*, e.g., blocking streets, sit-ins, or damaging property, what would most of those people think of you?

If you were to participate in political action that is *permitted*, what would most of those people mentioned above think of you?

Response values: 1 for "very negatively" to 5 for *very positively."*

Group Encouragement

Do any of those groups (of which respondent indicated s/he was a member encourage or discourage you to take part in activities like signing petitions or taking part in demonstrations, or don't they care one way or another?

Do any of those groups encourage or discourage you to take part in actions like sit-ins, blocking streets, or don't they care one way or another?

Response values: 1 for "I'm encouraged," 2 for "I'm discouraged," 3 for "group does not care."

Entertainment

Now I would like to ask some general questions about political activities. To what extent do you agree or disagree with the following statements?

Response values: 1 for "strongly disagree" to 5 for "strongly agree."

Being involved in politics is a very enjoyable experience.

Internal Behavioral Norms

I would like to ask you some more questions on general issues. To what extent do you agree or disagree with the following statements?

Response values: 1 for "strongly agree" to 5 for "strongly disagree."

1. If a citizen is dissatisfied with the policy of the government, he has a duty to do something about it.
2. Politics should be left to our elected representatives. Response values: 1 for "strongly disagree" to 5 for "strongly agree."
3. Violence against *property* in order to achieve certain political goals is morally justifiable.
4. Violence against *persons* in order to achieve certain political goals is morally justifiable.
5. If citizens struggle for important political causes, violating the law may be necessary.

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Democratizing for Peace

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The argument that democratization can bring about war is a powerful critique suggesting limits to the linkage between democracy and peace. This research examines this claim. Our findings demonstrate that democratizing polities are substantially less war prone than previously argued. By focusing on the characteristics of the transition process, we show that as contemporary polities become more democratic they reduce their overall chances of being involved in war by approximately half. We also find that rocky or especially rapid transitions or reversals are associated with a countervailing effect; namely, they increase the risk of being involved in warfare. Both in the long term and while societies undergo democratic change, the risks of war are reduced by democratization and exacerbated by reversals in the democratization process. To reach these conclusions, we developed and applied a logit model linking authority characteristics and war involvement using Polity III and Correlates of War databases.

Do polities become more peaceful as they democratize? Alternatively, is political change toward greater political democracy associated with increased likelihood of war? Research following Babst's (1964) observation of an apparent absence of a war between democracies has produced considerable empirical evidence for the liberal proposition (Kant [1795] 1991) that democracies rarely if ever fight one another (Bremer 1992, 1993; Maoz and Abdolali 1989; Maoz and Russett 1993; Raknerud and Hegre 1997; Russett and Maoz 1992). Despite substantial variation and healthy debate over the specific causal mechanism linking *joint* democracy and peace, there appears to be an emerging, if heady, consensus at least on the empirical existence of a "democratic peace" or an absence of war among democracies. In April 1995 the *Economist* (p. 18) summarized this by noting that "it takes two *not* to tango."

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An earlier version was presented at the annual conference of the International Studies Association, Toronto, Canada, March 18–23, 1997. This research is supported by a grant from the National Science Foundation, SBR 9511577. We are grateful for this and for institutional support from the universities of Colorado, Maryland, and Washington. Kristian S. Gleditsch also received the gracious support of the American Scandinavian Foundation. We are grateful to our collaborators, John O'Loughlin, Corey L. Lofdahl, Jordin S Cohen, David Reilly, and Michael E. Shin, for their support and help in this project. Jonathan Wilkenfeld, David Lalman, Harvey Starr, Christian Davenport, Jeff Kopstein, David John Longenecker, Richard Tucker, and participants in seminars at the University of California at Davis, the University of Colorado, and the University of Washington gave us many helpful comments on an earlier version, as did anonymous *APSR* reviewers. Randolph Siverson pointed us in the right direction when we were at the crossroads; we thank him for his wisdom.

The research was undertaken in the context of the Globalization and Democratization Program, also supported by the National Science Foundation (EHR 95-53453) and the University of Colorado. Faculty and students in the program have created a splendid interdisciplinary environment that supported our research, and we thank them, especially our faculty colleagues, Richard Jessor, Edward S. Greenberg, and Lynn Staeheli.

Research on the democratic peace has almost exclusively focused on the behavior of pairs of countries. The dyadic character of the unit of analysis applies not only to the issue of conflict (i.e., are the societies involved in a war with each other) but also to the major independent forces often employed to explain the conflict. The dyad is typically classified as being jointly democratic, mixed, or jointly autocratic. Some studies have also used measures of the mean or average level of political democracy in a dyad.[1] When the analysis has rested at this constructed dyadic level, the results have been consistent and have cumulated into what has become known as the democratic peace proposition.

When the unit of analysis is focused on the monadic level of individual states, however, most studies have failed to find strong or significant differences between the war participation of democracies and nondemocracies. While this conclusion has been questioned in some writings (Hewitt and Wilkenfeld 1996; Ray 1995a, 1995b; Rummel 1983, 1985, 1995), the conventional wisdom is that while democracies do not fight one another, they appear to be at least as war prone as other political systems (see, e.g., Dixon 1993, 1994; Morgan and Campbell 1991; Morgan and Schwebach 1992; Russett and Maoz 1993; Singer and Small 1976; but see also Gates, Knutsen, and Moses 1996). Why there is such a divergence between the national and bilateral versions of the idea that democracy leads to a more peaceful foreign policy continues to be puzzling. One early study (Chan 1984) suggested that ethnocentrism is at part to blame for the inference that democracies are by nature peaceful. A recent critique (Spiro 1994) suggests that the first finding is simply a result of having a very rare set of occurrences to study: Unusual (i.e., rare) events involving rare structures (democracies) are especially rare. The issue of whether it is useful to aggregate conflict initiators and targets into a

[1] The use of dyads as the unit of analysis does not require summing the parties' behavior. One alternative is to employ so-called directed dyads, for which the temporal sequence of the specific interaction between the parties is made explicit, e.g., indicating one party as initiator or recipient (see Rousseau et al. 1996, Rummel 1985).

single dyad at war is also viewed as problematic. The probability of participating in a war may also depend upon the number of politically relevant dyads in which a country is involved (Lemke 1995, Maoz and Russett 1993) as well as the regime characteristics of the other countries in these dyads. Relevant dyads are those in which there exists an actual opportunity for war, typically gauged by geographical proximity and/or the density of political interactions.

The conundrum of why democracies apparently do not wage war against one another but are not in general more pacific has failed to spawn a great deal of research. Thompson and Tucker (1997) note that in the past decade there have been more than 100 empirical papers targeted at the peaceful joint democracy proposition. A much smaller number of scholars have looked into why democracies are not considered sui generis to be more pacific.[2] Small and Singer (1976) found no difference between democracies and autocracies in the baseline probability of war involvement. Chan's (1984) research also demonstrated this empirical regularity. Both of the two major explanations of the democratic peace proposition, the normative and institutional, would seem to have broader implications for the putative peaceful nature of the modern polity. This conundrum is so important not only because it calls into question the theoretical underpinnings of the democratic peace hypothesis but also because it may undercut the idealist policy prescription shared by diverse political leaders; namely, widespread democratization will lead to widespread peace.

In this light, recent evidence has emerged that seeks—in part—to reconcile the differences between the bilateral and state-centric versions of Kant's longstanding ([1795] 1991) thesis. Mansfield and Snyder (1995a, 1995b, 1997) suggest that while democracies may eventually become more peaceful than autocratic societies, the transition phase of newly democratic states is especially dangerous. Nascent democracies are thought to be quite likely to become involved in foreign conflict. This is a consequence of the fragile position of the new elite, which is forced to appeal to nationalism in attempts to solidify its tenuous position. Accordingly, there is an increased propensity for war involvement. This may help explain the divergent empirical findings. If democracies are truly more peaceful, except during the very early stages of democratic institutionalization, then the nonpacific nature of democracies may be the result of war proneness in young democracies balanced by the pacific behavior patterns of mature democracies.

The notion that democratizing societies are war prone and conflict laden does call into question, however, whether democracies do in general have norms and institutions that prohibit conflict with other (similar) societies. This, in turn, raises the question of whether, in an era of widespread proliferation of weapons of mass destruction, democratization will always be easily adjudged a good thing. On the one hand, the absence of a strong monadic link between democracy and peace would not necessarily disprove the dyadic proposition that democracies rarely if ever fight one another. On the other hand, its presence would not put to rest all questions about the validity of the democratic peace. Yet, information about the form and nature of any monadic link would get us closer to answers to these questions. These ambiguities between different levels of analysis suggest that research on the democratic peace proposition may have left unexplored some possible implications of domestic political structure for conflict behavior.

In this article we examine the monadic aspects of the democratic peace proposition by revisiting the question of whether democratizing states are less war prone than autocratic states. Our focus is upon the process of how exactly states become more or less democratically governed. We probe whether this may help explain why democracies apparently are not very likely to wage war with other democracies. In focusing on transitions, we follow the suggestion of Mansfield and Snyder (1995a, 1995b) that immature democracies undergoing rapid institutional change may be more prone to war involvement. We do not systematically examine simultaneous factors that may explain the differing relationships between political structure and conflict behavior found on the various levels of analysis. We ignore the characteristics of other societies. We also do not address the issue of the actual timing of changes in political or institutional arrangements and structures, an unexplored aspect in the extant data on regime characteristics. Instead, we focus on another set of neglected features of domestic political structure with possible implications for conflict behavior: the effects of political transitions and regime change. In short, we reexamine whether democratizing states are more war prone. We also examine whether reversals of the democratization process tend to threaten peace.

THE DANGER OF DEMOCRATIZATION

Many studies of the democratic peace examine the effect of current domestic political structures on war participation.[3] Explanations of the democratic peace focus either on the existence of norms that facilitate the peaceful resolution of conflicts (Dixon 1993, 1994; Hewitt and Wilkenfeld 1996; Raymond 1994) or the structural features of democracies that make decision makers more cautious in the use of force (Bueno de Mesquita and Siverson 1995, Morgan and Campbell 1991, Morgan and Schwebach 1992). These include both formal institutional constraints on the executive decision to declare war and various electoral mechanisms that enhance the dependence of executive leaders on popular approval.

Contested elections increase the executive leader's sensitivity to political risk since they may call into question the tenure of the ruling authorities. The fear

[2] Rousseau et al. (1996) have examined dyadic versus monadic aspects of the democratic peace proposition as it relates to crisis behavior.

[3] Exceptions are Rousseau et al. 1996, Thompson and Tucker 1997, and Mansfield and Snyder 1995a, 1995b.

that failure may lead to loss of political power in subsequent elections will ensure some restraint in the use of force. In the same way, legislative approval is an important source of constraint on political leaders. Bueno de Mesquita and Siverson (1997) and Bueno de Mesquita et al. (1997) construct a simple theory based on coalition theory (i.e., Riker 1962) that illustrates these points. They show that democrats are punished more quickly than autocrats for losing wars. Consequently, in the presence of uncertainty about the outcome of a conflict, they are more likely to press for compromise.

The distinction between these competing views is commonly portrayed as a difference between structural and normative explanations of the democratic peace. As illustrated in Bueno de Mesquita et al. (1997), however, the distinctions may be subsumed under a broader theory of what motivates democratic as well as autocratic decision makers.

Many who accept the notion that democracies do not fight one another still consider democratization and political change something of a double-edged sword. While stable, well-established democracies may not fight one another, a possibly rocky process of democratization or transition toward a fragile democracy need not necessarily imply that countries become immediately more peaceful. Indeed, as shown by Lichbach (1984), a smooth transition from low to high levels of democratic governance is the exception, not the rule. Classic sequences from monarchy, through concurrent democratic and autocratic rule, to an overwhelmingly democratic political structure are considered to be "historical rarities" (Lichbach 1984, 77). A full 30% of the historical European cases seem to oscillate between democratic and nondemocratic forms of governance, according to Lichbach's study.

The degree of democratic commitment or time since democratization may be important for explanations emphasizing the role of norms as well as institutions. It is quite plausible that some time may have to pass before democratic norms or informal institutions become sufficiently well established to have the effect of inhibiting conflicts. Some have suggested employing a turnover or two consecutive elections as classification criteria for democracy (Huntington 1991; Ray 1995a, 1995b). This perspective calls into question whether one should expect to find a decreasing likelihood of war attendant to democratic transitions.

Yet, much stronger and more pessimistic claims have been made regarding the effect of democratization on the likelihood of conflict and war. Political instability and change in general are often considered to be associated with increased likelihood of conflict and with possible subsequent escalation and war involvement. Rapid democratization may bring about weak regimes unable to establish effective control and political order. While these new regimes may be less repressive and permit greater political freedom than their precursors, they also are subject to instability and attempts by challengers to seize power. Political instability and disorder may even encourage attacks from other countries. In fact, stable autocratic regimes may be less prone to conflict and escalation to war than transition regimes. In the same way, political leaders may be more dependent on popular support in young democracies. Absent fully established institutions and civic traditions, these are often considered subject to populist pressures emphasizing the short-term maximization of political support. Democratization often comes bundled with nationalism. In some cases certain groups, typically ethnic minorities, become targeted as second-class citizens and made scapegoats for existing problems. According to some observers, the transition in Eastern Europe has left the population "free to hate." In such circumstances, war often enjoys widespread support among the population. The invasion of the Malvinas/Falkland Islands by Great Britain initially boosted the popularity of the fledgling and increasingly unpopular Argentinean military regime. Populist politicians in struggling democracies may employ external aggression as a way of enhancing political support. The republics of former Yugoslavia provide another example of increased political freedom and external aggression seemingly going hand in hand.[4]

The above discussion suggests two competing hypotheses about the effect of democratization on the likelihood of war. Fortunately, these are nonnested, and evidence can be collected on each of them at the same time. Yet, our belief is that the dangers of democratization are less likely to be borne out in the analysis. Why? Primarily, we think there is a logical, if not necessarily empirical, inconsistency in accounting for the evidence on the democratic peace proposition while maintaining that democratizing states are war prone. If transitions to democracy are dangerous, then newly institutionalizing states will rely *more* on warfare in their foreign policy precisely because decision makers will rely on international violence during an important evolutionary period. That reliance will serve their internal political purposes, in terms of both retention of power and the initial shaping of institutions and norms. If warfare is evolutionarily dominant and successful in states undergoing democratization, it then seems likely that institutions and norms will emerge in a way to reflect this. Evidence of a general (nondyadic) trend of pacific relations in democracies would bolster the view that democracies do not wage war with one another. If democracies—even nascent ones—are more belligerent, however, then this raises considerable doubt about the logical status of a democratic peace. Perhaps that explains the great interest in the recent work of Mansfield and Snyder (1995a, 1995b) by scholars of the democratic peace.

Thompson and Tucker (1997) detail a number of potential weaknesses in the Mansfield and Snyder (1995a, 1995b) research design, and in one replication they conclude that "regime change and war involve-

[4] The Yugoslav example is complex, since the major conflicts broke out before the breakup of a former state. It is moot whether the Croatian-Serbian conflict qualifies as an interstate war. Furthermore, few consider the self-proclaimed Serb Republic of Bosnia to constitute an existing state. A discussion of Serbia can be found in Gagnon (1994/5).

ment are independent" (p. 450). Similarly, Enterline (1996, 197) found that democratization has a "negative impact on the likelihood of a state being on the initiating side of a dispute." These studies use slightly different case selection, variable definition, and statistical methods in arriving at differing conclusions. The first efforts in this area have not presented the accumulated, supporting replications that allow sweeping generalizations. It is clear that more work is needed and that the current findings seem especially fragile.

We are also persuaded that both norms and institutions may be said to produce greater caution in democrats than in autocrats. Because of swift and almost certain penalties for foreign policy failures in democracies, democratic leaders are likely to be cautious in the face of uncertainty. In representative forms of democracy, since legislatures must support foreign policy initiatives, there is an additional pressure for cautious, slow decision making that avoids conflict when faced with uncertainty. Empirical evidence is quite clear that even victories in war rarely enhance a leader's prospect of prolonging tenure (Bueno de Mesquita, Siverson, and Woller 1992). The incentives for war involvement are thus generally lower in democratic than nondemocratic countries. What is unclear is whether these pressures are muddled in the process of democratic (or autocratic) change.

Our research seeks to gain more information about this question by focusing on three aspects of regime change: direction, intensity, and nature. Is the direction of change toward or away from a greater level of democratic institutionalization? Is the modification in the structure of governance small or very large? Change from a monarchy to direct presidential elections by universal adult suffrage is substantial, whereas the shift from suppressed to restricted political participation is less so. Finally, is the change smooth or variegated? Since we know from Lichbach's (1984) study that smooth, linear change from autocratic to democratic forms of government is historically rare, it is important to find out whether some oscillation occurs or frequent reversals are evident. If the dangerous democratization argument is correct, then it is especially in turbulent situations that one would expect to find war-prone democracies.

We formulate this as a probability problem: Are states in transition, especially democracies, more likely to be involved in interstate wars? We choose to model this problem as logistic probability equation. We choose this form because it allows us to make reasonable statements about the probabilities of war behavior associated with certain trajectories of democratic change. The resulting direct and accessible model can be applied to a dependent variable that is binary, such as war involvement. Basically, we want to know the probability of a polity being involved in an interstate war at a given time based on its authority characteristics and how these have changed over the previous ten years. This is expressed as a logistic equation:

TABLE 1. Authority Characteristics and Democracy and Autocracy Scale Values in the Polity III Database

Authority Dimensions	Democracy "Points"	Autocracy "Points"
Competitiveness of political participation		
Competitive	3	0
Transitional	2	0
Factional	1	0
Restricted	0	1
Suppressed	0	2
Not applicable	0	0
Regulation of political participation		
Regulated	0	0
Factional or transitional	0	0
Factional/restricted	0	1
Restricted	0	2
Unregulated	0	0
Competitiveness of executive recruitment		
Election	2	0
Transitional	1	0
Selection	0	2
Openness of executive recruitment		
Election	1	0
Dual: Hereditary and election	1	0
Dual: Hereditary and designation	0	1
Closed	0	1
Constraints on chief executive		
Parity of subordination	4	0
Between parity or subordination and substantial limitations	3	0
Substantial limitations	2	0
Between substantial limitations and slight to moderate limitations	1	0
Slight to moderate limitations	0	1
Between slight to moderate limitations and unlimited Power	0	2
Unlimited power	0	3
Total scores	Σ	Σ

Note: Democracy and autocracy scores are simply the summation (Σ) of points from all of the authority dimensions. Following Jaggers and Gurr (1996) no democracy points are given for the absence of regulation of political participation which is taken to represent anarchy.

$$P_i = E(W_i = 1|A_i + A_i^d + A_i^m + A_i^v),$$

where W_i denotes conflict involvement, A_i represents the authority characteristics (i.e., $A_i \in$ {Democracy, Democracy minus Autocracy, Executive Constraints}, and the superscripts $\{d, m, v\}$ refer, respectively, to the direction (the absolute value of the change over the ten years), the magnitude (the actual change over the ten years), and the variance (over the ten years) of change in A_i. The authority characteristics are defined in Table 1. The probability of war for a given observation has the following logistic form[5]:

[5] The log-odds form (known as the logit model) is linear in variables

TABLE 2. Definitions of Independent Variables

Variable Label	Definition	Description
Current value (in nation i at time t)	$X_{i,t}$	The current value of the regime characteristics
Direction of change (in nation i between time t and $t-10$)	$X_{i,t} < X_{i,t-10} \Rightarrow -1$ $X_{i,t} = X_{i,t-10} \Rightarrow 0$ $X_{i,t} > X_{i,t-10} \Rightarrow +1$	Positive, if the change is toward democratic regime characteristics; negative, if the change is toward autocratic regime characteristics; zero, if there is no change
Change (in nation i between time t and $t-10$)	$X_{i,t} - X_{i,t-10}$	The signed, magnitude of change in regime characteristics, positive for democratic change
Variance of change (in nation i between time t and $t-10$)	$\frac{1}{10}\sum_{t=1}^{10}\left(X_{i,t} - \frac{1}{10}\sum_{t=1}^{10} X_{i,t}\right)^2$	Large values indicate rapid change and/or changes that go back and forth between democratic and autocratic characteristics

$$P_i(War = 1) = \frac{1}{1 + \exp^{-(\beta_0 + \beta_1 A_i + \beta_2 A_i^d + \beta_3 A_i^m + \beta_4 A_i^v)}}. \quad (1)$$

This simple model is quite different in some respects from most other efforts. First, our choice of independent variables focuses entirely on the transition process. Many other studies include only changes exceeding some specific magnitude or use dichotomous categories (i.e., change versus no change) over some period. These categorizations of political change risk being arbitrary. The mere existence of change is not by itself an adequate measure of democratization. Unlike those who conceive of democracy as an all-or-nothing affair, we think of democratization and political change as movements (though not necessarily smooth) along a continuum and examine the amount, direction, and variance of change along that continuum.

We believe that greater change toward democracy will be associated with a decreasing likelihood of war, while minor steps toward political democracy and cosmetic changes will exercise less effect on the avoidance of war. Political change or democratization can take many different forms, however, and need not proceed in a unidirectional or linear fashion. Our position contrasts with that of much of the literature, which claims that large or rapid change toward democracy often tends to be associated with greater instability and higher risk of reversal (e.g., Huntington 1984, 1991). The implication is that gradual changes involve a smaller likelihood of war than does rapid democratization. Furthermore, we posit that greater variability of political change within a country's political system will be associated with a higher likelihood of war. Accordingly, we focus on the moments of the distribution of change in authority characteristics, including the direction, magnitude, and variance, along with the actual current values.

and parameters, but it is even more difficult to interpret than estimated probabilities:

$$\ln\left(\frac{P_i}{1-P_i}\right) = \beta_0 + \beta_1 A_i + \beta_2 A_i^d + \beta_3 A_i^m + \beta_4 A_i^v.$$

Such a specification is novel in the literature, but it allows us to glean more details about the democratization process and how, if at all, it relates to the probability of being involved in a war. Unstable democratic transitions should exhibit a high degree of variance in their democratization scores. Countries moving quickly should exhibit a large value for the magnitude of change, but societies slow to democratize should exhibit a small magnitude of change, with low variance, over time. The next section describes the data, methods, and results from an empirical examination of the issue of whether democratization increases the risk of war.

EMPIRICAL EVIDENCE

Data

For assessing authority characteristics, we employ the 5.1996 version of the Polity III database (Jaggers and Gurr 1996). A more complete description of these data may be found in Gleditsch and Ward (1997), Gurr (1974), Gurr, Jaggers, and Moore (1989, 1990), and Jaggers and Gurr (1995). The data are more current than those available previously, with information on polities through 1994. In addition, a number of important revisions have been undertaken in the 5.1996 version, which are detailed in the web site (http://www.colorado.edu/IBS/GAD/spacetime/data/Polity.html). Most important are the corrected democracy scores of nineteenth-century monarchies and the inclusion of transition codes. Table 1 gave in tabular form the coding rules for assigning democracy and autocracy scale values in the Polity III framework. Table 2 provides the definition for each of the independent variables constructed from these values and employed in equation 1.

From these data and definitions we are able to calculate democracy scale values $\{D\}$; democracy scale values minus autocracy scale values $\{D - A\}$; the extent of constraints on the executive decision maker

{C}; and the direction—coded positively for movements toward democracy—and magnitude of change as well as the variance over the previous ten years in the above three variables, represented by the superscripts d, m, and v, respectively.[6]

Take for example the case of Argentina, which suffered a military coup d'état in 1966. As a result, its competitiveness of political participation moved from transitional to suppressed, resulting in a reduction of two democracy points and an increase of two autocracy points, the regulation of participation does not change, the competitiveness of executive recruitment changed from election to selection (with an attendant reduction of two democracy points and a gain of two autocracy points), the openness of executive recruitment remained the same, and the constraints on the chief executive were slight before the coup and unlimited thereafter (the democracy score does not change as a result of this, but Argentina gained two autocracy points). Taken together this set of changes resulted in the democracy score moving from 3 to 0, while the autocracy score rose from 4 to 9, and the democracy minus autocracy score moved from −1 to −9. During 1966, the direction of changes were toward autocracy (i.e., had a −1 value), the actual magnitude of change in democracy minus autocracy was −8, and the variance of change over the past ten years was calculated to be 6.4, a rather low value.

In 1983, Argentina experienced a return to more democratic forms of governance: When Raúl Alfonsin replaced the military junta, the democracy minus autocracy score moved from −8 in 1982 to +8 in 1983. This reflected change in the competitiveness of political participation, which moved from restricted to factional (leading to an increase of one democracy point and a decrease of one autocracy point), the regulation of political participation changed from restricted to factional or transitional (leading to a decrease in the autocracy score by two points), the openness of executive recruitment remained unchanged, the competitiveness of executive recruitment changed from selection to election (with a resulting increase in the overall democracy score by two points and a reduction of two for the autocracy score), and the extent of constraints on the chief executive went from a previous situation of unlimited power to parity or subordination (thereby yielding an increase of four democracy points, and reducing the autocracy score by three points). In this instance, the direction of change is positive (i.e., toward greater democracy) and the magnitude of change is large (17), while the variance of change over the prior highly unstable decade was calculated to be 55.66.

Two available Correlates of War (COW) databases address a similar spatial and temporal frame: the International War (COW-IW) (Singer and Small 1994, also described in Small and Singer 1982) and the Militarized Interstate Dispute (COW-MID version 2.0) (Gochman and Maoz 1984; Jones, Bremer, and Singer 1996). Yet, there are substantial differences between the two. COW-IW encompasses wars among member states in the international system as well as among those states and extrasystemic organizations or movements. Any conflict with more than 1,000 battle deaths is classified as a war. The extrasystemic category largely consists of colonial and/or imperial wars. COW-MID is more inclusive, focusing on "cases in which the threat, display or use of military force short of war by one member state is explicitly directed towards the government, official representatives, official forces, property or territory of another state" (Jones, Bremer, and Singer 1996, 168). This excludes extrasystemic wars. Mansfield and Snyder (1995a, 1995b, 1997) analyzed the democratization and war hypothesis by including extrasystemic wars, based on the rationale that nationalist aggression and diversionary use of force may lead to involvement in imperial and colonial wars outside the state system. Since wars are relatively rare, many analysts have examined the relationships between regime characteristics and conflicts that are well below the threshold of 1,000 battle deaths. The issue of whether available theories would pertain to weaker forms of conflict is disputed, however (Chan 1997, Enterline 1996, Gochman 1996).

The COW-MID database is considered by some to be more current and reliable than COW-IW. The theoretical criteria for inclusion are the same, and except for some minor revisions, analysts should expect the two databases to have nearly identical entries (except for the exclusion of extrasystemic events). No codebook is yet available for COW-MID, but a recent article (Jones, Bremer, and Singer 1996, 171) quotes the Small and Singer (1982) criterion of 1,000 battle deaths as a threshold for interstate war. We have not found the similarities between the two databases to be overwhelming. There are many important differences. Some nation-war-years found in COW-IW are not found in COW-MID (e.g., France in 1942–43). Some nation-war-years in COW-IW are classified as "display of force" in COW-MID (among others, China and Vietnam in 1985, Italy in 1912, and Kuwait in 1991). As many as 185 (nation-) war-years in COW-MID are not found in COW-IW. Small and Singer (1982, 55) make certain restrictions in order for individual country involvement to qualify as being "at war." Specifically, a member state either must suffer a minimum of 100 fatalities or have a minimum of 1,000 armed personnel engaged in active combat. Jones, Bremer, and Singer (1996) do not mention this issue, but COW-MID includes a separate coding of the specific level of hostility reached in a given country's dispute involvement. Excluding cases of participation at a level below interstate war reduces the discrepancies to 92 war-years. The major share of these seem to stem from different starting years. COW-MID typically assigns a more comprehensive coding; for example, it has the duration of the Mexican-American war as 1843–48,

[6] Following Mansfield and Snyder (1995a, 1995b), we use a ten-year period of change in democratization to explore its effect on the probability of conflict. We also estimated the model using windows of thirty and five years (the latter to follow Thompson and Tucker's [1997] use of a five-year window). These provide essentially the same information as the ten-year windows.

TABLE 3. Two Sets of Empirical Estimates of Logit Models of the Probability of War Based on Authority Characteristics

	Democracy Scale		Democracy minus Autocracy	
Independent Variables	Estimated Coefficient	Standard Error	Estimated Coefficient	Standard Error
Intercept	−2.528	.049	−2.510	.039
Current value	.012	.010	−.004	.006
Direction of change	.277	.119	.190	.091
Change	−.100	.029	−.030	.013
Variance of change	.016	.011	.008	.003
Likelihood ratio χ^2	14.58	$p = .006$	11.81	$p = .018$
Number of cases	10,681		10,681	

compared to 1846–48 for COW-IW.[7] This information could be mined to help reduce the discrepancies, but there are 162 conflict-years in COW-MID without country-specific involvement values.

We use COW-IW here because it is more conservative in coding war involvement—our main dependent variable—and because it includes extrasystemic wars, which are especially pertinent to the hypothesis of the dangers of democratization. Thus, we have used the most recent version of the COW database to ascertain whether each polity was involved in an interstate or extrasystemic war during each year from 1815 to 1992. We call this variable *war involvement*, denoted $\{W\}$. We coded countries as being at war in years in which they were classified by COW (Singer and Small 1994) as parties to one or more interstate or extrasystemic wars.[8] Data from Polity III (May 1996 version) and COW were merged to provide annual observations on each independent polity from 1815 to 1992. We estimate the model given in equation 1 with these data.[9]

Estimation and Results

Table 3 gives the results of two logit estimations of equation 1. These suggest that the direction, magnitude, and variance of change are important in determining the log-odds of a polity being at war. Focusing first on the democracy scale and democracy minus autocracy results (which are substantively equivalent), we find that the current level is not especially important, but the direction and magnitude of change are both strongly related to the log-odds of being at war, although these two effects are of opposite sign.[10] Changes toward greater democratization are associated with significantly greater log-odds of war involvement, a finding consistent with the dangerous democratization thesis put forward by Mansfield and Snyder (1995a, 1995b) and others. Yet, the actual magnitude of that change is also important as a predictor of war involvement, with larger changes toward democracy associated with *smaller* probabilities of war involvement. The latter result is not consistent with the findings of Mansfield and Snyder (1995a, 1995b, 1997). When one focuses only on whether a change occurs, greater democratization appears to enhance the probability of war involvement, whereas examining the extent of those changes reveals that the probability is reduced. Averaging these two effects may lead to the conclusion in some studies that there is no effect, a finding reported in Thompson and Tucker (1997).

It seems important to look at the cumulative, integrated effect of the direction, magnitude, and variance of change on the probability of war. Examining the logit results independently may be misleading, since these three variables do not vary independently; they are mathematically related. For example, if there is no change over a decade, the magnitude of change is zero, as is the variance. Although many possible combinations of these variables are possible, they are not completely independent. Moreover, these log-odds ratios are not especially easy to interpret in substantive terms, though they are positively related to the probability of war involvement.

What does this mean about the probability of being at war? Rather than present another table, we calculate the probabilities for the dependent variable as a function of some scenario involving the independent variables. For example, Figure 1 uses the empirical estimates from Table 3 and equation 1 to illustrate what happens if a country moves smoothly (at mean levels) from a low level of democratization (democracy scale

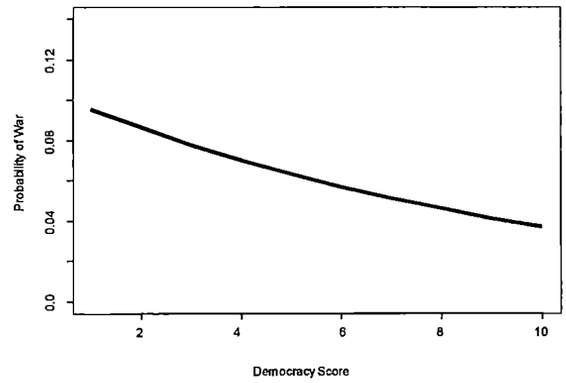
[7] While there was a border dispute during this period, it seems overly inclusive to code the 1843–45 period as a war between the United States and Mexico. The annexation of Texas occurred in late December 1845, and Taylor's "invasion" was in April 1846.

[8] Since the COW definition pertains primarily to properties of the conflict rather than characteristics of the individual states' participation, the criteria for war involvement on the monadic level can be ambiguous. A country may be classified as participating in a war while its own involvement remains low and few or no casualties occur. Alliance commitments with the warring societies may lead countries to declare war but not actively enter the conflict. Other countries may be involved in conflict and suffer battle-related deaths but not be classified as being at war if the overall level of violence does not reach the 1,000 threshold.

[9] We use SPLUS™ version 3.4 running on a SUN™ SPARC™ 5 running Solaris™ 2.4 for the analysis reported here; these results have been replicated using LIMDEP™ 7.0 running under MS-DOS™ 6.0 and SPLUS™ for Windows version 3.3.

[10] Based on these results and equation 1, prototypic democracies that do not change over ten years, for example, have a probability of being at war of 0.07, while prototypic autocracies have a corresponding probability of 0.08. This underscores the relative unimportance of the current regime characteristics. It should be pointed out, that the model was developed to assess the importance of changes in regime characteristics.

FIGURE 1. Estimates of the Probability of War Attendant to Democratization



Note: Empirical estimates in Table 3 applied to equation 1 as democracy score moves smoothly from 1 through 10.

of 1) to a high level of democracy (with a scale value of 10). The scenarios preserve the mathematical relationships among the independent variables. We examine the process of democratization and construct scenarios in which there is a transition of the authority characteristics over ten years. We look at democratization as well as "reversal." We present these results graphically in Figure 1, which displays the estimates of the probability of war involvement for societies undergoing democratic change. This yields the marginal changes in probabilities due to changes in the underlying process of democratization.

As Figure 1 details, democratization—whether in mild or strong degrees—is accompanied by reduction, not increase, in the risk of war. Though we do not present graphs of the converse, changes toward autocracy and reversals of democratization are accompanied by increased risks of war involvement. These risks are proportionally greater than the decline or benefits of further democratization. Thus, there is strong evidence that democratization has a monadic effect: It reduces the probability that a country will be involved in a war. Although the probability of war involvement does not decrease linearly, it does decrease monotonically, so that over the entire range of democracy minus autocracy values, there is a reduction of about 50%. During the democratic transition, at every point along the way as well as at the end points, there is an attendant *reduction* in the probability of a polity being at war.

We also find that reversals toward greater levels of autocracy (not shown) not only increase the probability of war involvement. Apparently, it is more dangerous to be at a given level of democracy if that represents an increase in the level of authoritarianism than it is to be at the same level of democracy if that represents a decrease in the authoritarian character of the regime. Stated differently, reversals are riskier than progress.[11]

TABLE 4. Empirical Estimates of Logistic Model of the Probability of War Based on Executive Constraints

Independent Variables	Executive Constraints Estimated Coefficient	Standard Error
Intercept	−2.663	.068
Current value	.043	.015
Direction of change	.229	.133
Change	−.110	.044
Variance of change	.07	.022
Likelihood ratio χ^2	21.89	p = .0002
Number of cases	10,681	

Note: These results are based on the executive constraints variable defined in Table 1. The range for the variable is [1,7]

It has been argued that institutional constraints are theoretically important in translating the effect of democracy into foreign policy (Bueno de Mesquita, Siverson, and Woller 1992; Siverson 1995). If the idea of *democracy* is separated into its major components, then the degree of executive constraints empirically dominates the democracy and autocracy scales (Gleditsch and Ward 1997). Accordingly, we demonstrate that moving toward stronger executive constraints also yields a visible reduction in the risk of war.

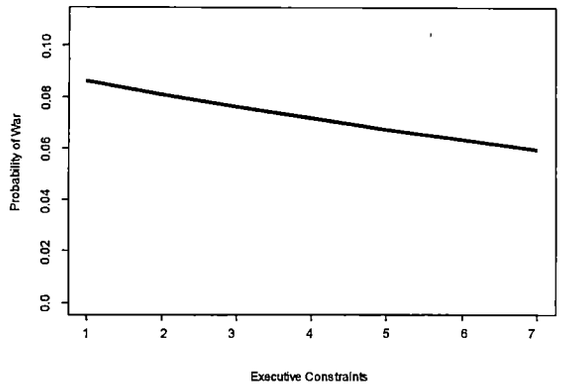
Table 4 presents the empirical findings from estimating the basic model using the executive constraints variables on the right-hand side of equation 1. These results are largely consonant with those found for the democracy and democracy minus autocracy scales presented in Table 3 but are much stronger in two respects. First, the overall model is more statistically distinct, with a probability of type II error at least one order of magnitude smaller (likelihood ratio χ^2 = 21.89, p = .000). Second, and more important, the direction of change variable is not significant, but all other variables in the model are strongly significant. The current value of executive constraints is shown to put upward pressure on the log-odds of being involved in war, while the magnitude of change toward greater executive constraints dampens the probability. As in the case for the democracy minus autocracy scale, the variance is strongly significant and has the effect of substantially increasing the likelihood that a polity will be at war.[12]

These results also suggest that Mansfield and Snyder (1995a, 1995b) may not have the story completely right but have not completely missed the boat. Democratic change, particularly increased constraint on chief executives, has a compound and confounding effect. Rocky changes toward democracy in general (or autocracy,

[11] Recent work by Rousseau et al. (1996) has questioned the appropriateness of using summed measures of dyadic behavior such as war involvement for testing possible monadic effects of authority structures. It is maintained that a crucial distinction must be made between the use of force in response to aggression and the initiation of force. While we consider the problem of identifying the conflict initiator troublesome—especially in protracted conflicts—supplementary tests for effects of democratization on the initiation of war nevertheless may provide additional evidence for evaluating our conclusions. We find that the likelihood of war initiation is reduced by more than 75% as countries move from a prototypic autocracy to a prototypic democracy, and it drops to less than 1% as polities reach higher values on the democracy minus autocracy scale.

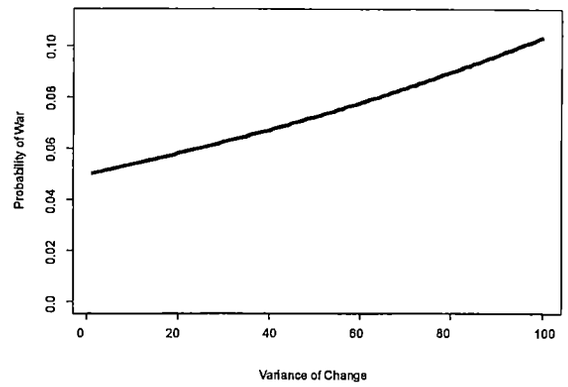
[12] We find virtually identical results for the analysis of war initiation with executive constraints.

FIGURE 2. The Probability of War Attendant to Changing Executive Constraints



Note: Empirical estimates in Table 4 applied to equation 1 as executive constraints move smoothly from 1 through 7.

FIGURE 3. The Probability of War Attendant to the Instability of Regime Change



Note: Empirical estimates in Table 3 applied to equation 1 as the variance of regime change (democracy minus autocracy) moves from 1 through 100 for the case in the middle of the scale moving quickly toward complete democracy.

for that matter) appear to heighten the likelihood of war involvement. Yet, sui generis, changes toward greater democracy via increased executive constraints appear to reduce the risk of war. The total effect of these forces is displayed graphically in Figure 2, which illustrates that if a polity moves from having no constraints on the executive decision maker (i.e., a value of 1.0) to a position of parity or subordination with the legislature (a value of 7.0), the probability of being involved in an interstate war is reduced by one-third, from about 9% to about 6%. This sheds light on precisely what aspect of democratization may reduce the probability of war: shared power between the executive and legislature, each largely staffed by officials pressured by public opinion. To the extent that changes toward democracy bring with them constraints on the executive branch of government, the attendant reduction in the risk of war appears quite robust.

How does the smoothness of the transition toward democracy or autocracy affect these conclusions? We find that transitions with periods of substantial changes toward and away from democracy are marked by an increased risk of war. Smooth transitions tend to be associated with a considerably lower risk. Figure 3 shows these results based on empirical estimates from Table 2, using the democracy minus autocracy scale. Democratic countries with the highest variance in authority characteristics over time have a risk of war approximately twice that of democracies without much variance.[13] This temporal investigation of the scope and extent of regime change allows us to understand two sets of competing findings. On the one hand, as countries become more democratic, other things being equal, they become more peaceful. On the other hand, if they experience setbacks as well as progress on the "road to democracy," then they are more likely to be involved in warfare along the way. At issue, apparently, is not the rapidity of change toward democracy but the linearity of the process. Smooth monotonic transitions are associated with the least risk and greatest benefit. Reversals, even in the short term, have the greatest risk. Prior analyses that did not focus on the process of transition could not discover these nuances.

CONCLUSION

Our results show that the process of democratization is accompanied by a decrease in the probability of a country being involved in a war, either as a target or as an initiator. These results were obtained with a more current (and corrected) database than was used in earlier work, and our analyses also focus more clearly on the process of transition. In comparison to studies that look only at the existence of change in authority characteristics, we examine the direction, magnitude, and smoothness of the transition process.

It is necessary to temper these results, however. First, our focus on the transition process excludes other important aspects of domestic and international politics that may affect the probability of being at war. We have not presented a complete framework. Although our model passes conventional statistical muster, it should not be used as a predictive model of war. We can correctly predict more than 90% of the outcomes, but all relate to the absence of war, the mode. War is rare, of course, and the predicted probabilities associated with these cases are higher, but they do not approach the 0.50 level that would be a minimum for predicting the presence of a war.[14]

Furthermore, our logistic analyses cannot rule out temporal or spatial effects that may outweigh the linkages we found between democratization and the propensity for war involvement. We know from other studies that both war involvement and democratization exhibit temporal as well as spatial patterns that are not

[13] Examples of countries with high variances include Argentina during the 1970s and 1980s, Guatemala during the 1960s, Austria before the World War II, Nigeria in the 1980s, and both Greece and Turkey during the post-1945 era.

[14] Nevertheless, the highest estimated probabilities are generally associated with cases in which there was a war involvement.

random. Statistical techniques for separating these effects and controlling for them in logistic models are not available. Indeed, it is necessary to transform the problem into a survival analysis with a spatial component in order to address these concerns more directly. That is a point of departure for future research but is a weakness in the current study.

Moreover, our focus on the monadic aspects of changes in regime characteristics ignores the strongest findings of the democratic peace literature. As a result, we have not examined the potentially powerful hypotheses that posit *joint* effects of democracy, common interests and alliances, the balance of capabilities, economic interdependence, and international commerce. Yet, by concentrating on the transition argument alone, we lose any traction that may be gained from these notions as well. In the same way, by focusing on democratization, we avoid generalizing about the causes of war. All these issues undoubtedly are important, and each is receiving considerable attention in the literature. Our purpose here was not to bring any evidence to bear on these ideas. Instead, we chose to look at whether democratizing states are more war prone than others.

Despite these limitations, our study makes a valuable contribution to knowledge about the linkages between democratization and conflict. We find evidence of a heightened propensity for war involvement in certain patterns of democratization and a diminished likelihood in others. Our analysis shows how the dangerous democratization thesis may be modified to be more consistent with the data. In particular, some aspects of democratization appear to increase the likelihood of war, as initially suggested by Gagnon (1994/5) and pursued by Mansfield and Snyder (1995a, 1995b, 1997). At the same time, it appears that the broad and overall effect of democratization is to promote peace.

Strong images support the notion of dangerous democratization. One is that of an incapacitated Russian leader faced with a welter of nationalist competition. The small bandage placed over the gaping wound in Grozny may be perceived as a opportunity that motivates a democratic consolidation. The conflict in the Balkans is often offered as stylized evidence that democratization is not only fragile but also dangerous. Events in Eastern and Central Europe have spawned considerable speculation about the dangers of democratization (e.g., Mansfield and Snyder 1995a, 1995b). Typical examples are Serbia, Croatia, Armenia, and Azerbaijan. Nevertheless, while these regimes may enjoy considerable popular support, it seems dubious whether they may accurately be characterized as democracies.[15] These were not independent countries before 1991, and it is hard to say how democratization—even if it is under way—can be separated from the process of state building and institutionalization. It is appropriate to question whether these examples are typical at all.[16] Many conflicts in the area have been predicted but have failed to materialize (e.g., Hungary versus Romania, Russia versus Ukraine, Poland versus Lithuania, Russia versus the Baltic Republics). Even the stylized facts are quite complicated and do not necessarily lead to the conclusion that newly emergent democracies are war prone. Indeed, not every political change is toward greater institutionalization of democratic norms and practices.

The democratic peace hypothesis has maintained that democracies do not go to war against other democracies. The dangerous democratization hypothesis has suggested that emergent democracies may be quite prone to international violence, largely because of "deformed" institutional forces. Our research does not rule out such an outcome, but it does demonstrate that democratization generally proceeds in a way to reduce the risk of war. It also shows that there is little statistical evidence to suggest that the movement toward more democratic practices renders the state more dangerous to international peace *unless* that transition is rocky and involves reversals along the way. Thus, it appears that the safest conclusion from these analyses is that democratic *reversals* increase the likelihood of warfare.

[15] In the Polity III data, Armenia has a democracy score of seven for the 1991–94, which meets the often used criterion of at least six. The others score as follows: Croatia, five in 1991 and 1992, two in 1993 and 1994; Serbia/Yugoslavia, two in 1991 and 1992, zero in 1993 and 1994; Azerbaijan, three in 1991, two in 1992, zero in 1993 and 1994. Note also that Serbia and Azerbaijan receive very high scores on the Polity III autocracy scale for 1993 and 1994 (six and eight, respectively).

[16] In their reply to Thompson and Tucker (1997) in the *Journal of Conflict Resolution*, Mansfield and Snyder (1997) list a number of cases to which they believe their model applies, including Rwanda in the 1990s. It is dubious whether these cases are consistent with their hypothesis that democratization and not autocratization may lead to war. It is hard to see how Rwanda can be classified as a democratizing regime. Many forms of political instability can be identified—domestic conflict, political violence, labor unrest—as having the potential to induce external conflict, but a regime losing control does not necessarily constitute democratization.

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Constructing a Supranational Constitution: Dispute Resolution and Governance in the European Community

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We present a theory of European legal integration that relies on three causal factors: transnational exchange, triadic dispute resolution, and the production of legal norms. After stating the theory in abstract terms, we explain the construction of the legal system and test the relationship among our three variables over the life of the European Community. We then examine the effect of the EC legal system on policy outcomes at both the national and supranational levels in two policy domains: the free movement of goods and gender equality. Our theory outperforms its leading rival, intergovernmentalism. The evidence shows that European integration has generally been driven by transnational activity and the efforts of EC institutions to reduce transaction costs, behavior which governments react to but do not control.

No international organization in world history has attracted as much scholarly attention as the European Community (EC).[1] The reason is straightforward: The EC has evolved from a relatively traditional (albeit multifaceted) interstate system into a quasi-federal polity. In a word, Europe has *integrated*, as the linkages between politics on the EC level and politics on the national level have expanded in scope and deepened in intensity. Scholars working in diverse fields, including public law, international relations, and comparative politics, have been fascinated by the integration process, not least because of the challenge of understanding the reciprocal effect, over time, of international and domestic systems of governance.

Current theoretical disagreements about how to understand European integration are largely disputes between intergovernmentalists, whose imagery is drawn from the international regime literature (Garrett 1992; Keohane and Hoffmann 1991; Moravcsik 1991, 1993; Taylor 1983), and supranationalists, whose imagery is often federalist (Burley and Mattli 1993; Leibfried and Pierson, eds., 1995; Marks, Hooghe, and Blank 1996; Sandholtz 1993, 1996; Sbragia 1993; Stone Sweet and Sandholtz 1997). Intergovernmentalists accord relative priority to member state governments—representatives of the national interest—who bargain with one another in EC fora to fix the terms and limits of integration. Supranationalists (especially the heirs of neofunctionalism), accord relative priority to EC institutions—representatives of the interests of a nascent transnational society—who work with public and private actors at both the European and national levels to remove barriers to integration and to expand the domain of supranational governance. This paper is implicated in these disputes. One of our claims is that, on crucial points, the intergovernmentalists have gotten it wrong.

More important, we propose a theory of European legal integration, the process by which Europe has constructed a transnational rule-of-law polity. The theory integrates, as interdependent causal factors, contracting among individuals, third-party dispute resolution, and the production of legal norms. We test the theory, with reference to the EC, in two stages. First, we explain the construction of the legal system, and analyze the relationships among our three key variables over the life of the EC. Second, we examine the effect of the operation of the legal system on governance, that is, on policy processes and outcomes, at both the national and supranational levels.

CONTRACTING, DISPUTE RESOLUTION, AND LAWMAKING

Our theory relies on three analytically independent factors that we expect to be interdependent in their effect. Because we believe the theory has general application (it helps us understand how all rule-of-law governmental systems may emerge and develop), we present it in an abstract form here. It has been elaborated more formally elsewhere (Stone Sweet n.d.)

The first factor is a simple contract—an exchange relationship—between two persons. Contracts, which are codified promises, fix the rules for a given exchange by establishing the rights and obligations of each contracting party with respect to the other. The contract is an inherently social institution, embedded in a cultural (or normative) framework that enables individuals not only to conceive, pursue, and express their interests and desires but also to coordinate them with those of others. Furthermore, to get to the very notion of a codified promise we must have language, notions of individual roles, commitment, reputation, and responsibility (which have no meaning outside a social setting) as well as some set of collective expectations about the future. As exchange proceeds over the life of the contract, or as external circumstances change, the

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The authors are grateful to the National Science Foundation, Grant No. SBR-9412531, the National Endowment for the Humanities, Grant No. FA-32480-94, the European Court of Justice, and the Robert Schuman Centre of the European University Institute for their support of this research. We also wish to thank James Caporaso, Lisa Conant, Russell Dalton, Harry Eckstein, Jonathon Golub, Bernard Grofman, Ronald Jepperson, Gary King, and Wayne Sandholtz for helpful comments.

[1] Although "European Union" is now commonly used to denote the European polity, we use "European Community" throughout. Formally, the EC remains distinct from the EU, and the EC is the most inclusive term for how the organization and its legal system function most of the time.

meanings attached to the same set of rules by the contractants may diverge. To the extent that such conflicts arise, contracting generates a social demand for third-party dispute resolution, for law and courts, the function of which is to sustain social exchange over time.

The second factor, then, is effective triadic dispute resolution. Without it, the costs of exchange may be prohibitive, since each prospective party may doubt that the other will abide by promises made over the life of the contract. A judicial system lowers these costs, providing a measure of certainty to each contractant and a means of reconsecrating the terms of the contract over time, given the certainty that differences in interpretation will arise as unforeseen circumstances arise. Transaction costs are particularly high in situations in which strangers—those who do not share a common normative framework (whether cultural or legal)—contemplate exchange and no effective triadic dispute resolution exists.

The triad—two disputants and a dispute resolver—constitutes a basic, probably primal, institution of governance (Simmel 1950, 145–69). In every human community about which we know anything, we find such triads, arrayed along a spectrum that stretches roughly from consent-based mediation to arbitration to coercive-adjudication. Commonly, triadic dispute resolution performs profoundly political functions, including the construction, consolidation, and maintenance of political regimes (Shapiro 1980, chapter 1), functions that inhere in the lawmaking dynamics of dispute resolution itself.

Consider formal adjudication, wherein judges are required, for legitimacy purposes among others, to provide legal reasons to support their decisions. When a judge decides, the lawmaking effect of the decision is always twofold. First, in settling the dispute at hand, the judge produces a legal act that is particular (it binds the two disputants) and retrospective (it resolves an existing dispute). Second, in justifying the decision, the judge signals that she will settle similar cases similarly in the future; this legal act is a general and prospective one (it affects future and potential contractants). Thus, judges do not simply or only respond to demands generated by social exchange. Rather, they adapt, continuously, the abstract legal rules governing exchange in any given community to the concrete exigencies of those individuals engaged in exchange.

The third factor is legislating, the elaboration of legal rules. Rules facilitate and also structure exchange by restricting some practices while permitting others. Conceived in this way, the legislator serves a social function rather similar to that of the judge: both produce rules that serve to reduce the transaction costs, enhance the legal certainty, and stabilize the expectations of those engaged in or contemplating exchange. Legislating, of course, is a far more efficient means of coordinating activity than is case-by-case adjudication and rule making. But because legal norms are so efficacious (immediately binding on broad classes of people and activities), their production poses a collective action problem. Partly for this reason, and partly due to the dynamics of judicial rule making, judges may legislate on matters before legislators do. In any case, in polities that possess both a permanently constituted legislature and an independent judiciary, lawmaking powers are shared, and boundaries allegedly separating institutional functions blur. Thus, legislatures rely on the legal system to enforce its law; and the judiciary possesses broad capacity to generate legal rules where none existed prior to a given dispute as well as to reconstruct legislative norms in interstitial processes of interpretation.

Viewed in dynamic relation to one another, contracting, triadic dispute resolution, and legislating can evolve interdependently and, in so doing, constitute and reconstitute a polity. Thus, as the number of contracts rise, the legal system will increasingly be activated. To the extent that the legal system performs its dispute-resolution functions effectively, it reduces contracting costs, thus encouraging more exchange. As the scope of legislation widens and deepens, the conditions favoring the expansion of exchange are constructed, the potential for legal disputes increases, and the grounds available for judicial lawmaking expand. New collective action problems are posed as older barriers to exchange are removed, and these problems push for normative solutions.

Components of the virtuous circle just described have been identified empirically and theorized by scholars working in diverse fields. North (1981, 1990) argues that differential rates of national economic development are in large part explained by the relative effectiveness of legal systems to reduce the costs of exchange among strangers. Although they did not focus on law and courts, Haas (1958, 1961) and Deutsch (1957) understood, somewhat differently, that sovereign states will respond to increasing levels of transnational interaction by integrating politically, that is, by creating common institutional and normative frameworks that in effect give birth to new systems of transnational governance. Haas used the term "spillover" to capture the expansive logic of integration. In their studies of the birth and subsequent development of legal systems, Kommers (1994), Landfried (1984, 1992), Stone (1992, 1994b), and Burley and Mattli (1993) show that tight linkages can develop between self-interested litigants and judges; these interactions generate a self-sustaining dynamic, which by feeding back into the greater political environment can reconfigure the inner workings of the polity itself. These sorts of "policy feedbacks," and their political consequences, are also familiar to historical institutionalists, who give them pride of place (Pierson 1993; Steinmo, Thelen, and Longstreth 1992).

In the rest of this paper, we examine the development of the European polity, focusing on the construction and operation of the legal system.

CONSTRUCTING THE SUPRANATIONAL POLITY

The emergence of a transnational rule-of-law governmental system cannot be presumed. Our theory sug-

gests that transnational exchange is a critical catalyst for such an event, generating a social demand for dispute resolution (transnational triadic dispute resolution), revealing important collective action problems that beg for normative solutions (transnational rules), and thereby pushing for modes of supranational governance. The theory further suggests that once the causal connections among exchange, triadic dispute resolution, and rules are forged, the legal system will operate according to a self-sustaining and expansionary dynamic. But the development of causal linkages among our three variables implies the existence of, respectively, some measure of individual property rights, some form of adjudication, and a lawgiver. For well-known reasons (e.g., Waltz 1979), these conditions have been notoriously difficult to achieve and sustain in the interstate system.

In Europe, the six states that signed the Treaty of Rome in 1958, establishing the European Economic Community, were able to overcome some of these difficulties but only partly. The treaty contained important restrictions on state sovereignty, such as the prohibition, within the territory constituted by the EC, of tariffs, quantitative restrictions, and national measures "having equivalent effect" on trade after December 31, 1969. It enabled the pooling of state sovereignty by establishing legislative institutions and a process for elaborating common European policies. And it established "supranational" institutions, including the European Commission and the European Court of Justice, to help the Council of Ministers (the EC institution that is controlled by national executives) legislate and resolve disputes about the meaning of EC law. Despite these and other important innovations, the member states founded an international organization, not a transnational rule-of-law polity. Some treaty provisions announced principles that, if implemented, would directly affect individuals (e.g., free movement of workers, equal pay for equal work between the sexes), but the treaty did not confer on individuals judicially enforceable rights.

Even within a European free trade zone we would expect the transaction costs facing transnational exchange to be higher than costs faced by those who contract within a single national jurisdiction, other things equal, to the extent that at the supranational level there exists no secure common legal framework comparable in its efficacy to that furnished by national legal systems. In the absence of such a framework, those who exchange cross-nationally would face a kaleidoscope of idiosyncratic national rules and practices that would act as hindrances. The establishment of an effective European system of dispute resolution as a means of overcoming national barriers to exchange is therefore a crucial first step.

In the next section, we briefly examine the European Court of Justice (ECJ) "constitutional" case law. These judgments recast the normative foundations of the EC, radically upgrading the capacity of the legal system to respond to the demands of transnational society. It bears emphasis that this case law constitutes a necessary condition underlying all the causal models tested in this paper.

The Constitutionalization of the Treaty System

The constitutionalization of the treaty system refers to the process by which the EC treaties have evolved from a set of legal arrangements binding upon sovereign states into a vertically integrated legal regime conferring judicially enforceable rights and obligations on all legal persons and entities, public and private, within EC territory. The phrase thus captures the transformation of an intergovernmental organization governed by international law into a multi-tiered system of governance founded on higher law constitutionalism. Today, legal scholars and judges conceptualize the EC as a constitutional polity, and this is the orthodox position (Lenaerts 1990; Mancini 1991; Shapiro 1992; Weiler 1981, 1991); international relations scholars are more reticent to do so for reasons internal to the development of international relations theory (Stone 1994a). In its decisions, the ECJ has implicitly treated its terms of reference as a constitutional text since the 1960s and today explicitly refers to the treaties as a "constitutional charter" or as "the constitution of the Community" (Fernandez Esteban 1994).

The ECJ, the "constitutional court" of the EC (Shapiro and Stone 1994, Weiler 1994), is the supreme interpreter of this constitution. The court's function is to enforce compliance with EC law. Although the outcome was not anticipated, the greater bulk of the court's case load is generated by preliminary references from national judges responding to claims made by private actors. The preliminary reference procedure is governed by Article 177 of the Rome Treaty. According to that article, when EC law is material to the resolution of a dispute being heard in a national court, the presiding judge may—and in some cases must—ask the ECJ for a correct interpretation of that law. This interpretation, called a preliminary ruling, shall then be applied by the national judge when settling the case. Article 177 was designed to promote the consistent application of EC law throughout EC territory. The member states did not mean to provide a mechanism by which individual litigants could sue their own government, or to confer on national judges the power of judicial review of national legislation. Both of these outcomes, however, inhere in the ECJ's vision of the community as a constitutional polity.

The constitutionalization process has been driven almost entirely by the relationship among private litigants, national judges, and the ECJ interacting within the framework provided by Article 177 (see Burley and Mattli 1993). The process has proceeded in two phases. In the 1962–79 period, the court secured the core constitutional principles of supremacy and direct effect. The ECJ made these moves without the express authorization of treaty law and despite the declared opposition of member states (Stein 1981). The doctrine of supremacy, announced in *Costa* (ECJ 1964), lays down the rule that in *any* conflict between an EC legal rule

and a rule of national law, the former must be given primacy. Indeed, according to the court, every EC rule, from the moment of entry into force, "renders automatically inapplicable any conflicting provision of ... national law" (*Simmenthal*, ECJ 1978). The doctrine of direct effect holds that provisions of EC law can confer on individuals legal rights that public authorities must respect and that may be protected by national courts. During this period, the ECJ found that certain treaty provisions (*Van Gend en Loos*, ECJ 1963) and a class of secondary legislation, called "directives" (*Van Duyn*, ECJ 1974a), were directly effective. The "regulation," the other major type of secondary legislation, is the only class of Euro-rule that was meant (according to the Treaty of Rome) to be directly applicable in national law.

These moves integrated national and supranational legal systems, establishing a decentralized enforcement mechanism for EC law. The mechanism relies on the initiative of private actors. The doctrine of direct effect empowers individuals and companies to sue national governments or other public authorities for not conforming to obligations contained in the treaties or regulations or for not properly transposing provisions of directives into national law. The doctrine of supremacy not only prohibits public authorities from relying on national law to justify their failure to comply with EC law but also requires national judges to resolve conflicts between national and EC law in favor of the latter.

In a second wave of constitutionalization, the ECJ supplied national courts with enhanced means of guaranteeing the effectiveness of EC law. In *Von Colson* (ECJ 1984), the doctrine of indirect effect was established, according to which national judges must interpret national law in conformity with EC law. In *Marleasing* (ECJ 1990a), the court clarified the meaning of indirect effect, ruling that when a directive either has not been transposed or has been transposed incorrectly into national law, national judges are obliged to interpret national law as if it were in conformity with European law. The doctrine thus empowers national judges to rewrite national legislation—in processes of "principled construction"—in order to render EC law applicable in the absence of implementing measures. Once national law has been so (re)constructed, EC law, in the guise of a de facto national rule, can be applied in legal disputes between private legal persons (i.e., nongovernmental entities). Finally, in *Francovich* (ECJ 1991a), the ECJ declared the doctrine of governmental liability. According to this rule, a national court can hold a member state liable for damages caused to individuals due to the state's failure to implement a directive properly. The national court may then require the state to compensate such individuals for their financial losses.

In this case law, the ECJ has imagined a particular type of relationship between the European and national courts: a working partnership in the construction of a constitutional rule-of-law European Community. In that partnership, national judges become agents of the community order—they become community judges—whenever they resolve disputes governed by EC law. The ECJ obliges national judges to uphold the supremacy of EC law, even against conflicting subsequent national law; encourages them to make references concerning the proper interpretation of EC law to the ECJ; and empowers them (even without a referral) to interpret national rules so that these will conform to EC law and to refuse to apply national rules when they do not.

The effectiveness of the EC legal system thus depends critically on the willingness of national judges to refer disputes about EC law to the ECJ and to settle those disputes in conformity with the court's case law. Although national judges embraced the logic of supremacy with differing degrees of enthusiasm, by the end of the 1980s every national supreme court had formally accepted the doctrine (Stone Sweet 1997a). National judges, persuaded by compelling legal arguments in support of supremacy, empowered themselves by, among other things, appropriating the power of judicial review of national legislation (Burley and Mattli 1993; Weiler 1991, 1994). The ease with which they were able to accommodate supremacy contrasts with the slower and more conflictual consolidation of the U.S. federal system (Goldstein 1994).[2]

Figure 1 plots the annual rate of Article 177 references, beginning with the first reference in 1961. It also temporally locates the leading constitutional decisions discussed here. The growth in the number of references is steady and dramatic. Without the doctrines of supremacy and direct effect, the level of preliminary references doubtlessly would have remained stable and low. In proclaiming supremacy and direct effect, the ECJ broadcast the message that EC law could be used by individuals, businesses, and interest groups to obtain policy outcomes that might otherwise be impossible, or more costly, to obtain by way of national policy processes. It is evident from the graph that litigants and national judges heard this message and responded.

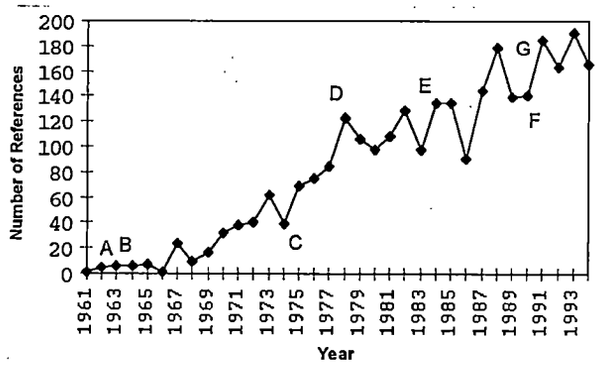
Finally, it cannot be stressed enough that the EC legal system was constructed without the explicit consent of the member states. They possessed the means to reverse constitutionalization, but only by revising the EC's constitution. Treaty revision requires the unanimous vote of all members, acting as a constituent assembly, followed by national ratification (according to diverse procedures, including referenda). Given this decision-making rule, it is not surprising that the member states have never overturned an ECJ interpretation of the treaties.

Data Analysis

Our theory yields a set of testable propositions. First, transnational exchange generates social demands for transnational triadic dispute resolution. Specifically, higher levels of cross-national activity will produce

[2] In the nineteenth century, transformations in the common law, rather than in constitutional law, were fundamental to American economic development and therefore to American integration (Horwitz 1977).

FIGURE 1. Annual Number of Article 177 References to the European Court of Justice



Source: The source for Article 177 references is data collected by the authors and the ECJ.
Note: Landmark decisions are highlighted: A, Doctrine of Direct Effect of Treaty Provisions (1963); B, Doctrine of Supremacy (1964); C, Doctrine of Direct Effect of Directives (1974); D, Simmenthal Doctrine (1978); E, Doctrine of Indirect Effect (1984); F, Marleasing Doctrine (1990); and G, Doctrine of State Liability (1991).

more conflicts between national and EC law and therefore more Article 177 references. Second, higher levels of transnational activity will push for supranational rules (judicial *or* legislative) to replace national rules. Third, to the extent that European judicial and legislative institutions function with minimal effectiveness, European integration—as evidenced by the rising tide of the ECJ's case law and of EC legislation—will feed back into society. The consolidation and expansion of European governance will fuel more transnational activity and provide the normative context for more Article 177 references in an increasing number of domains. Fourth, to the extent that the above propositions hold, transnational activity, transnational judicial activity, and the production of European legislation will develop interdependently, and this interdependence will drive European integration in predictable ways. That is, once the causal linkages among these three factors have been constructed, a dynamic, self-reinforcing process will push for the progressive expansion of supranational governance. These propositions, of course, depend critically on the prior announcement by the ECJ of supremacy and direct effect.

We tested our theory with data collected in 1995 at the ECJ in Luxembourg. With the help of the court, we gathered information on Article 177 reference activity from 1961 to mid-1995 for a total of 2,978 references. We then coded each by country of origin, year of referral, the national court making the reference, and the subject matter of the dispute, among other things. These data had never been compiled. We also compiled data on transnational exchange and the production of European legal rules; for the former, we make heavy use of data on intra-EC trade, because it is the only reasonable indicator of transnational exchange for which we have reliable information reported annually, partner-by-partner, over the life of the EC.[3]

We have argued that transnational exchange is fundamental to the construction of a transnational legal system. To begin testing the proposition, we confronted one of the deepest mysteries of European legal integration: What accounts for the wide cross-national variance in the number of Article 177 references? The scholarly literature on the problem has produced a handful of reasonable candidate explanations, including the role of legal culture, the bureaucratic organization of the courts, the extent of constitutional monism or dualism, and the length of a judiciary's experience with judicial review. Two recent studies, one by Dehousse (1996) and the other a collaborative project focused on the reception of supremacy by the national courts (Slaughter, Stone, and Weiler 1997), assessed these and other factors in comparative perspective. Both studies generally conclude that variance in the intensity of the ECJ-national court relationship is overdetermined and/or explained by factors operating with different effects across national borders.[4]

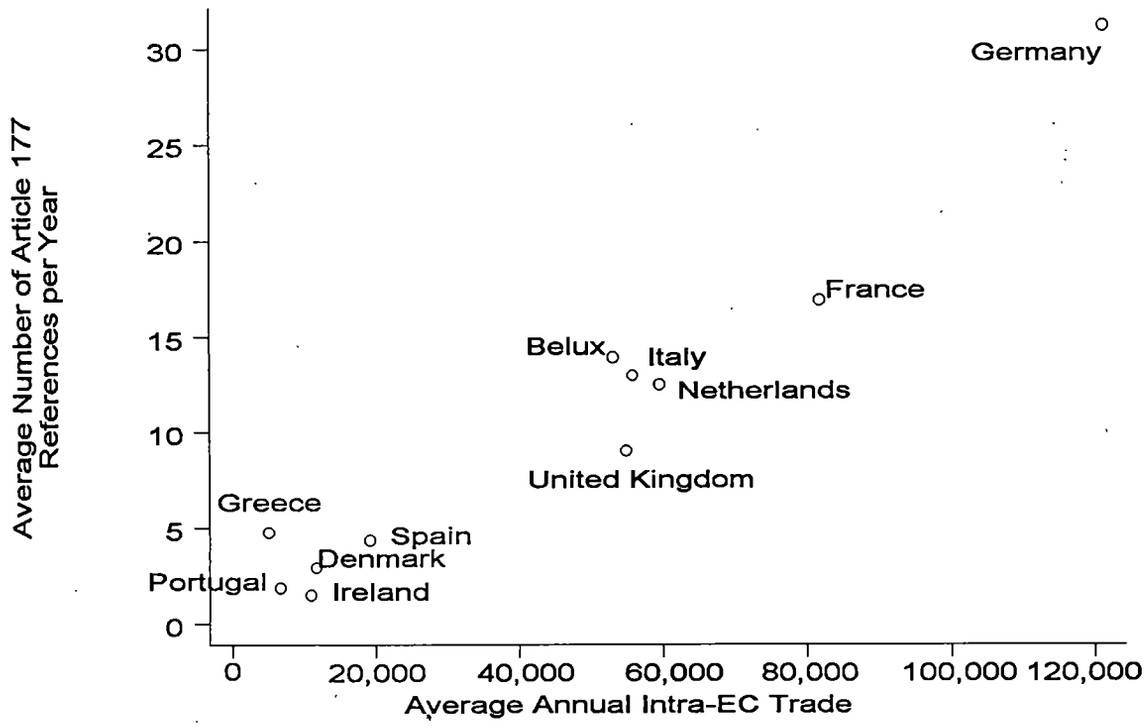
Our theory provides an alternative explanation, one based on cross-national activity. Figure 2 depicts the correlation of the average level of intra-EC trade over 1961–92 on the average number of Article 177 references per year from the national courts of each of the twelve member states.[5] We averaged the number of references annually in order to correct for the fact that some states joined the EC later than others, and we have combined reference data for Belgium and Luxembourg because the trade data for those states are combined by Eurostat reporting services. The linear relationship between intra-EC trade and references is nearly perfect (adj. R^2 = .92), with countries that trade more with their partners in the EC generating higher levels of Article 177 references. The results broadly support our theoretical claims. We also examined the effect on references of other plausible and quantifiable independent variables, including cross-national measurements of "diffuse popular support" for the EC legal system, population, and GDP, but none came

[3] We are not arguing that intra-EC trade, one type of transnational activity, subsumes other important forms of exchange, such as labor and capital flows, and the formation of EC interest groups and social movements. Unfortunately, data on these and other forms of exchange are incomplete and often unreliable. We would expect that increasing transnational activity of a particular type, within a given domain, will drive integration processes in that area (e.g., patterns in cross-national flows of workers will drive litigation in social security). Generally, we had good reason to expect that trade would dominate the construction of Europe since, for most of the life of the EC, the core of the European integration project has been the creation of a common market for goods and agriculture. For further discussion of this point, see tables 3 and 4.

[4] There is little point in formally testing these explanations. We know by simply looking at the raw data on references comparatively that alleged relationships do not hold between the factors cited above and national levels of Article 177 references (e.g., the more monist the constitutional order, the more references generated). If these factors do affect levels of Article 177 references, they must operate with different effects across the EC.

[5] In 1995, the Eurostat reporting service furnished annual intra-EC trade figures for 1958–92. Because the service has not yet updated these figures, and because subsequent data are reported on different scales, we have not used data after 1992 in any of the regression models reported here.

FIGURE 2. Intra-EC Trade and Average Number of Article 177 References



Source: Trade data are from *Eurostat: External Trade 1958–93*, Brussels: EC Publications, 1995. The source for article 177 references is data collected by the authors and the ECJ.
Note: For each member-state, the total number of Article 177 references was divided by the number of years in which that member-state has been making references to the ECJ. Belux is Belgium and Luxembourg combined. Trade figures are the annual average of intra-EC imports and exports for each member-state measured in Ecu (European currency unit), 1960–93. The regression equation is $y = .2105 + .0002247(\text{Trade}) + e$. Adjusted $R^2 = .92$, $n = 11$, and the *SEE* = 2.46. The *t*-statistics are .178 for the constant and 10.8 for intra-EC trade.

close to performing as well as intra-EC trade. See Appendix B.

Figure 2 depicts the relationship between intra-EC trade and Article 177 references cross-nationally, with no time element. Figure 3 depicts the relationship between the same two variables over time, since 1961, with no cross-national element. In this model, we include a dummy variable to account for (1) the constitutionalization of the treaties and (2) the prohibition of national restrictions on intra-EC trade (Article 30, EEC) that took effect on January 1, 1970. As we have seen, the doctrines of supremacy and direct effect made it possible for individuals to have their rights under EC law protected before their own national courts; and, as of 1970, Article 30—which proclaims the free movement of goods—provided the legal basis for traders to claim those rights (Poiares Maduro 1997).[6] We coded the dummy variable 0 from 1961–69 and 1 from 1970–92 (hereafter the "post-1969 dummy").

Figure 3 plots the actual and predicted annual levels of Article 177 references for the EC as a whole. The predicted level—generated by a regression analysis in which the dependent variable is the yearly number of Article 177 references and the independent variables are annual intra-EC trade and the post-1969 dummy—plots the references predicted by the independent variables. The adjusted $R^2 = .91$, and the coefficients for both intra-EC trade and the dummy variable are positive and statistically significant.

Our time-series data for intra-EC trade and Article 177 references are nonstationary (the augmented Dickey-Fuller test), a common problem for data containing a strong trend. We argue that our results are nevertheless valid for two reasons. First, the data on intra-EC trade and Article 177 references are cointegrated, indicating that a linear combination of the two variables is stationary.[7] Second, we checked for serial autocorrelation in the error terms and found none.[8]

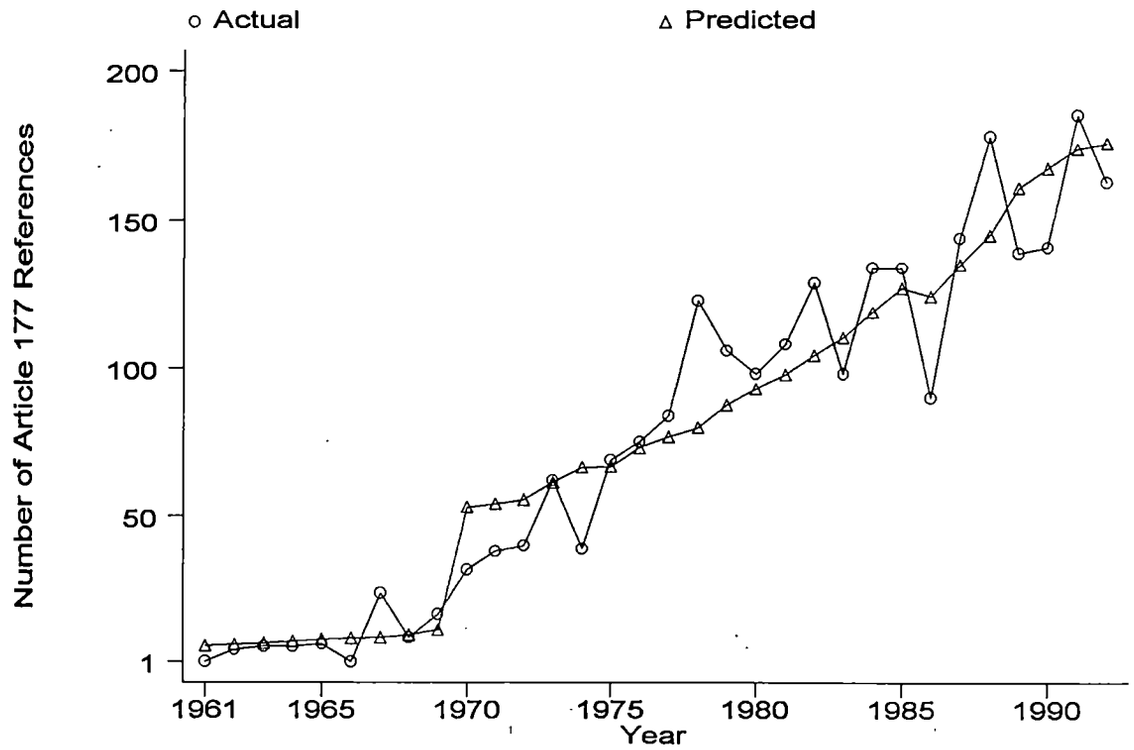
We would be more confident in our results if we had more observations. By using our time series and our cross-national data together, we were able to increase the number of observations and provide a more stringent test of the effect of transnational exchange on judicial activity. Table 1 presents the results of two pooled models. The first examines the effect of in-

[6] In addition (pursuant to Article 33, EEC), in 1970 the European Commission produced a directive clarifying the meaning of the principle of free movement of goods and the lawful exceptions to it.

[7] Using the Johansen cointegration test, we reject the hypothesis of no cointegration between these two variables at the .01 level.

[8] Using Econometric Views 2.0, we examined the correlogram. All the autocorrelations of the residuals, with the exception of one (at $t - 8$), were within plus or minus two standard errors of zero.

FIGURE 3. Actual and Predicted Annual Levels of Article 177 References from Intra-EC Trade



Source: Trade data are from *Eurostat* (1995). The source for Article 177 references is data collected by the authors and the ECJ.
Note: The actual line plots the yearly number of Article 177 references by national courts to the ECJ, 1961–92. The predicted line plots the number of annual references predicted by the regression analysis in which intra-EC trade and a post-1969 dummy variable (coded 0 from 1961–69 and 1 from 1970–92) are the independent variables, and the dependent variable is the annual number of Article 177 references for the EC as a whole. Levels of aggregate trade begin with the original six member states (Belgium, France, Germany, Italy, Luxembourg, and the Netherlands); as new member states join the EC, their trade figures are included. The regression equation is $y = 3.56 + .0000938(\text{intra-EC Trade}) + 39.93(\text{post-1969 dummy}) + e$. The adjusted $R^2 = .91$, $n = 32$, $SEE = 17.92$, and the Durbin-Watson statistic for the regression equation = 1.85. The t-statistics are 0.59 for the constant, 10.59 for intra-EC trade, and 4.52 for the post-1969 dummy.

TABLE 1. The Effect of Intra-EC Trade on Article 177 References: Pooled, Cross-sectional, Time-Series Models

	Model 1	Model 2
Intra-EC Trade	.000126***	.0000995***
	(18.89)	(13.29)
Post-1969 Dummy		7.64***
		(6.25)
Adjusted R^2	.73	.77
SEE	6.19	5.74
N	246	246

Source: The source for the trade data is *Eurostat* (1995). The source for Article 177 references is data collected by the authors and the ECJ.
Note: Entries are unstandardized regression coefficients, with t-statistics reported in parentheses. The dependent variable is annual Article 177 references for each member state, per year. The independent variable for model 1 is intra-EC trade, the value of both imports and exports for each member state (Belgium and Luxembourg combined) with all other member states, for each year. The independent variables for model 2 are intra-EC trade and a dummy variable coded 0 from 1961–69 and 1 from 1970–92. The model consists of 246 observations: Belgium-Luxembourg 1961–92; Denmark 1973–92; France 1961–92; Germany 1961–92; Greece 1981–92; Ireland 1973–92; Italy 1961–92; Netherlands 1961–92; Portugal 1986–92; Spain 1986–92; and United Kingdom 1973–92. Econometric Views 2.0 was used to estimate a fixed effects model. See Stimson (1985) and Sayrs (1989) for a discussion of pooled models.
***$p < .001$.

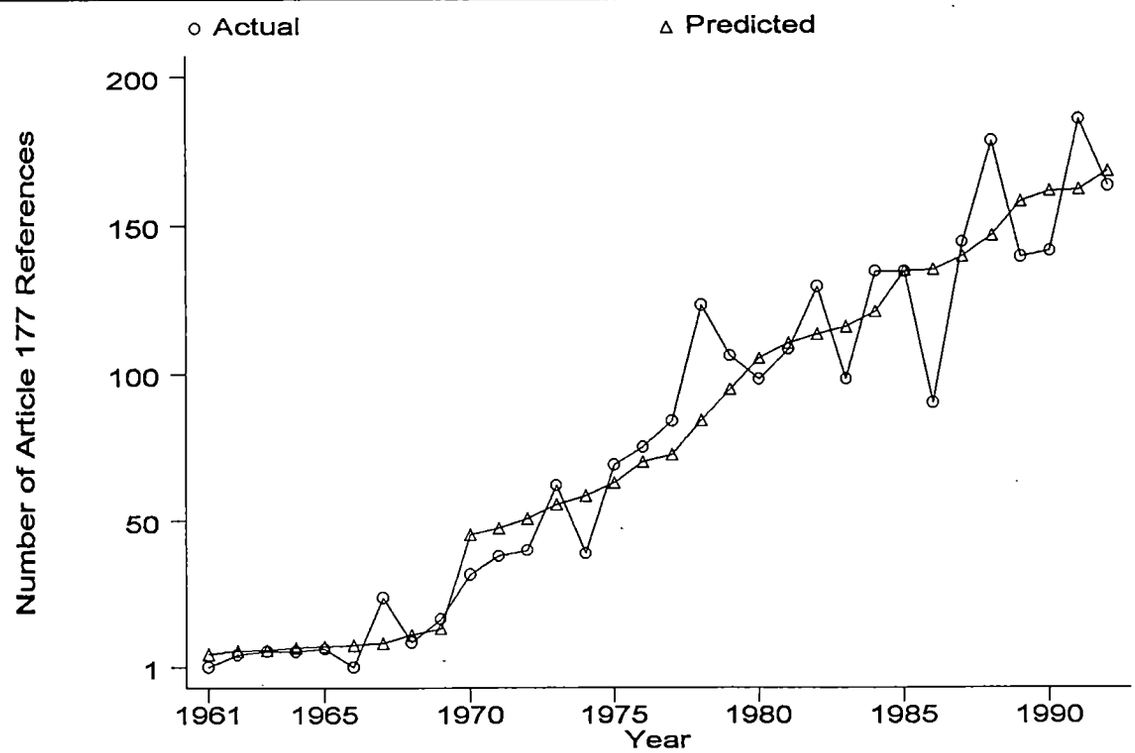
tra-EC trade on Article 177 references, cross-nationally and over time; the second examines this same relationship but with the post-1969 dummy included as a second independent variable. The trade variable in both models is positive and highly statistically significant. Following the advice of Stimson (1985) and King (1986), after estimating the models, we ensured that no serial autocorrelation of the errors existed.[9]

Thus, we find strong support for our claim that transnational exchange has been a crucial factor driving the construction of the EC's legal system.[10] Furthermore, our analysis does not conflict with—indeed, it builds on—the basic narrative told by legal scholars of how the ECJ constitutionalized the treaties.

[9] Using Econometric Views 2.0, we examined the correlograms for each panel. All the autocorrelations of the residuals were within plus or minus two standard errors of zero, and no significant patterns were found.

[10] We expect that as the European polity matures, the litigation of EC legal disputes will increase. We do not expect that Article 177 references will continue to rise indefinitely. The capacity of the ECJ to process references is limited. We predict that national judges themselves will increasingly resolve EC legal disputes on their own, without a prior reference. For a discussion of the problem of Article 177 and the overloaded docket of the ECJ, see Weiler 1987.

FIGURE 4. Actual and Predicted Annual Levels of Article 177 References from Intra-EC Trade and Euro-rules



Source: Trade data are from *Eurostat* (1995). Euro-rules are the annual number of directives and regulations promulgated by the EC. The source for Article 177 references is data collected by the authors and the ECJ.
Note: The actual line plots the yearly number of Article 177 references by national courts to the ECJ, 1961–92. The predicted line plots the number of annual references predicted by the regression analysis in which intra-EC trade, Euro-rules, and a post-1969 dummy variable (coded 0 from 1961–69 and 1 from 1970–92) are the independent variables, and the annual number of Article 177 references for the EC as a whole is the dependent variable. Levels of aggregate trade begin with the original six member states (Belgium, France, Germany, Italy, Luxembourg, and the Netherlands); as new member states join the EC, their trade figures are included. The regression equation is $y = 2.84 + .0000731(\text{Intra-EC Trade}) + 2.04(\text{Euro-rules}) + 31.35(\text{post-1969 dummy}) + e$. The adjusted $R^2 = .92$, $n = 32$, $SEE = 17.01$, and the Durbin-Watson for the regression equation = 2.18. The t-statistics are .50 for the constant, 5.55 for intra-EC Trade, 2.04 for Euro-rules, and 3.34 for the post-1969 dummy.

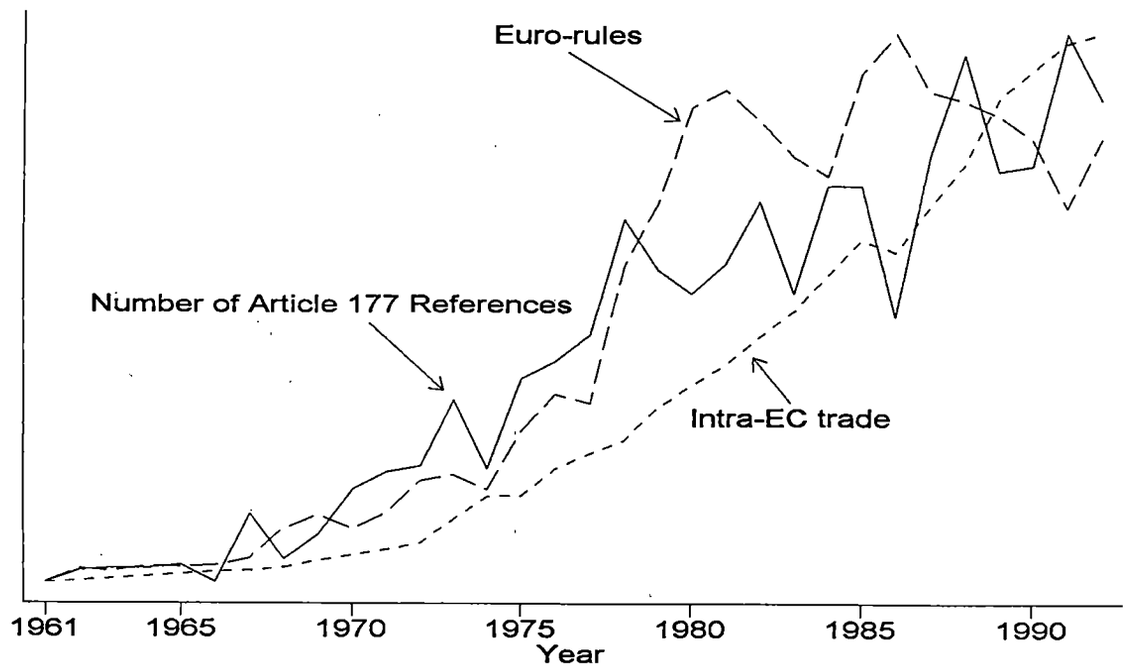
We have also argued that the emergence of effective transnational triadic dispute resolution is seminal to the emergence of supranational governance. Our theory suggests that the operation of the legal system will produce powerful feedback effects, the most important of which are normative (rule based). One crucial function of triadic dispute resolution is to produce stable, normative solutions to collective action problems. In principle, a governmental system can be constituted entirely by judicial rule making: The dispute resolver governs by the pedagogical authority of its decisions. In practice, courts share governmental authority with legislative bodies, not least because legislating is a more efficient way to produce legal rules than is adjudicating. Once a transnational legal system has been consolidated, the production of European legal rules—whether by judicial or legislative processes—will facilitate the expansion of transnational exchange.

To evaluate the interrelationships that may have developed among our three variables, we collected data on the annual production of the two general categories of EC legislation: regulations and directives (hereafter called Euro-rules). Both types of legislation are drafted and proposed by the European Commission, a supranational body that blends legislative and administrative functions (Ludlow 1991). Simplifying a complicated process, these proposals can be amended in interactions involving the European Parliament, the commission, and the Council of Ministers (Tsebelis 1994). Euro-rules are finally adopted by the council, a body composed of ministers from member governments, the exact composition of which is determined by the subject matter of EC law under discussion (Wessels 1991).

Figure 4 plots the actual and predicted annual levels of Article 177 references for the EC as a whole. The predicted line (generated by a regression analysis in which the dependent variable is the yearly number of Article 177 references and the independent variables are annual intra-EC trade, the annual number of Euro-rules promulgated, and the post-1969 dummy) plots the level of references predicted by the independent variables. The adjusted $R^2 = .92$, and the coefficients for all three variables are positive and significant, as expected.[11]

[11] Using the Johansen cointegration test, we rejected the hypothesis

FIGURE 5. Annual Levels of Intra-EC Trade, Euro-rules, and Article 177 References



Note: The Article 177 references are the yearly number for the EC as a whole, 1961–92. The Euro-rules are the annual number of directives and regulations promulgated by the EC. The intra-EC trade line plots levels of aggregate intra-EC trade for the EC as a whole. The graph has been rescaled since the variables are on different scales.

Figure 5 depicts the growth of transnational exchange (in the form of intra-EC trade), the evolution of transnational judicial activity (in the form of Article 177 references), and the development of transnational legal norms (in the form of Euro-rules). It thus shows the development of the European polity. The high intercorrelation among the three variables is another way of describing the virtuous circle at the core of our theory.

Do European integration processes serve to expand transnational activity? To answer that question, we compared annual rates of growth in trade over the life of the EC among (1) EC states with one another and (2) non-EC states with EC members. In order to maximize comparability, we focused on two groupings: the original six members and the three states (Denmark, Ireland, and the United Kingdom) that joined in 1973. As the numbers in Table 2 show, before 1973, the growth in trade was far higher among EC members than between EC states and the three nonmembers. Once these latter joined the EC, their growth rates rose above levels achieved by the original six. Although trade is a crucial measure of the degree of integration among EC members, its relative importance in activat-

of no cointegration among these three variables at less than the .05 level. To test further the existence of a cointegrating relation, we undertook two separate tests (see Johnston and DiNardo 1997, 266–70). First, using the augmented Dickey-Fuller test, we checked to ensure that the residuals from the regression analysis were stationary. We were able to reject the hypothesis of nonstationarity at the .01 level. Second, we fitted a general autoregressive distributive lag (ADL) specification to these variables. This entails using lagged values of the three variables on the right-hand side of the OLS equation. We then conducted F-tests on the sums of the coefficients for current and lagged values for each variable (results omitted). Results show that the sums of the coefficients for both the Article 177 references and intra-EC trade variables differ significantly from zero at the .01 level. The p-value for the sum of the coefficients for the Euro-rules variable was .076, which approaches statistical significance. Taken together, these tests provide further support for our claim that these three variables have come to develop interdependently.

TABLE 2. Average Growth Rates in Trade between Individual States and All EC Members, by Period, in Percentage

	1961–72	1973–92	Percentage Change
Belux	11.8%	9.4%	−20.1%
France	13.5	9.2	−31.7
Germany	12.1	10.4	−14.3
Italy	13.3	10.8	−18.2
Netherlands	11.3	9.4	−16.9
Average	12.4	9.8	−20.2
Denmark	6.0	9.9	+65.0
Ireland	9.3	11.4	+22.1
United Kingdom	8.8	11.3	+28.0
Average	8.0	10.9	+38.4

Source: *Eurostat* (1995).
Note: Entries under the 1961–72 and 1973–92 columns are the average rate of growth in trade between each member state listed with all members of the EC. Entries under the third column are the change registered, between the two periods, in the growth rates in trade between each state listed with all members of the EC. Belux combines Belgium and Luxembourg. Denmark, Ireland, and the United Kingdom entered the EC in January 1973.

TABLE 3. Distribution of Legal Claims by Subject Matter, Article 177 References

Subject Matter		1961–70	1971–75	1976–80	1981–85	1986–90	1991–95
Agriculture	%	20.4	30.9	25.1	20.3	19.6	12.5
	n	(30)	(109)	(176)	(153)	(160)	(135)
Free movement of goods		13.6	19.6	17.1	19.8	18.5	14.9
		(20)	(69)	(120)	(149)	(151)	(161)
Social security		19.7	12.5	10.3	8.4	9.1	10
		(29)	(44)	(72)	(63)	(74)	(108)
Taxes		11.6	2	4.6	4.8	8	8.5
		(17)	(7)	(32)	(36)	(65)	(92)
Competition		8.8	6.3	3.6	4.5	5	8.2
		(13)	(22)	(25)	(34)	(41)	(89)
Approximation of laws		2.8	.3	1.9	4.9	3.8	5.1
		(4)	(1)	(13)	(37)	(31)	(55)
Transportation		2	6	1.4	1.3	1.5	2.9
		(3)	(2)	(10)	(10)	(12)	(31)
Establishment[a]		.7	2.6	2.3	2.7	5.6	6.6
		(1)	(9)	(16)	(20)	(46)	(72)
Social provisions		.7	.3	1.1	3.7	4.2	8.8
		(1)	(1)	(8)	(28)	(34)	(95)
External[b]		.7	2.8	1.7	2.4	.9	1.5
		(1)	(10)	(11)	(18)	(7)	(16)
Free movement of workers		.7	4.3	2	4.1	5	3.2
		(1)	(15)	(14)	(31)	(41)	(35)
Environment		0	0	.4	1.6	.9	2
				(3)	(12)	(7)	(22)
Commercial policy and dumping		0	.6	1.4	.9	.9	1.8
			(2)	(10)	(7)	(7)	(19)
Total claims		147	352	702	754	816	1084
Percentage of total claims by period		3.8	9.1	18.2	19.6	21.2	28.1

Source: Data compiled by the authors with the help of ECJ.
Note: The table lists only the thirteen most important legal domains, which together comprise more than 80% of the 3,855 total claims. We coded references by subject matter and relevant provision of the EEC Treaty. Each of the legal subject matters listed in the first column corresponds to the articles of the EEC Treaty following in parentheses: *agriculture* (38–47); *free movement of goods* (9–37); *social security* (51); *taxes* (95–9); *competition* (85–94); approximation of laws (100–2); transportation (74–84); establishment (52–66); social provisions (117–22); external free movement of workers (48–50); environment (130R–T); and commercial policy and dumping (110–6). Although most references are limited to a single subject matter of EC law, some references contain claims based on as many as five subject matters. This accounts for the difference between the total number of references and the total number of subject matters invoked in references.
[a]The freedom to establish undertakings and to provide services.
[b]A miscellaneous category including all EC economic policies affecting the European Free Trade Area, the GATT, food aid, and special agreements with non-EC states.

ing the legal system also must be evaluated in terms of changes in the density and scope of Euro-rules.

The theory, after all, posits an expansive logic to integration processes. According to this logic, the growing interdependence of transnational exchange, judicial activity, and Euro-rules drives the progressive construction of the supranational polity. By that we mean the process by which governmental competences, in an increasing number of domains, are transferred from the national to the supranational level. Simply put, as triadic dispute resolution and other processes remove the most obvious hindrances to transnational exchange (border inspections, fees and duties, and so on), and as supranational coordinative rules replace the disparate rules in place in the member states, new obstacles to integration are revealed and become salient (such as regulations protecting consumers and the environment). These national rules and practices will be targeted by litigants, and pressure will be exerted on EC legislative institutions to widen the jurisdiction of EC governance into new domains. We think of this dynamic as a kind of legal "spillover."

Our data provide some preliminary support for our contention. First, we examined the evolution of the substantive content of Article 177 references. Recall that these claims constitute allegations, by private litigants, that rules or practices in place in an individual country conflict with Euro-rules. Litigants have therefore requested national judges to enforce EC law by, in effect, eliminating conflicting national rules or practices. Table 3 charts this evolution, vividly recording the extent to which the domain of EC law has expanded. The percentage of claims involving the direct exchange of goods—the free movement of goods and agriculture—has steadily declined, dropping from more than 50% in 1971–75 to 27% in 1991–95. At the same time, more indirect hindrances to trade, such as national rules governing equal pay for equal work (social provisions), environmental protection, and taxation policy, have become important sites of contestation.

Second, we assessed the relationship between trade and Article 177 references over time by including a variable to capture the interaction of intra-EC trade and time. Table 4 presents the results of this model. Our expectation about the interaction variable was that

TABLE 4. The Declining Effect of Intra-EC Trade on Article 177 References: Pooled, Cross-sectional, Time-series Models

Intra-EC trade	.000283***
	(5.26)
Post-1969 dummy	4.62**
	(3.11)
Trade *interaction	−.00000554**
	(−3.44)
Adjusted R^2	.78
SEE	5.61
N	246

Source: The source for the trade data is *Eurostat* (1995). The source for Article 177 references is data collected by the authors and the ECJ.
Note: Entries are unstandardized regression coefficients, with *t*-statistics reported in parentheses. The dependent variable is annual Article 177 references for each member state, per year. The independent variables are intra-EC trade, or the value of both imports and exports for each member state (Belgium and Luxembourg combined) with all other member states, for each year; a dummy variable coded 0 from 1961–69 and 1 from 1970–92; and an interaction variable, which is intra-EC trade multiplied by year. The model consists of 246 observations: Belgium-Luxembourg 1961–92; Denmark 1973–92; France 1961–92; Germany 1961–92; Greece 1981–92; Ireland 1973–92; Italy 1961–92; Netherlands 1961–92; Portugal 1986–92; Spain 1986–92; and United Kingdom 1973–92. Econometric Views 2.0 was used to estimate a fixed effects model. See Stimson (1985) and Sayrs (1989) for a discussion of pooled models.
**$p < .01$; ***$p < .001$.

the coefficient would be negative, indicating a declining effect of trade on Article 177 references over time. The coefficient for the interaction variable is indeed negative and statistically significant. We believe that the growing articulation and differentiation of the Euro-rules that constitute the EC's normative structure will gradually generate more Article 177 references.

In the next section, we examine more closely our contention that the operation of the EC legal system both provokes and reinforces the spillover effects that partly drive the construction of supranational governance.

DISPUTE RESOLUTION AND THE DYNAMICS OF SUPRANATIONAL GOVERNANCE

We have argued that integration processes are *generally* driven by transnational activity, by efforts of supranational institutions to reduce transaction costs, and by the positive feedback effects on transnational society of supranational judicial lawmaking and legislating. In this section, we cross-check our theory by, among other things, examining concrete policy outcomes in specific domains of EC law. We therefore shift the perspective from the broad relationships depicted in the statistical analysis to a more fine-grained examination of how, and to what effect, the legal system operates. Because our theory fundamentally conflicts with the dominant framework, intergovernmentalism, we begin by summarizing our differences.

Intergovernmentalists argue that national executives are in control of every crucial step in the construction of the European polity. Employing a logic derived from two-level game imagery (Evans, Jacobson, and Putnam 1993), Moravcsik (1993, 1994), for example, claims that governments, acting in the Council of Ministers and in summit meetings, establish the parameters that determine the content, scope, and pace of integration. Governments cooperate to achieve their policy goals and to enhance their autonomy in domestic politics. Due to the decision-making rules in place in most EC legislative processes (unanimity and super-majority voting), the Euro-rules produced reflect the preferences of those governments which support the least amount of integration in any given area (the lowest common denominator). The behavior of private actors, subnational public authorities, and the EC's organs are secondary; they serve to "consolidate" new levels of integration (Moravcsik 1995).

Intergovernmentalists also have sought to explain the operation of the legal system. Employing a logic derived from principal-agent theories of delegation (Kiewiet and McCubbins 1991), Garrett (1992) argues that the ECJ (the agent) serves the interests of the dominant members in the EC (the court's principals). The ECJ, intergovernmentalists claim, codifies the preferences of these states in its case law. It does so in order to avoid court-curbing measures and to secure compliance with its rulings.

In contrast, we argue that governments do not control legal integration in any determinative sense and therefore cannot control European integration more broadly. We do not want to be misunderstood. The EC polity contains strong "intergovernmental" components, that is, EC politics are partly constituted by the interactions among representatives of the governments. But it is our contention that "intergovernmentalism," when that term denotes the body of theory and causal propositions about European integration, is deeply flawed (see also Pierson 1996). In using the word *intergovernmentalism*, we need to distinguish the descriptive from the theoretical label. Moreover, any theory of European integration must notice and take account of the role of governments, clearly stating how that role is conceptualized.

The Council of Ministers and representatives of the member states are important actors in European politics. We understand their effect on integration to be positive when they (1) work with supranational institutions to adopt, at the supranational level, Euro-rules and (2) transpose, on the national level, European directives into national law. They have a negative influence on integration when they (1) block EC legislation (in their capacity as members of the council) and (2) refuse to comply with the Euro-rules they do not like (in their capacity as national governments).

Generally, we expect governments to be more reactive than proactive within integration processes. European integration facilitates not only transnational trade but also the construction of associations and social movements, and this exchange, as it rises, pressures governments to act in prointegrative ways. At any point in time, and in any particular area, governments can fail to respond to transnational interests, but (if levels of exchange are rising) only at ever-increasing costs. Thus, we expect that the pace of integration will vary

across issue areas, partly as a function of the relative intensity of transnational activity in a given area. And we expect the EC's legal system to serve the interests of transnational society, not individual governments, or even a consortium of powerful governments (the argument is elaborated more fully in Stone Sweet and Sandholtz 1997).

Empirically, our differences with intergovernmentalists must begin with the challenge of explaining the constitutionalization of the treaty system. As we have tried to demonstrate, constitutionalization profoundly transformed the EC polity. As discussed above, the ECJ, activated by Article 177 references, constructed the EC legal system: It worked to diffuse the doctrines of supremacy and direct effect and to provide national judges with the means to enforce EC law, even against national governments and the legislatures that governments control. Further, constitutionalization did not take place surreptitiously. During the proceedings that preceded the ECJ's announcements of supremacy and direct effect, various governments argued, in formal "observations"—briefs advising the court how it should decide—that the Treaty of Rome could not be interpreted so as to support either doctrine (Burley and Mattli 1993, Stein 1981). Nevertheless, the court revised the treaty by authoritatively interpreting it, and these interpretations transformed the nature of EC governance. Governments agreed to this transformation, but only after the fact and only tacitly, by progressively adjusting their behavior to the emergence of new rules.

We view the ECJ, not unlike Weiler (1981, 1991, 1994) and Burley and Mattli (1993), as generally working to enhance the autonomy of the EC legal system, autonomy that is then exercised to promote the interests of transnational society and to facilitate the construction of supranational governance. The court does not work in the interests of member governments, except in the very loose sense in which those interests can be construed as being in conformity with the treaty's purposes broadly—not narrowly—conceived. The move to supremacy and direct effect must be understood as audacious acts of agency. The ECJ could afford to move aggressively to revise the treaty on its own, because its formal relationship with the member states is a permissive one. Given the decision-making rule in place (unanimity), the credibility of the threat that the member states would reverse constitutionalization—or any ECJ treaty interpretation—was and remains low.

We now examine the dynamics of integration in two very different areas: the free movement of goods and the Europeanization of social provisions. While we cannot claim a representative sample, the cases selected vary along a number of dimensions, several of which deserve emphasis. The free movement of goods domain constitutes *negative integration*, the removal of national rules that hinder transnational exchange. This domain is a core value of the EC polity and is the most highly developed of European law. The production of EC social provisions constitutes *positive integration*, the elaboration of supranational rules that replace (or fix standards for) national rules governing in a particular area. Such rules regulate exchange among individuals and between individuals and their government. Unlike negative integration processes, wherein the dominance of the ECJ and its case law is virtually total, the competence to make rules in the social area is shared by the court and the EC's legislative institutions (including the Council of Ministers, which generally has the last word). Since the late 1970s, the EC has produced five major directives on equal treatment and nondiscrimination between the sexes in the workplace and in benefits. In negative integration processes the potential for large collective gains from trade is obvious; in positive integration processes, the efficiency logic of integration is greatly reduced. Given these and other important differences, to the extent that the EC legal system operates in invariant or similar ways, our claims are better supported or undermined.

We begin with Article 177 references. Recall that most are triggered by litigants who claim that rules or practices in place in a member state are not in compliance with EC law and request, nevertheless, that a national judge enforce EC law. Activated by these references, the ECJ was able to construct the EC legal system by taking decisions that voided the application of national rules and practices in favor of Euro-rules. Thus, most rulings are rendered in the bright light of clearly revealed preferences on the part of a member state *not* to comply. This dynamic belies, on its face, intergovernmentalist assertions. And, as mentioned above, governments participate directly in Article 177 processes by regularly filing with the court legal briefs defending the legality of their own (or any other national) rule or practice, and by indicating to the court how they believe the dispute ought to be decided. Thus, in advance of any important decision, the ECJ is normally well informed of governmental preferences.

Congruent with our theory, we expect Article 177 litigation to be patterned in predictable ways. In stating these expectations as testable propositions, we further clarify our differences with intergovernmentalists. First, other things being equal, references will target disproportionately those national barriers to transnational activity that hinder access to larger markets relative to smaller markets. The hypothesis can be tested by examining the effect of litigation on negative integration, that is, the removal of barriers to trade and other activity. Second, other things being equal, references will target disproportionately those national rules and practices that operate to downgrade the effect and application of European secondary rules. The legal system will then operate to push the lowest common denominator upward, in a progressive and prointegrative direction, nullifying the lawmaking effects of unanimity voting in the council. The hypothesis can be tested by examining the effect of litigation on positive integration via the production of harmonized European rules and their transposition into national legal regimes. Our general claim is, therefore, that the EC legal system works to dismantle barriers to transnational activity in place in the dominant member states and to ratchet Euro-rules upward from the lowest

TABLE 5. Article 177 References Regarding Free Movement of Goods and Social Provisions: Percentage Difference, Actual Number, and Proportional Share

	Free Movement of Goods			Social Provisions		
Member State	Percentage Difference	Actual Number	Proportional Share	Percentage Difference	Actual Number	Proportional Share
Austria	−.05	0	.4	.55	1	.1
Belgium	−4.89	42	74.8	.22	19	18.36
Denmark	−.57	10	13.8	6.92	15	3.4
France	.23	116	114.3	−12.90	7	28.5
Germany	9.32	265	202.3	−6.28	40	50.3
Greece	−.73	3	7.9	−.58	1	2.0
Ireland	−.38	5	7.5	1.87	5	1.9
Italy	−.82	84	89.4	−7.97	9	22.2
Luxembourg	−.30	5	8.8	−.45	1	2.2
Netherlands	−1.43	86	95.5	2.50	28	23.8
Portugal	.51	7	3.6	−.54	0	.9
Spain	.27	11	9.2	−.77	1	2.3
Sweden	.19	2	.7	−.11	0	.1
UK	−1.34	34	42.9	17.54	40	10.7

Source: Excerpted from Appendix A.
Note: The first column indicates the positive or negative extent to which litigants are attacking the rules of a particular member state in a particular legal domain relative to other member states and other areas. The second column indicates the number of Article 177 references in that legal domain in each member state. The third column indicates the number of references each member state *would have registered* if there were no difference between overall litigation rates for each member state and rates of litigation for each member state in each policy area. For instance, France accounts for slightly less than 17.1% of all references. Thus, France's proportional share of free movement of goods references is 17.1% of the 670 total references in that category, or 114.3. In other words, the proportional share entries are from the table of no association (the basis for the chi-squared test). French courts actually made 116 references in this same legal domain. Therefore, the percentage difference for France is calculated as follows: (116 − 114.3)/670. Entries may differ slightly due to rounding.

common denominator. If we are right, another proposition follows logically: State preferences will not have a significant effect on judicial outcomes. These claims and predictions conflict, fundamentally, with intergovernmentalist expectations.

Table 5 depicts cross-national patterns of litigation in the legal domains of free movement of goods and social provisions (more complete material is contained in Table A-1). The "percentage difference" column provides us with a rough benchmark for evaluating these patterns. The "actual number" column indicates the total number of Article 177 references registered in each of the member states, per legal domain. The "proportional share" column indicates the number of references each state would have generated if there were no difference between overall litigation rates and rates of litigation in specific legal areas. Thus, if a state accounted for 12% of total references, we assigned a proportional share of 12% of the references in each domain to that state (in other words, they are entries from the table of no association, the basis of the chi-squared test). We then subtracted, for each category and for each state, the predicted number of references from the actual number of references and standardized the difference by percentage. Thus, the percentage differences have positive and negative signs. A high positive value indicates that litigants are attacking the rules of a particular country in a particular legal domain relative to other countries and other areas. A negative value indicates that a country is not being dragged to the ECJ as often as we might expect based on overall litigation rates relative to other member states and policy areas.

Note that in the free movement of goods domain, accusations of German noncompliance dominate EC litigation. Of 670 references concerning that domain, 265 (40%) target German laws. This does not mean that Germany has been more protectionist than every other EC member. It does mean that the German market, the largest in the EC, is the prize of free traders. Furthermore, it means that the matrix of trade-relevant rules in place in Germany has provided the predominant context for the ECJ's construction of an integrative case law.

Confidence in our theory is further strengthened by empirical studies of outcomes. Kilroy (1996), in her analysis of free trade cases, assessed the relationship between observations—the briefs filed by the European Commission and governments in pending cases—and the ECJ's rulings. She found that in 81 decisions (two-thirds of her pool), the court struck down national rules as treaty violations; in 41 cases (one-third of her pool), the court upheld national rules as permissible under EC law. She further found that in 98 of 114 cases in which the commission intervened, the court sided with the commission. The commission's position therefore predicted the ECJ's decision 86% of the time. The position of governments utterly failed to predict the court's rulings; German interventions were found to be particularly ineffectual in generating outcomes. Following the logic of Garrett (1992), Kilroy (1996, 23) finds it "surprising that Germany has a relatively lower impact on the Court." But we do not. The EC's supranational institutions—especially the commission and the court—operate to facilitate transnational activity, not to codify or give legal comfort to the preferences of the dominant states.

These numbers tell only part of the story. As impor-

tant has been the positive feedback effect of ECJ decisions on integration processes. The free trade case law, initiated in 1974 with *Dassonville* (ECJ 1974b), sustained the integration project at a time when the legislative process was stalled by disputes among governments and between the Council of Ministers and the commission (Gormley 1985, Oliver 1988). Out of this case law came the famous principle of mutual recognition of national standards, which the ECJ used to help construct the common market. In developing this principle, the court demonstrated how member states might retain their own national rules, capable of being applied to the production and sale of domestic goods within the domestic market, while prohibiting members from applying these same rules to goods originating elsewhere in the community. The ECJ's case law also placed national regulations in "the shadow of the law," raising the specter of relentless litigation against rules that do not comply with the dictates of mutual recognition.

Simplifying a complex chain of events, the adjudication of disputes over free movement of goods between traders and member states triggered a political process by which the commission, in alliance with transnational business coalitions, converted mutual recognition into a general strategy that could be extended beyond free movement. That strategy ultimately resulted in the 1986 Single European Act. The literature on the act has sufficiently demonstrated that governments did not meaningfully control this process (Alter and Meunier-Aitshalia 1994, Dehousse 1994, Sandholtz and Zysman 1989). Instead, governments were forced to adapt to it. Governments did act, of course, in negotiating a treaty that codified prointegration solutions to collective action problems (Moravcsik 1991), including the principle of mutual recognition. But most of these solutions had already emerged out of the structured interactions among transnational actors, the ECJ, and the commission.

We turn now to social provisions, the cluster of treaty rules and directives governing nondiscrimination on the basis of sex in pay and employment benefits. Table 5 shows that litigation originating in the British judiciary has driven the ECJ's docket in this area. Fully 24% (40/167) of all references in this domain have attacked, as inconsistent with EC law, legal rules and administrative practices in the United Kingdom. It is well known that the British government has constituted the crucial veto point in legislative deliberations within the Council of Ministers on social provisions (Pillinger 1992, 85–101). Indeed, since the first directive was adopted by the Council of Ministers in 1975, the British government has not wavered in its intention, publicly declared, to veto any European proposal that would enshrine in EC law any rules not already enacted by Parliament (Kenney 1992). The data show that, in this sector, litigation has disproportionately attacked the national rules and practices that represented the lowest common denominator position on EC secondary legislation adopted by the council.

We then tested our predictions concerning outcomes and the effect of observations made by the commission and the governments in all Article 177 references to the ECJ in the social provisions area, from 1970 (the date of the first reference in this domain) through 1992. Rulings were coded into one of two categories: either the court accepted a national rule or practice as consistent with EC law or declared it to be in violation. Of the 91 judgments that could be unambiguously coded,[12] the ECJ declared violation of EC law in 48, a success rate of 53% for plaintiffs in national courts. The ECJ considered the lawfulness of British practices in 24 rulings, declaring violations in 13. Aggregating results from litigation involving the big three—France, Germany, and the United Kingdom—the court ruled violations in 24 of 41 decisions (59%). We also found, as Kilroy had in the free movement area, that the commission's briefs tracked results far better than did the observations filed by governments. The commission's success rate is a whopping 88%: 73 of its 83 observations predict the direction of the final ruling. The United Kingdom's rate of success was 58% (31 of 54 observations tracked final results). This challenges the intergovernmentalist assertion that the preferences of the most powerful states systematically constrain the ECJ.

The effect of the ECJ's case law on national and supranational policy processes and outcomes in the social domain has been deep and pervasive. A large body of scholarship (e.g., Ellis 1991; Harvey 1990; Kenney 1994, 1996; Pierson 1996; Pillinger 1992; Prechal and Burrows 1990) has documented the extraordinary extent to which the ECJ has used its powers for creative interpretation of EC secondary legislation, like directives, and to ratchet up the lowest common denominator position in the Council of Ministers. Kenney (1992, chapter 3), who has examined the relationship between EC and British sex discrimination law in great detail, shows that by the mid-1980s "the EC [had] eclipsed the British parliament as the arena of innovation" in this respect. Tory governments have been forced by national court decisions to ask Parliament, on successive occasions over the past fifteen years, to amend British statutes to conform to the ECJ's evolving case law.[13] In this area, at least, lowest common denominator bargains struck in the council of Ministers have not stuck. Instead, the court has more or less systematically ratcheted obligations upward, in a prointegration direction.

Finally, we examined the relationship between this case law and the work of the Council of Ministers. We found that the ECJ has used Article 177 references to

[12] We were forced to exclude data from ECJ judgments rendered in 1993 (14 cases); as of June 1997, the European Court Reports for that year were unavailable, having been recalled to correct for errors.

[13] Relative to other European judiciaries, one would expect the British courts to enforce EC law only with great difficulty. The doctrine of parliamentary sovereignty formally prohibits judicial review of legislation, on any grounds, and doctrines governing the resolution of conflicts between treaty law and parliamentary statutes conflict with the ECJ's doctrine of supremacy. Both of these long-lived orthodoxies have been swept aside, in areas governed by EC law, as the British judiciary has incorporated as national law the doctrines of supremacy, direct effect, and indirect effect (Craig 1991, Levitsky 1994).

legislate, by judicial fiat, provisions vetoed in the council. In a wave of cases decided in 1990 and 1991, the ECJ boldly usurped the council's legislative primacy by enacting (as a matter of treaty interpretation) the substance of provisions contained in three different legislative proposals.[14] Each of these proposals had been drafted by the commission, either to extend nondiscrimination provisions to new areas or to enhance the enforceability of Euro-rules on equal treatment in the national courts. And each had been vetoed by the United Kingdom, among other governments, in the council.

Thus, outcomes in both of the legal domains examined are broadly consistent with predictions derived from our theory, but they are inconsistent with intergovernmentalist expectations. Although our analysis does not constitute a definitive test, we are confident that our theory outperforms intergovernmentalist theories more generally.[15] Again, we have not argued that member state governments are irrelevant. It is our contention, however, that they do not meaningfully control integration processes. Existing intergovernmentalist theories of integration may well help us understand the bargaining processes in the Council of Ministers and in European summits. But this bargaining takes place within contexts constructed by processes explained by our theory.

In summary, we believe that the EC legal system operates according to a generalizeable dynamic. Individuals ask national judges to void national rules or practices in favor of EC legal rules within a particular domain of activity. Transnational dispute resolution—the interaction among litigants, national courts, and the ECJ—recasts the law governing that domain of activity and, therefore, the policymaking environment. As new rules are generated and existing rules are reinterpreted, national governments (in whose territory rules or practices are now out of step with EC rules) are placed in an ever longer and darker "shadow of the law." The court's case law provides the commission with the constitutional backing for its own policymaking goals, to the extent that these goals are themselves integrative. And this case law enhances the capacity of individuals to initiate future litigation, by providing potential litigants with more precise information about the content and scope of European law. Thus, in process tracing, we see again the self-sustaining dynamic that we theorized and then found in the quantitative analysis.

CONCLUSION

We have proposed a theory of how a transnational rule-of-law polity may emerge. This emergence, the theory implies, depends critically on the construction of causal linkages among three factors: exchange, triadic dispute resolution, and the production of legal rules. We derived a set of propositions from the theory and tested them in the case of the European Community. We then cross-checked our quantitative results by process-tracing in two discrete areas of EC law. We found broad support for the general theory and for specific claims about how the EC legal system operates.

We emphasized the transformative effect of the ECJ's early case law, which "constitutionalized" the treaty system. Relative to the EC as originally conceived by the member states (and relative to pure intergovernmental fora), constitutionalization made the EC far more responsive to the demands of transnational society—those who exchange across borders. In the EC, individuals can activate transnational adjudication processes on their own. In virtually all other international regimes, individuals must rely on intermediaries, usually representatives of governments, to press their claims and to pursue their other interests. But in the EC, the operation of the legal system has progressively reduced the capacity of national governments to control policy outcomes, while it has enhanced the policy influence of the EC's supranational institutions, national judges, and private actors.

We end by suggesting that our theory may help us understand the evolution of rule-of-law systems more generally. North (1990) has shown us that individual behavior, governmental organizations, and rules often evolve symbiotically, determining a great deal of what is most important about modern economic and political systems. North (1990, 35) has further argued that "impersonal exchange with third party enforcement . . . has been the critical underpinning" of successful modernization and political development. Shapiro (1980) has demonstrated that courts are crucial to regime formation, state-building, and the consolidation of political legitimacy. Judges, because they are agents of normative (rule-oriented) change in rule-of-law societies, possess the broad capacity to configure and reconfigure the polity. Certainly, they have done so in North America (e.g., Horwitz 1977; Russell, Knopff, and Morton 1989; Wolfe 1986), in Europe (e.g., Burley and Mattli 1993; Kommers 1989; Shapiro and Stone 1994; Stone 1992, 1994b; Weiler 1991), and in some international regimes (e.g., Hudec 1993, Stone Sweet 1997b). As we are now experiencing a "global expansion of judicial power" (Tate and Vallinder 1995), we have

[14] The court enacted: (1) in *Barber* (ECJ 1990b), abrogations of those provisions of the Equal Treatment (1976) and Social Security (1979) directives that had permitted member states to derogate from principles of equal treatment in retirement pensions; (2) in *Dekker* (ECJ 1991b), the main provisions of the "pregnancy directive," which were designed to protect pregnant women from discrimination; and (3) in *Hertz* (ECJ 1991c), the core elements of the proposed "burden of proof" directive, which was designed to shift the burden to the member states in cases involving sex discrimination.

[15] Intergovernmentalists tend to conceptualize the activities of the ECJ in terms given by game theory or principal-agent theory. In this imagery, the Eurolaw "game" is dyadic, played by member states (or principals) in one seat and the ECJ (the agent) in another (e.g., Garrett 1992, Kilroy 1996). Such analyses all but ignore two sets of actors—private litigants and national judges—who are crucial to how the legal system functions. Moravcsik (1993, 513) openly admits that his intergovernmentalism cannot explain the construction of the legal system (but see also Moravcsik 1995). It is our contention that the operation of the legal system—highly structured interactions among private litigants, national judges, and the ECJ—is at the very core of European integration processes writ large. Political scientists cannot begin to explain the dynamics of European integration without a coherent account of legal integration.

APPENDIX A. Article 177 References: Actual Number, Proportional Share, and Percentage Difference by Legal Subject Matter

	A	B	C	D	E	F	G	H	I
Austria	**−0.05%**	**−0.05%**	**−0.05%**	**−0.05%**	**−0.05%**	**0.55%**	**−0.05%**	**−0.05%**	**−0.05%**
Actual number	0	0	0	0	0	1	0	0	0
Proportional share	0.4	0.4	0.1	0.3	0.2	0.1	0.2	0.1	0.1
Belgium	**−4.89%**	**−6.83%**	**1.79%**	**−3.22%**	**17.56%**	**0.22%**	**−6.61%**	**5.91%**	**11.47%**
Actual number	42	33	29	5	112	19	2	28	31
Proportional share	74.8	85.1	25.0	7.0	43.5	18.4	4.9	18.3	15.3
Denmark	**−0.57%**	**−0.10%**	**0.17%**	**−2.07%**	**−1.81%**	**6.92%**	**0.21%**	**−1.46%**	**−2.07%**
Actual number	10	15	5	0	1	15	1	1	0
Proportional share	13.8	15.7	4.6	1.3	8.0	3.4	0.9	3.4	2.8
France	**0.23%**	**−3.72%**	**13.72%**	**−1.21%**	**−4.01%**	**−12.90%**	**5.64%**	**0.60%**	**−5.41%**
Actual number	116	102	69	10	51	7	10	29	16
Proportional share	114.3	130.0	38.2	10.7	66.4	28.5	7.5	27.9	23.3
Germany	**9.32%**	**13.54%**	**−11.03%**	**15.80%**	**−5.62%**	**−6.28%**	**−27.96%**	**−14.99%**	**0.43%**
Actual number	265	334	43	29	96	40	1	25	42
Proportional share	202.3	230.0	67.5	18.9	117.6	50.3	13.3	49.4	41.3
Greece	**−0.73%**	**−0.26%**	**−0.29%**	**−1.18%**	**−0.67%**	**−0.58%**	**−1.18%**	**3.70%**	**−0.45%**
Actual number	3	7	2	0	2	1	0	8	1
Proportional share	7.9	8.9	2.6	0.8	4.6	2.0	0.5	1.9	1.6
Ireland	**−0.38%**	**1.23%**	**0.66%**	**−1.13%**	**−0.61%**	**1.87%**	**−1.13%**	**0.09%**	**−0.40%**
Actual number	5	18	4	0	2	5	0	2	1
Proportional share	7.5	8.6	2.5	0.7	4.4	1.9	0.5	1.8	1.5
Italy	**−0.82%**	**1.71%**	**2.27%**	**−0.66%**	**−11.82%**	**−7.97%**	**32.10%**	**1.89%**	**−6.06%**
Actual number	84	115	35	8	6	9	20	25	10
Proportional share	89.4	101.6	29.9	8.4	51.9	22.2	5.9	21.8	18.3
Luxembourg	**−0.30%**	**−0.39%**	**−0.60%**	**−1.05%**	**−0.28%**	**−0.45%**	**−1.05%**	**1.39%**	**1.87%**
Actual number	5	5	1	0	3	1	0	4	4
Proportional share	8.8	10.0	2.9	0.8	5.1	2.2	0.6	2.2	1.8
Netherlands	**−1.43%**	**−2.34%**	**−4.45%**	**0.02%**	**5.73%**	**2.50%**	**1.64%**	**−2.08%**	**−3.32%**
Actual number	86	91	22	9	78	28	7	20	15
Proportional share	95.5	108.6	31.9	8.9	55.5	23.8	6.3	23.3	19.5
Portugal	**0.51%**	**−0.54**	**1.25**	**−0.54**	**−0.28**	**−0.54**	**−0.54**	**0.07%**	**−0.54%**
Actual number	7	0	4	0	1	0	0	1	0
Proportional share	3.6	4.1	1.2	0.3	2.1	0.9	0.2	0.9	0.7
Spain	**0.27%**	**−1.11**	**1.31**	**−1.37**	**−0.09**	**−0.77**	**−1.37**	**4.73%**	**−0.64%**
Actual number	11	2	6	0	5	1	0	10	1
Proportional share	9.2	10.4	3.1	0.9	5.3	2.3	0.6	2.2	1.9
Sweden	**0.19%**	**−0.11%**	**−0.11%**	**−0.11%**	**−0.11%**	**−0.11%**	**−0.11%**	**0.50%**	**−0.11%**
Actual number	2	0	0	0	0	0	0	1	0
Proportional share	0.7	0.8	0.2	0.1	0.4	0.1	0.1	0.2	0.2
United Kingdom	**−1.34%**	**−1.04%**	**−4.63%**	**−3.24%**	**2.05%**	**17.54%**	**0.41%**	**−0.31%**	**5.27%**
Actual number	34	41	4	2	33	40	3	10	16
Proportional share	42.9	48.8	14.3	4.0	24.9	10.7	2.8	10.5	8.8

Note: A = free movement of goods; B = agriculture; C = competition and dumping; D = external policy; E = social security; F = social provisions; G = environment; H = establishment; I = free movement of workers; J = taxes; K = transportation; L = common policy; M = approximation of laws; N = other. Actual number indicates the number of Article 177 references in that legal domain for each member state. Proportional share entries are the number of cases that each member state *would have registered* in each legal domain if there were no difference between overall litigation rates for each member state and rates of litigation for each member state in each policy area. Thus, if Germany accounts for 30% of the references overall, we assigned a proportional share of 30% of the references in each policy area to Germany. In other words proportional share is the "table of no association" (the basis of the chi-squared test). Bold entries are percentage differences, which are calculated the following way: (Actual number − Proportional share)/total number of references in the legal domain. These entries are indicative of the positive or negative extent to which litigants are attacking the rules of a particular member state in a particular domain relative to other member states and other areas.

APPENDIX A. (continued)

J	K	L	M	N
−0.05%	1.42%	−0.05%	−0.05%	−0.05%
0	1	0	0	0
0.1	0.04	0.2	0.1	0.3
−3.13%	7.96%	−8.94%	−0.52%	−0.21%
20	13	1	15	66
27.8	7.6	5.0	16.8	67.3
4.36%	6.76%	0.16%	0.06%	−1.57%
16	6	1	3	3
5.1	1.4	0.9	3.1	12.4
−3.43%	−5.32%	−8.20%	−0.07%	8.95%
34	8	4	24	157
42.4	11.6	7.7	25.7	102.7
−0.91%	−2.29%	9.77%	−9.66%	−11.49%
73	19	18	29	113
75.1	20.5	13.6	45.5	181.8
−0.38%	−1.18%	−1.18%	−1.18%	−1.80%
2	0	0	0	18
2.9	0.8	0.5	1.8	7.1
−1.13%	−1.13%	3.32%	−1.13%	−0.63%
0	0	2	0	3
2.8	0.8	0.5	1.7	6.8
2.30%	−7.48%	4.42%	10.05%	3.56%
39	4	8	33	102
33.2	9.1	6.0	20.1	80.3
−0.64%	0.42%	−1.05%	0.37%	0.94%
1	1	0	2	12
3.3	0.9	0.6	1.9	7.9
3.00%	−2.51%	−0.94%	2.04%	1.65%
43	8	6	23	96
35.4	9.7	6.4	21.5	85.8
0.67%	0.93%	1.69%	0.17%	−0.37%
3	1	1	1	1
1.3	0.4	0.2	0.8	3.2
−0.16%	−1.37%	−1.37%	1.47%	−0.04%
3	0	0	4	8
3.4	0.9	0.6	2.1	8.2
−0.11%	−0.11%	−0.11%	−0.11%	0.06%
0	0	0	0	1
0.3	0.1	0.1	0.2	0.7
−0.39%	3.88%	2.48%	−1.45%	−2.60%
15	7	4	7	23
15.9	4.4	2.9	9.7	38.5

Note: A—See preceeding page.

every reason to theorize more rigorously the political effect of triadic dispute resolution.

APPENDIX B

We tested whether higher levels of references per member state are generated, respectively, by (1) higher levels of diffuse support for the European legal system (measures developed by Caldeira and Gibson 1995), (2) larger populations (aggregated as the average population for each member state, 1961–93), and (3) larger economies (aggregated as the average GDP for each member state, 1961–93). The dependent variable for each model is the average number of Article 177 references per member state, and $n = 11$. Our results are summarized as follows:

	Intercept	t	coefficient	t	Adj. R^2	SEE
Diffuse support	10.08	3.92	19.88	1.18	.04	8.54
Population	6.40	1.55	9.43 (10^{-8})	1.16	.03	8.55
GDP	3.33	1.48	1.48 (10^{-11})	4.30	.63	5.27

The index of diffuse support for the ECJ is from Caldeira and Gibson (1995, 364). Population data are from the World Tables of Economic and Social Indicators (World Bank 1990). GDP data for 1960–69 are from the Cross-National Time-Series Study Banks 1976); and GDP data for 1970–94 are from the World Tables (World Bank 1995).

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Liberal Theory and the Idea of Communist Justice

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My aim is to bring Marx's general theory of exploitation into line with contemporary theories of distributive justice advanced by liberal egalitarians, theories that stress the ideal of equal concern and respect for persons qua *persons and require implementation of the ideal consistent with what historical circumstances permit. I begin by dismissing the labor theory of value as relevant to Marx's general theory, emphasizing instead a "property rights matching power" account. I conclude with some speculative comments about Kantian influences in Marx's sense of justice. Throughout, I assume that Marx believes that exploitation is unjust, and I pursue and sharpen this view in my analysis of Marx's criticism of the distributive principle of socialism.*

"Jeder nach seinen Fähigkeiten, jedem nach seinen Bedürfnissen!" (Marx and Engels [1875] 1962, 21).[1] This is the distributive principle advocated by Marx and his followers for communist society. In this society, there is no market for production or consumption goods, the market having been phased out during the transition period separating capitalism from communism. During this transitional interval, people consent to the gradual substitution of their individual purchase in the marketplace for the collective provision of consumption goods. The substitution would be piecemeal, geared to providing an increasing number of public goods.[2] Beginning with basic necessities, prices would be lowered slowly while the government's exchange branch notes the reaction of demand to price changes. If demand is fairly inelastic and goods nonsubstitutable, then prices can be lowered to zero. The reduction of prices and their eventual disappearance would be paid for by lowering the money wages paid to individuals in exchange for their work, by substituting a social payment for an individual wage. For example, instead of a worker receiving $100 daily, s/he might receive $80, but that person's medical care, education, musical training, access to national parks, bread, fish, and salt would be "free" for the taking. Already in advanced capitalist countries, especially those with strong labor movements (e.g., Sweden), there is a not insignificant public welfare sector, one which "foreshadows, at least potentially, the mode of distribution of the future" (Mandel 1968, 657). Erik Olin Wright thinks that state institutions in social democracies which specialize in the decommodified production of public health, education, public recreation, and so forth, "should be regarded as also constituting elements of an *embryonic postcapitalist mode of production*" (1997, 463). Since under the communist mode all costs would be socialized and all goods would become public, there is a sense in which communism is the ultimate welfare state.[3]

How Marx justifies the move from market distribution to the ultimate welfare state constitutes the topic of my article. Having once sided with the prevalent view that Marx defends the choice of communism over socialism (the transition period with its market in consumption goods) by appealing to communitarian values (DiQuattro 1978), I have now come to encourage an interpretation attributing to Marx a sense of justice that does not differ fundamentally from that found in egalitarian versions of liberal theory. Communism is assuredly beyond markets, but it is not beyond liberal justice, as followers of Hume's criteria of justice have it. Indeed, for Marx, communism is the perfectly just society, and it is from this ideal that Marx works up his theory of exploitation and assesses the status of justice in precommunist societies. In what follows, I elucidate the liberal and individualistic roots of Marxian justice by focusing on the Marxian concept of exploitation. I set out the view that Marx has a general normative theory of exploitation derived from a model of a perfectly just society in which everyone, by virtue of personhood, is deserving of equal welfare. Accordingly, societies that deny this by permitting morally arbitrary factors to determine distributive shares stand condemned as exploitative and unjust. I begin clearing the path of analysis by dismissing the thesis that the labor theory of value, as a technical theory of exploi-

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Some of the ideas in this paper were first presented at the annual meeting of the American Political Science Association, August 1990, San Francisco, and later at the Political Theory Convocation, Department of Political Science, Texas A&M University. I have benefited from comments by John Roemer and Danny Goldstick on earlier versions of this paper. Their disagreements with parts of my analysis prompted elaboration and revision. Thanks also to John Paddy Burke and Lyman Legters of the Douglas Institute.

[1] "From each according to ability, to each according to wants!" I later justify translating "Bedürfnissen" as "wants."

[2] "A *public good* is any good or service which is *de facto* provided for or subsidized through government budget finance" (Birdsall 1965, 235). In this definition, "publicness" is an act of legislative designation, not of any technical characteristics of the goods or services. It is a deliberately broad definition, circumventing narrow criteria of public goods, such as "indivisible *in principle*" or the more ideologically laced "what the market cannot produce" (Markovich and Pynn 1988, 48). In the broad definition, something such as the provision of health care can be either private (divisible and purchased individually in the market) or public (state-rendered indivisible and publicly available) depending on the decisions of the government's exchange branch. The expression "exchange branch" is from Rawls (1971, 282).

[3] The nature of the communist state is for Marx different from that in class-divided societies (Schmitt 1987, 198–200). Furthermore, on the Marxian view, in capitalist economies there are structural limits on how far decommodification can proceed, entertainingly revealed in Wright's "parable of the schmoo" (1997, 4–9).

tation, has an important bearing on Marx's general normative theory of exploitation. Having dispensed with the labor theory, I proceed to delineate and defend a "property rights matching power" account of the general technical theory. And though this account draws on the work of John Roemer, it differs importantly by rejecting both his interpretation of Marx and his game-theoretic approach to exploitation. My aim is to bring Marx's general normative theory into line with contemporary theories of distributive justice advanced by liberal egalitarians, theories that stress the ideal of equal concern and respect for persons *qua* persons and require implementation of the ideal consistent with what historical circumstances permit. I conclude with some speculative comments about Kantian influences in Marx's sense of justice. I should add that I do not start from scratch in my consideration of Marx on the topics of morality, justice, and exploitation. In the debates over whether Marx believes exploitation is unjust, I am on the side that argues he does, and in this article I pursue and sharpen this interpretation, in particular in my analysis of Marx's criticism of the distributive principle of socialism.[4]

LABOR THEORY OF VALUE

If there is such a thing as the general Marxian theory of exploitation, by which I mean a technical and normative theory according to which inequalities generated by any economic system can be explained and ethically evaluated,[5] then it does not matter that the labor theory of value is false. Even the most orthodox Marxists must agree to its irrelevance, since for Marx the labor theory of value finds application only in capitalist economies, whereas the general theory of exploitation covers all economic systems. The labor theory purports to explain the values of and ratios at which commodities exchange in competitive markets, indeed, only in competitive *capitalist* markets, since for Marx only in those markets does labor take on an abstract form.

In the first part of *Capital*, Marx conceptually ties together "abstract labor" and the "value" of commodities, with the theoretical result that commodities possess value only if produced by abstract labor. "A use-value, or useful article, therefore, has value only because human labor in the abstract has been embodied or materialized in it" (Marx [1867] 1965, 38). Since abstract labor is the numeraire commodity and accordingly essential to value calculation, only in capitalism can the labor theory of value do any explanatory work. Furthermore, as indicated below, *abstract labor* and the *value* it produces are not merely logical or theoretical abstractions but, instead, concrete historical phenomena associated with the rise of capitalism.

Previous to capitalism, labor is mainly heterogeneous, employing specific facilities in distinct ways and generating products of determinate physical properties capable of satisfying particular wants. This power to satisfy wants stamps these products with use-value. But because use-values are themselves qualitatively different—precisely because the concrete labors that produce them are—they lack a commonality that renders them comparable for purposes of aggregation. On this account, they are not reducible to exchange-value and cannot constitute the factor according to which exchange-values are determined. There is a problem of aggregation here, analogous to that of the utilitarian's calculation of the interpersonal comparison of utilities. "The *very* fact that these different labour-times, expended in a capitalist economy, are added together means that they are treated as abstract labour-time. (One can no more add 7 hours of *concrete* coal-mining to 3 hours of *concrete* tea-making than one can add 7 apples to 3 oranges)" (Steedman 1977, 19).

In capitalism, labor gets stripped of its concrete particularity and is transformed into a simple, homogeneous, and aggregative form; it is labor as such, all types of labor rolled into one, universal, simple, the kind of labor that Adam Smith incorporated into his analysis. Paul Sweezy (1942, 31) notes that this reduction of labor to the common denominator of simple units is not an arbitrary abstraction forced on reality by the Marxist conceptual framework: "It is, as Lukács correctly observes, an abstraction that belongs to the essence of capitalism." Or as G. A. Cohen puts it, "under capitalism the abstract aspect [of labor] takes precedence *in fact* and in consciousness" (1974, 247, emphasis added).

Just why labor *actually* assumes this distilled form under capitalism has prompted different explanations from Marxist theorists. Does labor take on an abstract form because of the compelling *efficiency* of competitive capital and labor markets? Or is it because a deskilled, interchangeable, and mobile labor force assists managers in *controlling* workers in capitalist enterprises? These explanations are not always complementary and may be contradictory, as reflected in the tension implicit in Adam Smith's ([1776] 1982, chapter 1) description of the debilitated state of workers in his famous pin factory. Even in the face of his praise of the efficiency aspect of abstract labor, he called attention to the alienated, stunted human beings whose creative capacities and entrepreneurial talents were lost forever to productive endeavor. Unlike Smith, Marx had at his disposal a theory that accounted for the "contradiction." To paraphrase Marx ([1867] 1965, 486–8) and Engels ([1878] 1947, 441–4), "modern industry" (which develops the forces of production) compels the replacement of the detail worker, reduced to a fragment of a person by the division of labor, by the fully developed, multifaceted individual, adapted to a variety of social functions that gives free scope to his or her natural and acquired powers; but the capitalist character of modern industry, its relations of production

[4] One of the best discussions of these debates is in Peffer (1990, chapters 4–7).

[5] Following John Roemer, I distinguish throughout this paper between "*technical exploitation*" and "*ethical exploitation*." I am concerned with both. The former is a positive theory that explains how inequalities come about; the latter is a normative theory that morally assesses these inequalities (e.g., as just or unjust) and how they come about (e.g., justly or unjustly). See Roemer 1988, chapter 2.

(which sustain class power), generate a compliant, unifaceted, subdivided, and "Taylorist" work force. This results in the "contradiction" that reproduction of class power gets in the way of the development of the forces of production, in particular, technology and the all-around development of human labor.

At any rate, on whichever side of the control-versus-efficiency debate the weight of argument leans (see Marglin 1976), Marx is of the view that abstract labor prevails only in capitalist market economies. "The abstract category 'labor,' 'labor as such,' labor *sans phrase*, the point of departure of modern economics, thus becomes a practical fact only there [the United States, the most modern form of bourgeois society]" (Marx [1859] 1970, 210). This means that the labor theory of value and its corollary, the theory of surplus value, cannot be an essential part of Marx's general theory of technical exploitation. The labor theory explanation of allocation and distribution is limited to capitalism. The labor theory, for example, does not explain allocation and distribution in slavery or feudalism. In these societies, labor is still concrete and produces mainly use-values.

To say that abstract labor, and therefore value, prevail only in capitalism implies that the labor theory does not apply to simple commodity production, where independent producers, owning their own means of production, engage in market exchange. To some students of Marxism (e.g., Meek 1977, chapter 7) this is a controversial claim, since it seems to them that Marx, in the first part of *Capital* and after the manner of Adam Smith, initially employs the labor theory to explain exchange in simple commodity production, and only after asserting its validity there does he move on to apply it to capitalism proper. But this reading of Marx is off because the labor theory requires the existence of competitive capital and labor markets without which there would be nothing to compel the transformation of concrete labor into an abstract form and, as we have seen, minus abstract labor, there is no value, no law of value, and therefore nothing for the labor theory of value to explain. It suffices to refute the claim that the labor theory applies to simple commodity production to point out the absence of abstract labor there. When this is combined with a key element in Marx's theory—that abstract-labor markets emerge through pressure and compulsion and are sustained only by competition in capital markets—we end up with a tight account of Marxist theory that looks like this:

> Under simple commodity production the labourers are united by ownership with their means of production. As a result there is no free and unconstrained labour mobility from industry to industry, and no labour market. Hence there is no force which brings the labourers' rates of remuneration into equality. Such equality is only possible with private ownership when the link between labour and its means of production is severed. This tallies with an argument . . . that the orthodox Marxian notion of "abstract labour" is only applicable to capitalism. Hence no homogeneous measure of labour time is possible under simple commodity production. The category of abstract labour is a reflection, according to Marx, of a situation under capitalism where all forms of labour are united by the fact that they are exchanged on a market. Abstract labour, thus defined, has no material basis under simple commodity production (Hodgson 1981, 106; also see Morishima and Catephores 1978, chapter 7).[6]

POWER AND MATCHING PROPERTY RIGHTS

If the labor theory of value does not matter to the general theory, then what does? I argue that property rights do. In this respect, my argument resembles that of John Roemer, but it deviates insofar as it considers Marxian exploitation theory interesting for the same reason that property rights theory is.[7] These theories are one and the same. My rendition of property rights theory stresses the role of power and the property rights matching it, which implies that the analytical Marxists are on to something in saying that "the Marxist condemnation of the injustice of capitalism is not so different from the conclusion that other and apparently less radical and contemporary theories of political philosophy reach, albeit in language less flamboyant than Marxism's" (Roemer 1988, 5). In particular, I argue that Marx and liberal theorists such as John Rawls are the same in their ruling out, on the grounds of justice, the role of morally arbitrary influences in the determination of distributive shares. By way of getting there, I start with a tale spun by Milton Friedman (1962, 162, 165), which I embellish as follows.

Imagine four Crusoes shipwrecked on the high seas, with each happening to land on one of four proximate

[6] John Weeks (1981) traces the idea that the labor theory explains exchange in simple commodity production to Engels and accounts for its popularity in light of its accession to authority in Soviet Marxism. His exegesis shows that, for Marx, "value systematically rules exchange only under capitalist relations of production and in no other system, historical or hypothetical" (p. 28). I think that empirical evidence backs up Hodgson's and Weeks's reading of the labor theory. For example, colonial North America resembled a simple market economy—one without significant capital and labor markets—and there the prices of exchanged commodities were not largely market driven. Prices were largely set by custom and prevailing (often religious) conceptions of justice, not by what the market would bear. Prices or "exchange values" of *this* kind can hardly be explained by the labor theory (see Henretta 1991; Kulikoff 1992). Marx ([1867] 1965, 765ff) himself characterized the economy of the American colonies as simple commodity production.

[7] Roemer thinks that Marx's general theory of technical exploitation is uninteresting because it relies on the labor theory of value. Throughout his work, Roemer contrasts his property relations definition of exploitation to that of Marx's labor theory, referring to and "discarding" the latter as the "classic" or "traditional" definition of Marxist exploitation (1988, 11–2). Roemer (1994a, 97) considers an "unequal exchange of labor" definition of exploitation to be a "generalization of Marx's," and he writes that "capitalist exploitation turns out to be equivalent to Marxian exploitation, when labor is homogeneous and equally endowed to all" (1982, 29). My view differs from Roemer's in asserting that property relations theory *is* Marx's general technical theory of exploitation. Perhaps what leads to Roemer's mistake is his belief that the labor theory applies to noncapitalist economies. He thinks, for example, that socially necessary labor time, as Marx conceives of it, exists not only in simple market economies but also in autarkic economies which lack exchange among producers (Roemer 1988, 16). For reasons I have already provided, this cannot be the case, since the fact of abstract labor, which is peculiar to capitalism, is a logical precondition of the existence of socially necessary labor.

islands, only one of which is rich in resources and the rest barren and inhospitable. The islands, previously unowned by anyone, fall prey to ownership claims, with the result that the lucky Crusoe, *X*, holds the rich property. Friedman implies the moral legitimacy of this distribution by appealing to the values of liberty and consent, which he drives home by analogy to lotteries. Unless everyone initially agreed (hypothetically, no doubt) to leave it to happenstance to decide distributive outcomes, everyone would be denied the opportunity to win the jackpot prize. Robert Nozick (1974) would place more emphasis on natural rights and their origin in being in the right (previously unowned) place at the right time (no one else got there first). But in spite of their different emphases, both Friedman and Nozick agree that brute luck can be ethically decisive in deciding who gets what, in this case effecting a configuration of property that advantages *X* relative to the other Crusoes. Continuing the tale, suppose that *X* expends some of his abundant resources to employ the services of individuals on subsequently discovered adjacent Pinkerton Island. The inhabitants of this island are a soldierly lot and especially suited to policing *X*'s productive assets.[8] The Pinkertons' initial task is to prevent incursions upon *X*'s assets by the unlucky islanders, but their work extends to enforcing contracts. Since the unfortunate Crusoes can barely scratch out a living on their desolate properties, they bite at the Pareto-optimal offer to exchange their labor power for access to *X*'s island. For every five deer they jointly hunt and catch, they get to keep three, the remainder go to *X*, who gets two deer in comparison to their one each.

Two points need to be made at this point in the story, one bearing on the labor theory of value, the other on power and choice. First, we assume that though hardly any labor goes into their catch, deer are nonetheless highly valued because of their scarcity and utility on the poorer islands. Indeed, it costs twice the labor to catch beavers, which are abundant on the poor islands, but they have less value than deer because they yield less utility. Labor time does not determine the value of deer, whether it is the labor embodied at the time of the catch or the socially necessary labor time required at the time the catch is exchanged at the end of the week, when the workers give *X* five deer and get three back.[9] What determines the value of the deer is their scarcity and utility, and what gets *X* his share of this value are the terms of the agreement negotiated with the other Crusoes. Is this a case of Marxian exploitation? The workers have hunted down what has value, even though their labor did not create that value. But must not the charge of ethical exploitation still hold for Marxists because *X* appropriates some of the value "even if wholly determined by the extent and intensity of desire for it" solely on the basis of his ownership of scarce and useful resources (Cohen 1980)? Even if we worked up this illustration into a full-blown capitalist market system, complete with competing capitals and capitalist supervision of work, the labor theory could be false, and the charge of exploitation still convincing.

This fundamental point receives sophisticated and mathematical treatment in the various models Roemer constructs to illuminate how Marx's normative concerns need not depend upon the labor theory of value. Roemer shows how all sorts of arrangements, developed in stories like the one above, count as ethically exploitative even in the absence of the unequal exchange of labor, which he (wrongly) takes to be at the heart of Marx's general technical exploitation theory. Leaving the mathematical models aside, consider once again the Crusoe fable. Instead of *X* hiring out the other Crusoes, he lends them some deer to breed on their islands. He charges interest in the form of a cut of the deer's offspring. Assume, further, that deer-breeding involves no labor on the part of the Crusoes and that all of them, including *X*, work the same amount of socially necessary labor time in catching deer. But *X* ends up with more deer than he catches. Not because he works less than is socially necessary to catch deer, but because he owns scarce resources that he has lent out at interest.[10] Labor has hired capital, and, as a consequence, technical exploitation emerges through the credit market. Moreover, Roemer shows in another of his models that technical exploitation can result without either labor or credit markets. It can also result from initially unequal resource distribution plus trade (as Marx understood; see Elster 1985, 181–2). In the Crusoes scenario, the trade advantage would accrue to *X* since he owns the more valued deer; in exchanging them for the less valued beaver, he would get the better of the deal even though, let us suppose, it costs twice the labor to catch beaver than deer.

Oskar Lange, one of the pioneers in meshing neoclassical formulations with Marxian exploitation theory, more than fifty years ago captured the basic idea underlying this kind of analysis of exploitation. He stressed as the distinguishing feature of Marxian economics its emphasis on the "institutional datum" or class relations of society and pointed out the lack of advantage the labor theory has over the property relations account of technical exploitation.

> Thus the labor theory of value has no qualities which would make it, from the Marxist point of view, superior to the modern more elaborate theory of economic equilibrium.... In the Marxian system the labor theory of value serves also to demonstrate the exploitation of the working class under capitalism.... But the same fact of exploitation can also be deduced without the help of the labor theory of value.... If interest is explained by the marginal

[8] I leave aside the whole issue of why the Pinkertons contract with *X* rather than simply take his island. As far as the point of this tale is concerned, we could just as well have imagined that the shipwrecked Crusoes, having landed on the poor islands, found themselves adjacent to the rich island already occupied by the Pinkertons.

[9] I am not saying that this part of the story captures what goes on between capitalists and workers in a capitalist market system. There, workers get paid wages for laboring a set time. This allows the bosses to keep as many deer as the workers can be pressured to catch, while keeping their wages down to the contractual limit of three deer.

[10] The deer are scarce relative to the demand of the poor Crusoes. They are not scarce relative to the demand of *X*. In this case, scarcity is created by the system of property rights, and *X* undergoes no disutility or "waiting" by giving credit to the poor Crusoes.

> productivity theory of capital, it is only because the workers do not own the capital they work with that interest is the personal income of a separate class of people. If interest is regarded as due to a higher valuation of present rather than future goods it is only because the workers do not possess the subsistence fund enabling them to wait until the commodities they produce are ready that the capitalist advancing it to the workers gets the interest as his personal income. Just as in Marx's case it is because the workers do not possess the means of production that surplus value is pocketed by the capitalist (Lange [1935] 1968, 77–8).

If none of this persuades about the irrelevancy of the labor theory, assume that instead of *X* contracting with the other Crusoes he forces them to work for him, as in feudalism. This is the clincher in the argument that the labor theory is dispensable to Marxian exploitation theory. Serfs do not produce value, according to Marx (for the reasons covered in the first part of my article); yet, as Cohen (1980, 155) says, "if anything, [feudalism] is the *paradigm* of exploitation in Marxism." *X* uses the Pinkertons (in feudalism, many of the *X*'s *are* Pinkertons) to compel directly the other Crusoes to catch deer so many days a week, giving over to *X* as many deer as required by political command and consistent with the minimal consumption needs of the serfs. Here use-value reigns supreme; there is no value, no place for socially necessary labor (by definition, the quantity of abstract labor required at the time of market exchange), and hence no way of explaining technical exploitation by the labor theory. Still, Marx dubs feudalism as exploitative. So how can his general theory of exploitation consist in, or be based on, the labor theory?

True enough, Marx explains how inequality comes about in feudalism by noting that serfs produce a surplus product from the labor they expend beyond what goes to themselves. But this talk about necessary and surplus labor does not imply that Marx's explanation is based on the labor theory of value. The serf's surplus product does not possess surplus value; if anything it possesses surplus *use-value*. Since, in feudalism, it is impossible to say that serfs produce value and to measure their socially necessary labor time, it cannot be said that lords end up with the surplus-value generated by the socially necessary labor of the serfs. Inequality cannot be explained in terms of the unequal exchange of labor, as in the labor theory, since that entails the aggregation of abstract labor. Lords do end up with appropriating part of the use-value of products, but in feudalism use-value is not (cannot be) calculated in terms of abstract labor time. What the lord gets is an amount of use-value that is determined by a combination of custom and coercion; that is, property rights backed by power. Cohen is right that feudalism, not capitalism, is the paradigm of exploitation in Marx. It is because capitalism is like feudalism that the former is exploitative. Roemer puts things backward in portraying capitalist exploitation as the model of Marxian exploitation theory ("capitalist exploitation turns out to be equivalent to Marxian exploitation"; see footnote 7 above), and this is why he fails to see that the Marxian theory of exploitation is interesting for the same reason that property relations theory is.

Consider, finally, this twist in the story, which leads into the promised consideration of power and choice in Marxian exploitation theory. The asset-poor Crusoes overthrow *X* and his goons, redistribute property, and end up with an equal share of collectively held alienable productive assets. Socialist property rights supplant feudal and capitalist ones (whether by "skipping stages" need not concern us), and now all the Crusoes have free access to hunting deer. Now according to a certain definition of exploitation (e.g., Reiman 1987), which is supposed to elicit an understanding of Marxian exploitation, socialism and exploitation are mutually exclusive. This is because (1) there are no classes in socialism, and (2) workers control the workplace in socialism. This account includes *classes* as a necessary condition of Marxian exploitation: A society is exploitative when unpaid labor is forced out of one class and put at the disposal of another. It also insists that this surplus is squeezed out of workers at the point of production. For example, in capitalism the bosses organize the workplace to maximize the production of surplus value; to make the most of the labor power the worker has sold to the capitalist for a set time.

But how can there be an affinity between this definition and Marx's if Marx intimates that socialism itself involves exploitation? In the *Critique of the Gotha Programme*, Marx expresses dissatisfaction with socialist distribution—to each according to work—despite the absence of classes and coercion in the workplace. Marx thinks there is something ethically amiss here because, even though everyone has property rights in alienable means of production (deer, in my example) and the principle of distribution "recognizes no class difference since everyone is only a worker like everyone else," some workers get larger consumption bundles than others irrespective of the concern for equal welfare ([1875] 1955, 24). Marx is bothered that "one will be richer than another" (p. 24), signifying that an unequal distribution of welfare, even when it does not result in class division (as Marx defines it), is of normative importance to Marx. That this is so tips us off that the core of Marx's general theory of exploitation no more requires classes than it does workplace domination or the labor theory of value.

For Marx, a problem arises in socialism because nonalienable assets (e.g., hunting talents and skills) are unequally distributed among the workers so that those Crusoes with superior endowments are able to get more deer, either as a consequence of working longer (due to their physical endurance) or working less but catching more deer (thanks to talent, cleverness, etc.). Since the main distributive standard is "to each according to duration and intensity" of labor performed, individuals who are "superior to others physically and mentally" can cash in on their fortuitous endowments or "natural privileges" (Marx [1875] 1955, 24). The "right" to a greater income stems from a property right in human capital, which is unequally distributed. Differential power to produce relatively more of those things geared for production by the social plan is thus

given an important place in deciding distributive outcomes.

Notice that, for Marx, in socialism *an unequal exchange of labor is not a necessary condition for exploitation to occur*. For example, *A* and *B*, engaged in joint production, may work the same amount of labor time and receive equal wages, but *B* may be prone to greater fatigue or have greater wants than *A* that cannot, moreover, be satisfied by the limited amount or kind of public goods available in the decommodified sector. Marx talks about *B*'s being married, having more children, and "so on and so forth" than *A*, so that *B*'s wants (quantitatively or qualitatively understood) cannot be satisfied by the expenditure of income generated by labor ([1875] 1955, 24). The distributive standard of socialism allows *A*, who benefits from *B*'s participation in joint production, to exploit *B* by denying *B* want satisfaction equal to *A*'s. This is defective from the standpoint of Marxist justice. It is this moral defect that prompts Marx to refer to the distributive standard of socialism as "stigmatized by a bourgeois limitation," a not unsurprising characterization, since the transition period contains both bourgeois muck and communist promise ([1875] 1955, 23). As we shall see, which way this transition tilts turns on the competition between contending ideas of justice.

For Marx, the most important determinant of distributive outcomes in capitalism is class structure and its matching system of property rights. Class relations are power relations, and they find expression in property institutions. "Find expression in" is an obscure Marxist way of saying that property rights are legal relations tied contingently to power relations. Cohen (1978, chapter 4) reads Marx as treating property rights as a legal category connected functionally to power relations, with property rights "matching" power relations in a way that the latter explain the former. In all class-divided societies, one class holds the means of production; another class, lacking free access to these means, is under a combination of political and economic pressure to surrender a part of the social product to the owning class. But *class* power is not the only determinant of distributive inequality, especially in capitalism, whose system of property rights matches both class power *and* the power of individuals generally. That Marx understood this is indicated in his criticism of "crude communism" in the *Manuscripts*, one which, as Shlomo Avineri (1968, 225–6) says, "closely resembles the account of socialism given in the *Critique*." In both places, Marx focuses on *individuals*, who during the transition period suffer the consequences of left-over bourgeois norms and practices.

Though there is a difference in emphasis—in the *Manuscripts* Marx attacks wage equality because it results in invidious comparisons among individuals, and in the *Critique* he criticizes wage inequality because it is unfair—Marx consistently condemns the infectious bourgeois strains of social alienation and welfare inequality that carry over to individuals in socialism. Marx is keenly aware that capitalism differs from other social formations by releasing individuals from previous customary and political restraints on egoistic behavior, with the market providing the procedure wherein individuals can and *must* use their power to bargain for advantage. The market, as a *distributive* device, forces among individuals a self-interested rationality geared to the competitive struggle for existence. It provides no insurance that those who lose out in the competitive game will be compensated for their loss, and it leaves its players no choice but to join in the fray in the hope of furthering advantages or maintaining positions. Indeed, as Hobbes knew so well about the competitive game, the maintenance of position translates into the seeking of advantage over others, such that even merely self-interested persons graduate to egoism under the pressure of unrestrained competition (see Macpherson 1962, chapter 2).

What is involved here is ironically exposed by libertarian defenders of capitalism. Friedrich Hayek (1976, 94) writes: "Our only moral title to what the market gives us we have earned by submitting to those rules which make the formation of the market order possible." The rules are those of the game of "catallaxy," which "spontaneously" yield the right moral result, a result dubbed "just" precisely because it is the outcome of people's acting within the market rules of the law of property, tort, and contract. Like most games, the outcome results from "a contest played according to rules and decided by superior skill, strength, or good fortune" (Hayek 1976, 115), which to Milton Friedman reduces to the latter. For Friedman, despite the lip service that we all pay to "merit" as compared to "chance," market distribution is akin to a game or lottery that everyone agrees to enter with the anticipation of winning a jackpot prize. "Most differences of status or position or wealth can be regarded as the product of chance at a far enough remove," so that it is luck which determines largely who gets (and should get) what in capitalist market society (Friedman 1962, 165–6). If one is fortunate enough to be born into a wealthy family,[11] or to be born in a part of the world rich in natural resources,[12] or to have certain naturally or socially acquired characteristics that fetch high market prices,[13] or to make profitable investments under conditions of uncertainty,[14] then one qualifies

[11] For example, 67% of the large fortunes in the United States are derived from inheritance (Brittain 1978).

[12] Consider, for example, how the natural animal resource distribution hundreds of years ago affected the consequent world distribution of economic and political power. Successful mammal candidates for domestication—the cow, sheep, goat, pig, and horse—were found almost exclusively in Eurasia, giving its population long-lasting advantages over human populations on other continents (especially sub-Saharan Africa). These animals provided meat, fertilizer, wool, hides, milk, labor, and means of transport prior to the Industrial Revolution. And in war, there is the standout example of Eurasia's horses in the European conquest of the Aztec and Inca empires, not to mention the even more devastating infectious diseases carried by imported domesticated animals that decimated non-Eurasian peoples lacking prior exposure. See Diamond 1994, 1997.

[13] On the multitudinous and subtle ways that luck significantly influences social mobility in market societies such as the United States, see Jencks 1979.

[14] Christman (1994, 89–95) makes a good case that the entrepreneur is undeserving of profit because it results from imperfect market conditions for which s/he is not responsible.

for disproportionate rewards. Coming to the marketplace with whatever resources or factors of production they "happen" to own, individuals engage in exchange and are aware that the initial distribution of property rights affects the outcome. The bourgeois principle—"To each according to what he and the instruments he owns produces"—calls on individuals to accept the resulting distributive configuration as "just" (Friedman 1962, 161–2). Justice is here reduced to a market concept, far removed from any nonmarket standards identified in "substantive" (Hayek) or "patterned" (Nozick) terms conforming to overarching social ends or purposes. Justice is the outcome of the pure procedure peculiar to the market. It describes the result of market competition and sanctions the successes of the lucky, the cunning, and the powerful.

Thinkers such as Hayek, Friedman, and Nozick are embarrassed by this procedural result, prompting their convoluted treatment of the theory and practice of liberty. Friedman (1962, 14) conflates choice with liberty, asserting that all choices in market capitalism are "strictly voluntary." Nozick (1974, 262–4), the more sophisticated of the bunch, redefines liberty so that its presence finds detection paradigmatically only in exchanges that take place in the context of capitalist property relations. For Nozick, by definition, owners of "properly-acquired" property do not possess any economic power and cannot logically force anyone to come to terms.[15] And for Hayek (1976, chapter 10), since coercion implies personal intention and responsibility, there can be none exercised in the marketplace, which coordinates things in impersonal fashion and with unforeseen consequences.

Frank Knight, Friedman's mentor,[16] knew that moves like these are all a sham. In "The Ethics of Competition," one of the few honest defenses of capitalism I have ever seen, Knight goes down the list of the fashionable ethical defenses of capitalism and finds all of them wanting. Capitalist markets systematically reward luck, not moral desert; they work to undermine the virtues of friendship and moral excellence and, most important, substitute for them the "ethics of power."

> Everyone is compelled to play the economic game and be judged by his success in playing it.... The luck element is so large—far larger than fairly successful participants in the game will ever admit—that capacity and effort may count for nothing. And this luck element works cumulatively, as in gambling games generally.... Differences in the capacity to play the game are inordinately great from one person to another. But as the game is organized, the weak contestants are thrown into competition with the strong in one grand melee; there is no classification of the participants or distribution of handicaps such as always recognized to be necessary to sportsmanship where unevenly matched contestants are to meet. In fact the situation is worse still; there are handicaps, but as we have seen, they are distributed to the advantage of the strong rather than the weak.... It is in terms of power, then, if at all, that competitive economies and the competitive view of life for which it must be largely accountable are to be justified. Whether we are to regard them as justified at all depends upon whether we are willing to accept an ethics of power as the basis of our world view (Knight 1935, 64–5, 68).[17]

The role that power plays in assigning distributive shares in capitalism is an unwanted legacy bequeathed to socialism, one of the "birth pangs of the old society from whose womb it emerges" ([1875] 1955, 23). It is the basis of a distributive standard and practice based on "bourgeois right," as we have seen Marx put it, or what Rawls (1971, 65–83) calls the systems of natural liberty and liberal equality, according to which individuals are forced to compete for outcomes with whatever resources they happen to have. "Natural liberty" requires only formal equality of opportunity whereas "liberal equality," the more meritocratic, concedes the need for fair equality of opportunity, yet both permit the distribution of goods according to natural and social contingencies. The initial distribution of naturally or socially acquired powers, when mediated by the distributive standard of socialism, yields differential outcomes. The legitimizing principle is "equality of opportunity," both formal and fair, to secure advantage.

What is wrong with socialism, then, is that it permits universal exploitation among individuals. Its distributional principle justifies the use of power to gain advantage with the consequence that welfare gets distributed not according to wants possessed by persons qua persons but persons as possessors of differential power, and this constitutes a morally arbitrary basis for the distribution of welfare. Marx rarely says explicitly, as in the quotation which concludes this section, such a Kantian-sounding thing, but it is, I venture, the plausible interpretation of his theory of ethical exploitation and the sense of justice underlying it. Marx is for everyone having his or her wants satisfied equally. Welfare equality is the aim of communism, Marx's good society in which the productive forces

[15] Nozick writes: "A person's choice among differing degrees of unpalatable alternatives is not rendered nonvoluntary by the fact that others voluntarily chose and acted *within their [capitalist property] rights* in a way that did not provide him with a more palatable alternative ... [Whether other people's option-closing] action makes one's resulting action nonvoluntary depends on whether these others *had the right* to act as they did" (1974, 262–4, emphasis added). Cohen (1991, 226), reviewing Nozick's argument, observes that "it should be clear that 'libertarian' capitalism sacrifices liberty to capitalism, a truth its advocates are able to deny only because they are prepared to abuse the language of freedom."

[16] Friedman studied under Knight at the University of Chicago's Department of Economics.

[17] The thrust of Knight's essay is that this justification does not mesh with the ethical point of view. "Thus we appear to search in vain for any really ethical basis of approval for competition as a basis for an ideal type of human relations, or as a motive to action. It fails to harmonize with either the Pagan [Greek] ideal of society as a community of friends or the Christian ideal of spiritual fellowship. Its only justification is that it is effective in getting things done" (1935, 74). When this point is added to Knight's view that alternative economic systems (e.g., socialism) fare even worse with respect to liberty and the ideals he mentions, we have his defense of market capitalism. It is the least worse of all possible worlds, surely a less flashy defense than we find in Nozick, though socialists would insist that Knight's opposition to socialism rests on an honest mistake.

have been developed sufficiently to accomplish this distributive ambition.

Welfare equality differs from resource equality. That Marx is a welfare egalitarian shows up in his rejection of wage equality (*Manuscripts*) and in his strategy of enlarging the public goods sector to achieve communism (*Manifesto* and *Critique of the Gotha Programme*). This contrasts Marxist egalitarianism to that of theorists such as Rawls and Dworkin (1981a and b), who advocate an egalitarian distribution of resources (e.g., wealth and income) and leave it up to individuals to use or expend most of them in a market setting.[18] The Rawls-Dworkin approach attracts some egalitarians because it is inspired by a concern with individual choice and responsibility, but its weakness has been underscored by other egalitarian theorists (e.g., Cohen 1989), who argue convincingly that resources are often a bad proxy for welfare. They ask: Is it not welfare that resources are supposed to provide, why resources are important? The telling cases are those of individuals who suffer severe disabilities or who have expensive tastes, neither of whose deprivations are adequately addressed by equal distribution of resources. Clearly, Marxists side with the strategy that best accords with egalitarian intuitions. But if so, then what kind of welfarist is Marx, assuming that welfare translates into preference satisfaction?

Marx's criticism of Bentham for confusing the preferences and utilities of the English shopkeeper with those of people in other classes and different historical periods (Marx [1867] 1965, 609) and Marx's theory of false consciousness, which distinguishes between subjective preferences and objective interests, demonstrate that though he begins his analysis with people's extant, subjective preferences, he does not end there. As Buchanan interprets him, Marx distinguishes between distorted and undistorted preferences or desires, describing communism as a unique form of social life because of the absence of the malformed desires that go with the roles people occupy in class-divided societies. It is from the standpoint of the preferences that people would have or would develop in communism that preferences prevailing in precommunist societies receive ethical evaluation (Buchanan 1982, 27–31).[19] I will say more about this later, given its relevance in securing to Marx liberal credentials, but for now it will do to say that Marx is interested—in his ethical theory—in that kind of welfare involving ideally considered preferences, what Arneson (1990, 163) calls "hypothetical rational preferences" and what Cohen (1989, 909n) includes as belonging to a type of "objective theory of welfare." Notice that treating wants in this counterfactual way conflates wants with needs, rendering it conceptually easy to say that wants, "as corrected by full information and considered reasonable judgment" (Arneson 1987, 527n), mean nothing less or more than needs, the basic intuition being that what a person needs is what s/he would ideally prefer. Arneson (1990, 191) writes: "The conviction that mere preferences [ideally considered] are analytically distinguishable from true human needs may prove to be illusory."[20] This is similar to the view of Marcuse (1964, 4–6), nicely condensed by Miller (1976, 129–30): "Marcuse is claiming that under conditions of freedom individuals' preferences are to be taken as defining their needs, but not otherwise."

But even if one resists this conceptual compression, as does Braybrooke (1987), it seems both lexicographically and theoretically warranted to read Marx, in *Critique of the Gotha Programme*, as talking about wants, not needs, which is what Braybrooke himself asserts. He notes that *Bedürfnisse* expresses variably the concepts "want" or "need," and that when Marx uses the word he is frequently translated as meaning "want" (e.g., he defines use-value as the power to satisfy wants; and there is the famous reference to labor as "life's prime want"). Braybrooke thinks that the best way to fill in Marx's communist principle of distribution—"to each according to"—is with *wants* because needs fail to gel well with Marx's theoretical emphasis on the infinitely expansible desires made possible (if not driven) by the development of the forces of production, which reaches its apex under communism. Because these desires are so Promethean, it is likely that Marx was not thinking of needs "strictly speaking, derived or otherwise," but of wants "whether or not the wants answered to needs" (Braybrooke 1987, 239). Or as Ignatieff (1985, 125) puts it: "The task of human progress [for Marx] was to increase the productivity of labour so that all human beings could be freed from the slavery of basic need to embrace the free choice of desire." It is an added benefit of this interpretation that it comports with the mature (post-*Manuscripts*) Marx's rejection of strong talk about a universal human nature and with his dismissal of appeals to "truly human nature" as a way of grounding the ethical worth of communism. An objective theory of welfare differs from more traditional—nonliberal and perfectionist—

[18] Rawls and Dworkin allow for public provision to accommodate urgent wants, but their main distributive device is the market. Rawls makes it difficult for the government's exchange branch to trade private for public goods if the basic institutions are already ordered according to his two principles of justice. Following Wicksell, he requires a near-unanimous vote to accomplish the trade. For criticism of Rawls on this point, see Krouse and McPherson 1988.

[19] Buchanan (1982, 54) thinks that Marx's theory "admits of either an objectivist or subjectivist, perfectionist or welfarist reading [and that] Marx neither specifically addresses this question nor writes anything that definitively commits him to one or another response to it." Allen (1973) argues that the texts favor a welfare reading. If Marx is a welfarist, more needs to be said about (1) how justice rules out certain kinds of welfare and (2) how far a welfarist is committed to the satisfaction of expensive preferences in the face of scarcity of resources. Marx, I think, handles the first satisfactorily but has little to say about the second, perhaps because he assumes a lack of scarcity in communism. The analysis of contemporary egalitarian theorists who are not as optimistic as Marx on the scarcity issue, yet who are sympathetic to Marx, is highly useful (e.g., Cohen 1989). Schwartz (1995) thinks Marx is *dangerously* utopian on the scarcity issue.

[20] The conceptual tie between wants and needs is analogous to the conceptual compression of informed preferences and interests, as found in Mansbridge 1983 and Goldstick 1994. For more on the objective theory of welfare, see Railton 1986 and Brandt 1979. Wood (1990, 33), who disapproves of this theory, names it "idealized preference naturalism."

theories that rest heavily for their justification upon thick claims about needs peculiar to an unchanging human nature.

Leaving behind this excursus on Marx and welfare theory, and returning to Marx's criticism of socialism, I conclude this section with the reminder that for Marx socialism gives way to communism through the abolition of (alienated and "necessary") labor and the enlargement of the public goods sector. Communism is the "realm of freedom," the freedom for all *to* satisfy freely developed wants and the freedom *from* a system of exploitation that allows some to realize their wants only at the expense of others. "In the place of the old bourgeois society . . . we shall have an association in which the free development of each is the condition for the free development of all" (Marx [1848] 1962, 54).

COMMUNIST JUSTICE

Is communism also the realm of justice? My argument suggests an affirmative response. I want to say, in agreement with Elster (1985) and taking issue with Lukes (1985) and others, that Marx has a "hierarchical" or ideal theory of justice according to which an equal distribution of welfare counts as a perfectly just distribution.[21] Marx subscribes to an ideal theory that determines what is the nearest thing to full justice in less than favorable historical circumstances, so that historically prior distributive principles provide second-best criteria of justice. Unless we read Marx in this way, we cannot make sense of his criticism of precommunist societies, including socialism, as unjust. The fact that Marx sometimes characterized slavery, feudalism, and capitalism as exploitative even during their heyday, when they still played a role in developing the forces of production on the historical stage and while their own ideologies of justice held sway, implies that Marx was working with ethical criteria external to those systems. And his criticism of socialism implies that the distributive principle of socialism cannot constitute the ideal evaluative perspective. Lukes (1985, 70) is wrong in claiming that "Marxian exploitation counts as such only when set against the external standard of justice provided by the contribution principle of socialism," as is Buchanan (1982, 59) in arguing that "for Marx, the very need for principles of distributive justice is conclusive evidence of defects in the productive processes that form a society's core."

For thinkers such as Lukes and Buchanan, Marx's communism is beyond justice. "The superiority of communism is not that it finally solves the problem of distributive justice, rather it makes the whole issue of justice otiose" (Buchanan 1982, 59). This not unpopular view, in recent times resurrected by Rawls, is inspired by Hume's criteria of justice, which include scarcity and conflict of the kind absent from Marx's image of communism. But why should we let Hume's conceptual fiat forbid us to ascribe justice to a society lacking in his circumstances? Hume himself is unclear about what it means to say that a society failing to meet his criteria is beyond justice, his wording suggesting something less and other than the conclusion that there is no place for the *concept* of justice in a society such as communism. Hume ([1777] 1968, 185–8) talks about appeals to justice being "suspended" or "useless." But does it follow that a society in which *appeals* to justice are moribund is neither just nor unjust, that the *concept* of justice has no place? Why might not the society be so just that there are few, if any, occasions for anyone to protest injustice or claim justice? In a qualifying passage in *A Theory of Justice*, Rawls writes that

> a society in which all can achieve their complete good, or in which there are no conflicting demands and the wants of all fit together without coercion into a harmonious plan of activity, is a society *in a certain sense* beyond justice. It has eliminated the occasions when the appeal to the principles of right and justice is necessary [in a footnote, Rawls mentions Marx's communism as beyond justice in this sense]. I am not concerned with this ideal case, however desirable it may be. We should note though that even here the theory of justice has an *important theoretical role*: it defines the conditions under which the spontaneous coherence of the aims and wants of individuals is neither coerced nor contrived but expresses a proper harmony consistent with the ideal good. I cannot pursue these questions further. The main point is that the principles of justice are compatible with quite different types of regime (Rawls 1971, 281–2, emphasis added).

The Rawlsian version of Hume's criteria suggests that from the standpoint of ideal theory, with its emphasis on the theoretical role of justice, it is sensible to characterize a society lacking scarcity and conflict as just—indeed, as *perfectly* just. Plato and Aristotle inhabit this tradition in ideal theory. In their ideal states (kingship and aristocracy, respectively), citizens and noncitizens occupy their proper roles and shape their wants in accord with them. In their justly ordered paragons, there is neither conflict nor scarcity, and it is from the vantage point of their models that they evaluate actual states. My point, of course, is not that Marx's good society resembles Plato's or Aristotle's. In important respects its egalitarianism, especially its antipathy to the division of labor, renders it antithetical. Marx is one, however, with the classical distinction in political philosophy between the ideal and the actual, assessing the latter in light of the former, and doing so without falling into the trap of utopianism. Marx is no utopian; he has no quarter with schemes that try to institute ideals prematurely. He justifies exploitation in history even in the face of his characterization of it as unjust. It is justified because it is an essential ingredient in developing the forces of production, and that development is a necessary precondition for the perfectly just society—communism—the practical vantage point

[21] I may not be in complete agreement with Elster if, for him, "self-realization" means something other than the type of welfare I have attributed to Marx. If welfare is understood differently, especially as hedonistic, then self-realization and welfare may contrast, as they do for Roemer (1994b, 11–2). Note that I have equated welfare with preference satisfaction, which covers preferences for self-realization, and not with an agreeable state of consciousness.

from which the justified exploitation of the past is condemned as unjust.[22]

Marx's justification of exploitation is neither (1) a species of "the end justifies the means" ethics, in the pejorative sense that evil means are deliberately chosen from an array of viable options, nor (2) a morally incoherent idea. First, Marx's justification of exploitation needs to be understood in the context of his theory of history that accounts for exploitation. In Marx's theory, class oppression and exploitation are likely a necessary condition for the development of the forces of production up to the point at which the creation of a massive surplus and parallel cultural accomplishments set the stage for communism. "Class oppression is necessary to bring society to [the socialist and communist] stage[s] of productive development.... The oppression of one class by another [is] an inevitable cost attached to [the] technically necessary differentiation [entailed by the division of labor]" (Cohen 1978, 207, 211). Far from recommending exploitation, Marx takes it to be an inevitable injustice which, in a way I suggest below, receives ethical scrutiny from the standpoint of communist justice.

This sense of justification can be found even in the theory of history of Kant, the last theorist to whom we would think of attributing an "end justifies the means" ethics. Kant accounted for the "cunning of nature" (on this expression, see Yovel 1980) at work in history according to which exploitation and other evils unwittingly prepare humanity for that which is also morally demanded. In his historical and political work, writes Kain (1988, 16, 44), Kant argued that "the historical development of culture and social institutions is a necessary presupposition for the possibility of morality... [and that] antagonisms, propelled by vainglory, lust for power, and avarice, activate us and cause us to develop our powers and capacities. In time, Kant thinks, such a society will be transformed into a moral whole." For Kant, the main antagonists are nations, for Marx they are classes, "but aside from this... the notion of a conflict of particular interests leading toward what would be demanded by the categorical imperative is much the same for both Kant and Marx" (Kain 1988, 44). Yet, neither Marx nor Kant recommends injustice when it can be avoided, and neither preaches acceptance of exploitation when historical openings permit its amelioration or elimination. Their explanations of injustice should not be mistaken for their sanctioning it, when avoidable.

Second, the idea of justifying exploitation is ethically coherent and can be located in the best of what are commonly designated nonconsequentialist theories. Rawls (1971, 542), for example, justifies the sacrifice of rights and liberties if it "is necessary to enhance the quality of civilization so that in due course the equal freedoms can be enjoyed by all." Even slavery is justified if historical circumstances impose a choice between slavery and alternatives even less just (Rawls 1971, 248). In Marx's case, exploitation is justified because, according to historical materialism, exploitation is a necessary precondition for the attainment of a civilization free from exploitation. And each form that exploitation takes throughout history is justified during its period of vitality because it is progressively less unjust. Feudal injustice is morally preferable to the injustice of slavery, capitalist injustice is preferable to feudal injustice, socialist injustice is superior to capitalist injustice, all the way up to communist justice, which sets the standard of assessment and, consistent with Marx's antiutopianism, establishes the guidelines for moral praxis: Settle for the least unjust form of exploitation from the choices on the historical agenda; struggle against exploitation when it is historically feasible and hence not counterproductive. Within these limits, Marx ([1844] 1964, 52) appeals to "the *categorical imperative to overthrow all those conditions* in which man is an abased, enslaved, abandoned, contemptible being." As Yack (1986, 256) puts it, "Marx obliges us to attack the *conditions* that fail to respect man's humanity. If Kant demands that we overcome heteronomy in the will, Marx [inspired by the leftist Kantians] demands that we overcome heteronomy in the world."

It is because Marx sometimes represents societies as ethically exploitative in their historical prime that Roemer's counterfactual definition of exploitation misses the Marxian ethical point that it is supposed to capture, and it does so because it substitutes an aggregative principle for Marx's distributive one. According to Roemer's game-theoretic definition, people are ethically exploited if they *would be better off* in the event that the distribution of the means of production were rendered more egalitarian. "A group of people S is exploited by its complement S′ in a society with private ownership of the means of production if S would benefit, and S′ would suffer, by a redistribution of ownership in the means of production in which each owned his per capita share" (Roemer 1994a, 97). On this definition, serfs were feudally exploited if they would have been better off had the distribution of feudal property been egalitarian, which is to say if feudal lords had not controlled the labor of serfs. The abolition of feudalism would still leave us with capitalism. And workers are capitalistically exploited since they would be better off if the distribution of property under capitalism were egalitarian, which is to say if capitalists did not own the bulk of the alienable means of production.

Now, if we go along with this definition of exploitation, it follows that a society is not ethically exploitative prior to its historical task in developing the forces of production; that feudalism, for example, was not exploitative during the period when a more egalitarian distribution of property would not have improved the welfare of the serfs. Yet, Roemer knows that Marx considers feudalism and capitalism exploitative during their period of bloom. "Historical materialism," writes Roemer, "claims that each form of *exploitation* is socially necessary from a dynamic point of view," and

[22] "From the moment the workingmen class movement became real, the phantastic utopias evanesced, not because the working class had given up the end aimed at by these Utopists, but because they had found the real means to realize them" (Marx, first draft to *The Civil War in France*, cited in Elster 1985, 218).

the view that "early capitalist society was not unjust, assuming that capitalism was an optimal economic structure for the development of the productive forces, and even for human welfare, at that time . . . is a noncredible position" (1988, 145, 144, emphasis added). Thus, we get Roemer's conundrum: On his game-theoretic definition, there is no exploitation in feudalism, capitalism, and so forth, during their heyday, but on the Marxian and credible account there is exploitation, as in the concept of "socially necessary exploitation" that Roemer (1988, 143–7) tacked on in his more recent theorizing, trying to accommodate his original theory to Marxian concerns. But this latter concept counts not so much as an amendment to the game-theoretic approach as a contradiction. It goes against the grain of the general theory forced by the game-theoretic methodology, with its neoclassicist emphasis on aggregative principles. On my reading (which attributes to Marx the ideal of equal welfare), and on Roemer's admission, Marx condemns exploitation on *distributive* grounds, so that even if exploited "coalitions" would suffer an aggregate reduction in their average or total welfare in the event that they were to withdraw their per-capita share of assets from their extant situation, they still would count as being ethically exploited. This reflects both the Marxist and an intuitive sense of justice and thus has the advantage over the game-theoretic approach as a way of elucidating the Marxian project.

For Marx, the development of the forces of production is a necessary precondition for communism but, as Moore (1980) has argued persuasively, this precondition is not sufficient. The theory of historical materialism does not explain the transition from socialism to communism. In other words, upon the achievement of socialism, it becomes historically possible to stay there or to move to communism, the point being that the distributive practice of communism does not seem to be required by (explained by) the development of the forces of production. "That transition is explained in terms of the superiority of communism as an ideal of distributive justice" (Moore 1980, 36). "The argument from historical materialism proves at most that classless economies will develop to a point where men can choose between socialism and communism. The prediction that they will choose communism rests wholly upon the argument from distributive justice" (Moore 1980, 45). It turns out, then, that Marx's ideal theory functions both negatively as critique of precommunist societies and positively as recommendation encouraging the move to the good social life, an association that ensures an equal distribution of welfare, which is to say the full development of the inequality among individuals, of the inequality of their capacities, desires, and personalities. Communist justice establishes the equal right to personal inequality in a setting of social equality. This equal right, respectful of persons qua persons, transcends bourgeois right, which safeguards those whose interests can be realized only at the expense of others. This "higher right," which Moore traces to the influence of Hegel's *Philosophy of Right*, corresponds to the ideal justice of Marx's communist society. Whether Moore is correct in his genealogy is an issue I take up in the following speculative remarks.

Marx correctly says, in *Critique of the Gotha Programme*, that every right, in its content, is a right of inequality. He is confused, however, in claiming that every right suffers from the defect of treating each individual in one particular aspect only, for example, basing distributive rights on the equal standard of labor contribution. Marx's general rule for communist distribution, after all, accords to all the "higher right" to equal welfare and thus considers recipients in all the unequal aspects that define their particularity. Communist right does not discriminate according to "one-sided" criteria, à la bourgeois *recht*, but transcends that narrow horizon with a distributive principle making possible for each person an all-sided individuality that finds realization in both the spheres of production and consumption. In other words, Marx's communism does not consist in an identity of interests or homogeneity among some amorous communal assemblage with the unity of man with man as its basis, but rather

> a community [within which] people may still express themselves in different ways and have different interests. Marx's point is that these interests will be harmoniously related and united. They may be different as the notes of a musical chord, and yet still fit together to form a coherent, harmonious, meaningful whole. The unity or harmony of interests will then preserve the particularity of the individual parts: "The unity of man with man" . . . is based on the real differences of men (Brenkert 1979, 121).[23]

The concept of communist right, therefore, does not entail prejudice against the welfare of individuals with different abilities, wants, or plans of life. To whom are we to trace this conceptually coherent and pluralistic idea of right?

It would be a mistake, following Moore (1980), to attribute unqualifiedly Marx's "higher right" to Hegel. First, Hegel's superior right embodies not only general rules (which is fine) but also patterns of solidarity that characterize *communities*. But we have good reason to believe that Marx's communism is anything but a community, especially if the latter possesses the sense given to it by contemporary communitarian theorists such as Sandel (1982), MacIntyre (1981), and Taylor (1979). There is little in Marx suggesting that in communism the *identity of persons* is constituted by parochial, national, neighborhood, or family attachments, and there is everything that suggests Marx's

[23] Kymlicka (1989, 126) writes: "I think the Kantian strain in Marx is a more promising starting point [than the strong, Aristotelian perfectionist one]. According to this premiss, individuals should be free to decide for themselves what is worth doing with their lives, within a community that is transparently intelligible, and that treats each individual as an equal. But of course that is the premise that liberals start out from as well." Kymlicka does ascribe to Marx, I think correctly, a weak kind of perfectionist ethics that is compatible with pluralism, what I call thin perfectionism and what Rawls (1971, 326) calls "perfectionism [of an] intuitionistic form," which he dubs a reasonable view and one not inconsistent with liberal justice. This kind of justice incorporates an equality principle along with a principle of perfection and so differs from the perfectionism of antiliberal thinkers such as Aristotle, Nietzsche, Leo Strauss, and their epigones. See Kymlicka 1989, 35, and Freeman 1994, 341–2.

antipathy to the communitarian *way of life*, complete with its patriotic rituals, conformist morality, and structured attachments. Might it be that, as Lukes (1985, 93) wonders, the unity of Marx's communism is no more than the "unity of freely associating sovereign choosers," "free of extraneous purposes, natural necessities, and social duties," "combining to attain the full development of human mastery over the forces of nature ... as well as humanity's own nature?"[24] Lukes is not sure, but Cohen is. For Cohen (1978, 133), Marx's communism not only is not a community but also is an alternative to society. Communism abolishes roles and the mediation of institutions, leaving individuals to face one another and themselves "as such," freed from the ossified social activity that stands as an objective, alienating power above them.[25]

Second, Hegel is the wrong place to go when justifying an interpretation (which Moore accepts) of Marx as ideal theorist, as one who has an ideal theory of justice that functions to assess precommunist societies. Wood (1981, 132) has an Hegelian reading of Marx: "For Marx, as for Hegel, the morally rational is determined by the socially actual," which is to say that Marx is interested only in the sociology of morals, not in any first-order moralizing. This is the view that Marx considers slavery, feudalism, and capitalism to be just according to their own standards (which are the only relevant ones), that Marx has no "properly" moral theory, and so on. It is Wood's view, as Ryan (1987, 119–20) puts it in a more recent espousal, that "Marx followed Hegel in disbelieving in the existence of the realm of the *ought* which stood opposed to the *is*." Intimately related to this slant on Marx, and partially responsible for it, is the idea that Marx has no moral agenda because of a distinction thought to exist between moral and nonmoral goods, the former including things such as right, justice, and duty, the latter including items such as pleasure, self-realization, and happiness. In short (and in line with a familiar distinction in moral philosophy), it is said that Marx puts the nonmoral good before the moral right and that his sole aim is the maximization of the former in communist society (Wood 1981, chapter 9).

But if the argument I have set out possesses the plausibility I have awarded it, then this cannot be right. This is because Marx attacks precommunist societies, including socialism, as ethically exploitative independently of whether they foster the development of the forces of production or maximize the nonmoral good. His criticism of socialism is especially telling because this is one of the few occasions on which he speaks plainly about a defect in a distributive principle, and the context makes it clear that he is speaking of a moral defect. Do not moral concerns about distribution imply an interest in justice? Marx's condemnation expresses a moral indignation at institutions that permit and pressure individuals to treat one another systematically not as ends but means, his language implying a sense of justice that has more in common with Kant than Hegel. This is not to say that Marx is unconcerned with maximizing nonmoral goods, such as freedom and self-realization, along with their material and cultural bases, but it is to say that he is interested in their radically egalitarian distribution.[26] Peffer identifies Marx's moral theory as "mixed deontologist," as one

> that considers the production of the (specified) moral goods as a relevant consideration but holds, nonetheless, that the right action may not be the one that maximizes the nonmoral good. The production of the nonmoral good may be constrained, for example, by the principles of its distribution or by other principles of right that are not themselves validated on the basis of their production or maximization of the nonmoral good. John Rawls and William Frankena are examples of contemporary moral philosophers holding mixed deontological theories (Peffer 1990, 84).

Three caveats are in order. First, to call attention to Kant-Marx affinities does not entail that Marx buys into the transcendental element of Kantian ethics any more, say, than Rawls does in his Kantian interpretation of justice as fairness. The ethical principle of equal concern and respect for persons is what Rawls gets from Kant, and these are persons subject to the general conditions of human life, not ontologically "noumenal" nor "pure intelligences not subject to [the restraints and circumstances of social existence]" (Rawls 1971, 257).[27] Second, the Rawlsian version of the principle accords a primacy to the social, in contrast to Kant's application to the personal maxims of conscientious individuals in everyday life (Rawls 1980, 552). Marx similarly distanced himself from Kant on this score, albeit more polemically ([1844] 1964, 207–10). Third, Rawls no more than Marx accepts the radical anticonsequentialism of standard interpretations of Kant according to which the principle of treating persons as ends in themselves cannot be used to sacrifice rights and liberty in the name of promoting conditions necessary to bring about the Kingdom of Ends.[28]

[24] Also see Luke's (1967) fine piece in which he sets the communitarian views of Durkheim against those of the "anti-social" Marx. Yack (1986, 292), like Lukes, understands that "the unstated premiss of Marx's argument ... is that man possesses the negative capacity to resist identification with any particular external condition, the capacity that Kant and Hegel use to define man's humanity and special dignity."

[25] The relevant passages are in Marx and Engels ([1846] 1964, 86–95).

[26] "The idea of treating people as equals is fundamental to Marx. It often appears in Marx in the same form that it appears in Kant, i.e., that we should treat people as ends not means. This requirement underlies both his critique of capitalism and his defence of communism" (Kymlicka 1989, 107). For more on Kantian themes in Marx, see Peffer 1990, 118–23.

[27] And see Rawls 1993, 27, where he clarifies the mistakes of Sandel.

[28] For heterodox interpretations, see Cummiskey 1990 and especially Korsgaard 1986. Korsgaard distinguishes between ideal and nonideal theory and explains the role of the latter in Kant's ethics. The basic idea is that nonideal theory applies under nonideal conditions—those that prevent realization of the ideal principles which prevail in the Kingdom of Ends—and requires conduct that can be realistically expected to further conditions in which realization is feasible. This should not be confused with consequentialism. "In a non-ideal case, one's actions may be guided by a more instrumental style of reasoning than in ideal theory. But non-ideal theory is not a form of consequentialism. There are two reasons for this. One is that the goal set by the ideal is not just one of good consequences, but of a just

I cannot *prove* the conjuncture between Marx and Kant on the topic of exploitation and justice as discussed here. As almost everyone who has written on Marxian justice and morality stresses, Marx's refusal to engage in systematic talk about this topic allows for different readings. Exegesis provides no controlling evidence. It is a matter of plausibility, not proof, that as the Kantian socialists argued at the turn of the century, "there is a Kantian dimension to [Marx's] work" (Van Der Linden 1988, 64) as well as the obvious Hegelian one, leaving open the question of whether or how these dimensions can ever be reconciled. But it suffices here if I have advanced the view that Marx has a general theory of exploitation—logically independent of value, labor, and class—derived from a model of a perfectly just society in which everyone, by virtue of personhood, is deserving of equal welfare.[29] Those societies which deny this by permitting morally arbitrary factors to determine distributive shares stand condemned as exploitative and unjust.

state of affairs. If a consequentialist view is one that defines right action entirely in terms of good consequences... then non-ideal theory is non-consequentialist. The second reason is that the ideal will also guide our choice among non-ideal alternatives, importing criteria for this choice other than effectiveness" (Korsgaard 1986, 343n).

[29]Galston (1991, 131–2) writes: "There is nothing self-contradictory about basing a [desert] claim on some undeserved feature of yourself... [e.g., if] moral personality is the basis of your claim to be taken into consideration." He goes on to say that Rawls "establishes as the sole, relevant desert basis a feature of our lives (moral personality) that equally characterizes nearly all of us." The problem is that Rawls's second principle justifies an unequal distribution of economic and social goods, and this has generated the criticism that Rawls fails to accommodate properly the practice of desert, substituting for it that of "entitlements" for which individuals seem to be undeserving. This is not the place to elaborate on this complex issue, except to say, with Cohen (1989), that Rawls relies on entitlements only because he thinks that the idea of rewarding desert is impracticable, that is, because of the extreme difficulty in arranging distributive institutions which systematically separate morally arbitrary considerations from those involving free choice and self-determined effort and responsibility. And this separation needs to be done in cases of expensive preferences in the face of scarcity. Marx, given his rosy assumption of abundance in communism, does not even begin to address this problem, but contemporary egalitarian theorists have worked up the outlines of hypothetical distributive schemes that might sort out the morally relevant from the morally arbitrary. See, for example, Roemer 1993 and Van Parijs 1995; also interesting is Miller's (1996) treatment of market distribution and moral desert.

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Political Liberalism, Deliberative Democracy, and the Public Sphere

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Theorists of democracy emphasize the importance of a public sphere, distinct from the apparatus of the state, where citizens can freely associate, deliberate, and engage in collective will formation. Discourse ethicists and deliberative democrats locate the public sphere within civil society and the manifold associations that comprise it. For Seyla Benhabib, the public sphere is constituted by the anonymous "public conversation" of civil society. By contrast, John Rawls has a much more limited conception of the public sphere. For Rawls, public reason, which establishes norms for democratic discourse, applies to a limited domain. I defend Rawls's view against the charge that it depends upon an untenable distinction between the public and nonpublic spheres. I argue that Rawls's more limited "liberal" conception better guarantees the heterogeneity of associational life in civil society. I then argue that Rawls violates his own principles by partially collapsing the public-nonpublic distinction with potentially illiberal consequences.

"The recovery of the public realm" might be an appropriate way to characterize a recent trend in democratic political theory. Democratic theorists, in the tradition of Tocqueville, now emphasize the importance of voluntary associations in civil society for the generation of "social capital," that is, the various norms associated with the democratic art of collective self-rule and decision making (Putnam 1995). Deliberative democrats and discourse theorists emphasize, in the Arendtian tradition, the importance of a free public sphere, separate from the apparatus of the state and economy, where citizens can freely debate, deliberate, and engage in collective democratic will formation. Jürgen Habermas and various deliberative democrats emphasize the importance of both an Arendtian public sphere and associations in civil society. These theorists locate the public sphere not in a common public space, as in the Greek polis, or in a Rousseauan general assembly attended by all citizens, both of which are seen as untenable alternatives under conditions of modernity. They locate it within civil society itself, that is, within all the manifold forms of associational life that comprise civil society in the modern democratic state. According to Seyla Benhabib (1996, 73–4), the reason a deliberative model of democracy can dispense with the fiction of a mass assembly is that it locates the public sphere in a plurality of modes of association within civil society: "It is through the interlocking net of these multiple forms of associations, networks and organizations that an anonymous 'public conversation' results." Benhabib (p. 75) contrasts this model with the "liberal model" of John Rawls, for whom the public sphere is located not within associational life in civil society but, rather, within a more restricted sphere, including first and foremost the legal sphere and its institutions.

In this article I shall argue for just such a limited liberal conception of the public sphere as presented in the work of Rawls. This more limited conception actually better serves to guarantee the autonomy of associational life within civil society and its independence from the coercive powers of the state (or, for that matter, from the "coercive power" of normative political theory). My purpose is in no way to question the significance of associational life either for the process of democratic will formation or for its role in instilling the various norms associated with democratic citizenship, that is, its value as social capital. Rather, it is to argue for the value of a principled liberal distinction between the public and the private.

What I have in mind here by preserving the public-private distinction is not the Arendtian concern that the rise of the social has effaced the distinction between the public and private and hence the dividing line between the realms of "necessity" and "freedom," reducing political life to a form of "household administration" (Arendt 1957, 33–49). Nor is it the related Habermasian concern that the space of democratic decision making has now been "colonized" by technical administrative imperatives (Habermas 1985, 1989). These are legitimate concerns. What I mean by the private sphere is simply the familiar liberal concept of a realm of thought and action that is protected from the coercive power of the state and that involves the concepts of limited government, liberty of conscience, and the separation of church and state. In the "Liberalism of Fear," Judith Shklar (1989) identified the maintenance of just such a public-private distinction as a kind of shibboleth for identifying a "true" form of liberalism:

> [Liberalism] must reject only those political doctrines that do not recognize any difference between the spheres of the personal and the public. Because of the primacy of toleration as the irreducible limit on public agents, liberals must always draw such a line. This is not historically a permanent or unalterable boundary, but it does require that every public policy be considered with this separation in mind and consciously defended as meeting its most severe current standard. The important point for liberalism is not so much where the line is drawn, as that it be drawn, and that it must under no circumstance be ignored or forgotten (Shklar 1989, 24).

I shall argue that, although this is certainly not their intent, some discourse ethicists and deliberative democrats are in danger of collapsing this distinction with potentially illiberal consequences. And I shall argue that Rawls himself ultimately violates his own founda-

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tional principles by partially collapsing this distinction with the same consequences.

POLITICAL LIBERALISM AND PUBLIC REASON

In *Political Liberalism* (1993, xvi–xviii), Rawls asks how persons in a democratic society whose political culture is characterized by a diversity of opposing and irreconcilable religious, philosophical, and moral doctrines can freely endorse a common political regime. In other words, what is the basis of political legitimacy in a pluralistic society? For Rawls, the answer depends upon articulating a conception of justice that can be endorsed by the various "reasonable" comprehensive views in society, that is, one that can be the focus of an overlapping consensus despite profound disagreement on other matters of "ultimate concern." To uncover such a consensus, the conception of justice to regulate society, and the political society regulated by that conception, "should be as far as possible, independent of the opposing and conflicting religious and philosophical doctrines that citizens affirm" (p. 9). Among such conflicting doctrines, Rawls now includes the liberalisms of Kant and Mill, as well as his own *A Theory of Justice* (1971), seen as fostering certain values (i.e., individuality and autonomy) meant to govern much if not all of life (Rawls 1993, xvi–xvii, 98–100). In a society characterized by value pluralism, the values of individuality and autonomy as regulative ideals for all of life will not be, and should not be expected to be, an ideal for all citizens. According to Rawls (p. 13), what distinguishes political liberalism from such "comprehensive" liberalisms as those of Kant and Mill, as well as all other comprehensive views, is its limited scope. It applies to a limited public domain—that of persons as citizens—and the values and virtues it invokes are meant to apply to persons in their capacities as citizens, that is, to be "political" as opposed to "ethical" virtues and to be justified on the basis of certain qualities of character necessary for citizens in a democratic regime (p. 195).

All these distinctions, namely, that between a common public political conception of justice and various comprehensive views within society, between "political" and "ethical" virtues, between persons qua citizens and persons qua persons, between political values and other values that apply to "all of life," and between the modes of reason appropriate to the public sphere (i.e., public reason) and those appropriate to other "spheres," depend ultimately upon one fundamental distinction—that between the public and private. Yet, Rawls (1993, 220) specifically states: "The public vs. nonpublic distinction is not the distinction between public and private." He avoids the locution of a public-private distinction because of the individualistic non-associational connotations of the word "private," maintaining that "there is no such thing as private reason" (p. 220). The word *nonpublic* is employed by Rawls most often to denote various forms of associational life within society—what in *A Theory of Justice* are called "social unions." These include the family as well as all kinds of voluntary associations in civil society, such as clubs, churches, and universities (Rawls 1971, 520–9).

Paralleling this distinction, Rawls makes a fundamental distinction between public and nonpublic "uses of reason." The nonpublic uses include the "social," the "familial," and the "individual" (Rawls 1993, 220). Most often by the term "nonpublic reason" Rawls intends the social uses of reason, and it is in this sense that I shall employ the term throughout. These nonpublic reasons are simply the various forms of reasoning, whatever they may be, that characterize associations within civil society. It is, for example, the reasoning of "churches and universities, scientific societies and professional groups. . . . Nonpublic reasons comprise the many reasons of civil society and belong to what I have called the background culture in contrast with the public political culture" (p. 220). By contrast, public reason is a normative ideal, meant to apply in particular to the judiciary and a supreme court—called the "exemplar" of public reason; to political officials when they "speak in official forums"; to the executive in its public acts and pronouncements; to members of political parties and candidates in their campaigns; as well as to citizens when they "engage in political advocacy in the public forum" and vote on issues concerning "constitutional essentials and matters of basic justice" (pp. 215–6).

In brief, public reason limits, in the specific venues and circumstances noted above, the reasons invoked in a process of justification or argumentation to those that appeal to political values or the values of a "political conception of justice," which conception itself involves a conception of society as a fair system of cooperation among free and equal citizens possessed of certain basic rights and liberties and entitled to certain basic opportunities (Rawls 1993, 15–6, 223). The same criteria that determine what can count as legitimate reasons for the selection of principles of justice also ideally determine what can be advanced as legitimate reasons in the public sphere: "In justice as fairness, then, the guidelines of public reason and the principles of justice have essentially the same grounds" (pp. 225–6). Because of a presumed divergence of moral and religious interests, the limitations imposed by the veil of ignorance preclude the parties in the original position from giving as a reason for the selection of a principle of justice that, for example, it is prescribed in the Decalogue (Rawls 1971, 14, 127, 327). Similarly, public reason in its strictest form precludes appeals to particular comprehensive moral, religious, or philosophical doctrines in the public sphere (Rawls 1993, 10, 214–8).[1]

[1] Rawls (1996) revised his view somewhat in Section 5 of the Introduction to the paperback edition of *Political Liberalism*. There he says that reasonable comprehensive doctrines "may be introduced into public reason at any time, provided that in due course public reasons, given by a reasonable political conception are presented sufficient to support whatever the comprehensive doctrines are introduced to support" (p. lii). There are many problems with this formulation. By allowing only reasonable comprehensive views into public reason, Rawls adds very little to his original formulation, inasmuch as reasonable comprehensive views are defined as those

In addition to limitations on content, public reason incorporates certain limits on method. These involve an adherence to general principles of reasoning, criteria of relevance, and rules of evidence—"accepted general beliefs and forms of reasoning found in common sense, and the methods and conclusions of science when these are not controversial" (Rawls 1993, 224–5). As with the restriction upon reasons, the restrictions on method are meant to provide a public basis of justification accessible to citizens generally: For example, revelation would be "ruled out of court" (literally and figuratively) as a rule of evidence. Thus, public reason seeks to create a common ground of public-political justification, which by limiting itself to the values of the political and ways of reasoning available to citizens generally can provide an acceptable basis of legitimation for citizens in a pluralist society. As such, public reason is a form of what Gutmann and Thompson (1996, 55) have characterized as a principle of "deliberative reciprocity": "When citizens deliberate, they seek agreement on substantive principles that can be justified on the basis of mutually acceptable reasons."

CONTESTING THE LIMITED PUBLIC DOMAIN

Distinguishing the Domains

The first objection I shall consider is that public reason, by attempting to limit the justification for legislation to an appeal to political values, relies upon a rigid a priori division between the public and private, one in which the public is defined as that domain of actions seen as appropriately regulated by principles of justice and the coercive power of law. According to Habermas (1995, 129), such an a priori boundary conflicts with historical experience and with "the fact that the historically shifting boundary between the public and private spheres has always been problematic from a moral point of view." Similarly, Benhabib (1992, 99–100) maintains that "democratic politics challenges, redefines and renegotiates the divisions between the good and the just, the moral and the legal, the private and the public." She argues that practices once considered to be part of the private domain, such as certain aspects of worker-employee relations, are now seen as "public," in the sense of being viewed as matters of political justice and as appropriately regulated by law. Conversely (although Benhabib does not make this point), some matters once viewed as public in the sense of being regulated by law are now seen as part of the private domain (e.g., contraception).

Yet, the concept of public reason is in no way inconsistent with the fact that matters once considered part of the private domain have come to be seen as appropriately regulated by law and, hence, to that extent public, or that matters once considered to be appropriately regulated by law are now viewed as part of the private domain. When matters once viewed as "private" come to be seen as proper objects of legislation, such legislation is itself generally justified in terms of public-political values. For example, the repudiation of laissez-faire economics, upheld by the Supreme Court in *Lochner* v. *New York* (1905), entailed a shift in the view of which practices could be appropriately regulated by law and thus entailed a corresponding shift in the view of the boundary separating the public and private domains.[2] The justification for the subsequent legal regulation of such practices, however, has been presented in terms of the promotion of the political values of regulating the health, safety, and economic well-being of the general public, and in this way such regulations have been justified in a manner consistent with the limitations of public reason.

Does the acknowledgment of a potentially shifting boundary between the public and private domains entail, then, that everything is potentially subject to public determination and legal regulation, as long as a justification for such regulation is presented in terms of public reasons?[3] The answer is "no," for a reason related to the concept of public reason itself: The limitations of public reason are in part a reflection of the *limited ends of government*. As noted above, for Rawls the principles of justice and the principles of public reason have the same grounds. Since it is assumed that persons have diverse comprehensive (e.g., religious) beliefs that they seek to advance, one proper role of government is seen as upholding the liberty to exercise such beliefs. In this way, the free exercise of religion remains a "private" right of individuals (although it is a right most often exercised by individuals as members of various groups). Any attempt to regulate this "region" of the private domain—that concerned with religious belief and practice—would entail simultaneously a violation of public reason and a violation of the basic principles of the liberal democratic state, for it would constitute a violation of the principles of the separation of church and state and liberty of conscience. This does not mean, of course, that such practices cannot be regulated if they are deemed to endanger the health and safety of members of the community or violate their rights as citizens: Churches can excommunicate heretics but not burn them at the stake. But the justification for such regulation must itself be presented in terms of

which will support the proper balance of political values and which do not "run afoul" of public reason (1993, 243–4). Only "reasonable" comprehensive views are allowed into public debate, but before any debate has occurred, before the "reasonable" balance of political values has been determined, how do we know which comprehensive views are reasonable concerning a given issue? For more on this point and the problem with the definition of a "reasonable" comprehensive doctrine, see my discussion below.

[2] The shift to which I refer is not the regulation of liberty of contract, since private contracts have always been "public" to the extent that such contracts have been upheld by law. (Similarly, the "free market" is not a "private market," to the extent that market transactions are upheld by the coercive power of law and the state.) I refer to the regulation of practices once genuinely unregulated and hence "non-public," such as workers' wages and workplace safety, which subsequently came to be legally regulated "public" concerns.

[3] I am indebted to an anonymous reviewer for raising the question of whether a strict conception of public reason is consistent with a belief that everything is in principle subject to public determination.

the limited, that is, political, ends of government.[4] Thus, substantive limitations upon the proper ends of government determine both the limits of the public domain and the limitations imposed by public reason. Since public reason itself places a limit on what can be viewed as a public harm or a public good, it places a limit upon what can be a matter for legislation. Salvation is not a public good; damnation is not a public harm, since, in Lockean terms, "the Care of Souls is not committed to the Civil Magistrate" (Locke [1689] 1983, 26).[5]

This point leads to the following related objection. The distinction between the reasoning of associations within civil society and the public domain subject to the limitations of public reason is an untenable one, inasmuch as many civil associations are also public bodies—such as churches and universities—and therefore must comply with some of the same legal and constitutional restrictions to which other more obviously public (i.e., governmental) institutions are subject. As Benhabib (1996, 76) notes: "All clubs, associations and organizations within civil society have charters that can be subject to public as well as legal scrutiny." A country club that discriminates on the basis of race may have to change its charter if it seeks to become a legally incorporated entity. From this Benhabib concludes that "the constitutional-democratic state and the institutions of civil society are therefore not as sharply separable as some of Rawls's formulations suggest" (p. 76).

While it is certainly true that all such associations can be subject to legal regulation, one cornerstone of American constitutionalism, the so-called state action doctrine, depends upon the recognition of a fundamental distinction between the public and nonpublic spheres in this regard. Concerning the importance of maintaining this distinction, particularly in relation to voluntary associations in civil society, Justice Douglas wrote in his *dissenting* opinion in a case in which the granting of a liquor license to a "whites only" fraternal organization was found not to constitute state action:

> My view of the First Amendment and the related guarantees of the Bill of Rights is that they create a zone of privacy which precludes government from interfering with private clubs or groups. The associational rights which our system honors permit all white, all black, all brown, and all yellow clubs to be formed. They also permit all Catholic, all Jewish, or all agnostic clubs to be established. Government may not tell a man or a woman who his or her associates must be. The individual can be as selective as he desires. . . . The problem is different, however, where the public domain is concerned (*Moose Lodge No. 107* v. *Irvis* 1972, 179–80).

If a church or a university discriminates on the basis of race, then it can lose tax-exempt status, the granting of such status being seen as an instance of state action and hence as bringing such institutions into the public domain *to that extent (Bob Jones University* v. *United States* 1983). Of course, as Douglas's dissent in *Moose Lodge* indicates, the question as to what constitutes state action and hence the "public domain" is a highly contested and complex issue. Justice Rehnquist, who wrote for the majority in *Moose Lodge*, rightly noted in another case: "While the principle that private action is immune from the restrictions of the Fourteenth Amendment is well established and easily stated, the question whether particular conduct is 'private,' on the one hand, or 'state action,' on the other, frequently admits of no easy answer" (*Jackson* v. *Metropolitan Edison Co.* 1974, 349–50).

It is not my intention here to enter into a consideration of the complexities of the state action doctrine. Furthermore, as Cass Sunstein has noted, (1993, 160) the pertinent question is often not whether the state has acted at all—upholding a racially restrictive contract or evicting protesters from a mall seem unambiguous instances of state action—"but whether the state action *at issue in the relevant case* violates the pertinent provision of the Constitution," that is, violates the equal protection clause or any other constitutional provision. According to Sunstein, however, his claim that upholding laws of contract, tort, and property constitutes unambiguous instances of state action is "emphatically not that there is no line between public and private action, or that private action is constitutionally restricted," or that "everything is state action" (p. 159). My point here is not to emphasize that the boundaries dividing the public and nonpublic in regard to associational life are easily determined or uncontroversial; rather, what Shklar (1989, 24) said of liberalism in general applies in this case as well: What is of vital importance "is not so much where the line is drawn, as that it be drawn, and that it must under no circumstance be ignored or forgotten."

Public Reason and Civil Society

There is, of course, another way in which constitutional doctrine supports the public-nonpublic distinction in regard to associations within civil society, and these are the general provisions of liberty of conscience as embodied in the First Amendment. Thus, while liberty of conscience protects the individual from the church—persons may freely join and leave religious associations with no loss of civil liberties—it also protects the church from individuals, other associations, and the government. As Rawls (1993, 221) notes, "equally, liberty of conscience and other liberties such as freedom of association protect churches from the intrusions of government and from other powerful associations." And from its earliest formulations, the doctrine

[4] For more on this point and its implications in regard to the establishment and free exercise clauses of the First Amendment, see my discussion below.

[5] Stating this does not entail that it is illegitimate for the state to take measures to promote in citizens the various political virtues associated with democratic citizenship. As Rawls (1993, 195) notes: "If a constitutional regime takes certain steps to strengthen the virtues of toleration and mutual trust, say by discouraging various kinds of religious and racial discrimination (in ways consistent with liberty of conscience and freedom of speech), it does not thereby become a perfectionist state of the kind found in Plato and Aristotle, nor does it establish a particular religion as in the Catholic and Protestant states of the early modern period."

of the separation of church and state has been invoked as a way to protect the autonomy of churches, as well as individual liberty of conscience, from governmental intrusion.[6]

The doctrine of the separation of church and state means that churches and other religious associations have a protected domain of "nonpublic" reasoning subject neither to the coercive legal power of the state nor to anything like the limitations of public reason—construed as a normative ideal, not a legal requirement (Rawls 1993, 213, 220–2). Such a domain constitutes the "internal life" of religious associations. Members of a congregation will normally appeal to the "whole religious truth" as they see it in regard to most matters, and this includes political matters. Appeal is made to criteria of truth and rules of evidence specific to their faith, for example, to the divine nature of a text or a prophet or to divine revelation. Of course, religious beliefs and practices may be exposed to criticism by others within civil society; they may become part of a larger "public" debate. Such beliefs and practices may themselves be influenced by debate in civil society and respond to changing societal and cultural norms. In this regard, Benhabib (1992, 65) notes how the "private and public languages of morals intertwine, and how the conventional, religious private morality of individuals and sects gets infiltrated and influenced by the post conventional morality of the liberal democratic state."

It is certainly not my intent to deny the reality of this process. The internal life of religious and other associations is rarely hermetically sealed from the outside world and the surrounding civil society (although some sects do strive to preserve their "traditional" ways and to insulate themselves as far as possible; see the well-known case *Wisconsin* v. *Yoder* 1972). Debates concerning, for example, the ordination of women, the marriage of priests, the doctrine of papal infallibility, and a host of other issues "religious" in nature can and do become topics of debate in civil society, involving interdenominational dialogue, the media, and the participation of any and all associations and individuals who feel that they have a stake in the matter. Here, too, nothing like the limitations of public reason are appropriate. Feminists—Catholic or not—may attack the doctrine of the Catholic church concerning the ordination of women by appealing to whatever principles they wish, and the church may respond, as it has responded, by appealing to its particular understanding of religious doctrine and a given religious tradition. Debates of this kind may be unabashedly partisan in the sense that appeal is appropriately made to sectarian religious principles which may not constitute acceptable reasons for others. In debates of this kind, however, what is at issue does not concern law, that is, the use of the coercive power of the state. Churches are voluntary associations: Those dissatisfied with the policies of a given denomination are free to engage in public protest, leave the faith, join another denomination, or establish one of their own.

Even if public reason allows for flourishing debate in civil society, what if the debate does concern a matter for legal regulation? Are not many of the most bitterly contested issues in our society today precisely of this sort? Here it is important to stress the limited role of public reason. Rawls (1993, 215) says that in addition to particular venues associated with governmental power and governmental officials, public reason holds for citizens when they "vote in elections when constitutional essentials and matters of basic justice are at stake" and "when they engage in political advocacy in the public forum." While he says that it holds for members of political parties and candidates and for groups who support them, is anything more implied by his description of the "public forum"? I think that in addition to such unambiguously political forums as the platforms of political parties, public reason can reasonably be applied in those venues where debate or advocacy occurs on matters of potential legislation concerning citizens in general *and the arguments—the reasons invoked—are intended to apply to any and all citizens*. Obviously, what I have just described would include some but not all media; it would not include religious television programs or religious newspapers or journals. But it would apply to members of any and all religious associations when, for example, they appear on the local news to advocate for or against legislation.

This may seem like an overly subtle and potentially illiberal application of the principle. Certainly, freedom of expression places no legal restrictions upon forms of argumentation in the media or any other public venue. Here it should be emphasized that the question is not one of law. Public reason is a self-imposed principle, a "duty" of civility and reciprocity, no more subject to legal enforcement than duties of politeness or etiquette. In Kantian terms, it is a duty associated with the doctrine of virtue, not the doctrine of right: "The system of the doctrine of duties is now divided into the system of the *doctrine of Right (ius)*, which deals with duties that can be given by external laws, and the system of the *doctrine of virtue (Ethica)*, which treats of duties that cannot be so given" (Kant [1797] 1991, 185). The difference is that, for Rawls, the duty of adhering to public reason is meant to involve a more limited political rather than an ethical virtue (both, however, are *moral* virtues—the political is a subset of the ethical/moral).

The problem with this, according to Thomas McCarthy (1994, 52), is that "political discussion would have to be radically transformed whenever the venue changed in relevant ways, even if the very same people were discussing the very same issues." But, he continues, "can individuals reasonably be expected to divorce their private and public beliefs" in such a way as not to appeal to the whole truth as they see it? In contempo-

[6] See, for example, the doctrine of "evangelical separation" associated primarily with Roger Williams, according to whom "worldly corruptions . . . might consume the churches if sturdy fences against the wilderness were not maintained" (Howe 1965, 6). Yet, Williams also thought it was possible for religions to accept a certain amount of state aid without state control, so his view has been characterized as one which saw the state as properly fostering a climate conducive to all religions and allowing for the existence of various forms of de facto establishment (Howe 1965, 11–2).

rary pluralistic democratic society, however, people do often tend to adhere to something like the limitations of public reason—or come to adhere to them—when engaging in political advocacy in the particular limited forums mentioned above. The reasons may be simple prudence, that is, a desire to win over a larger constituency, an obvious objective when one speaks in forums directed to the "public at large." A politician in a religiously pluralist district, state, or country would be ill advised to argue in support of a given piece of legislation on the grounds that it is concordant with or supported by a particular religious doctrine, for the obvious reason that if her goal is to convince others to support the legislation, then she should offer to them reasons that they might reasonably be expected to accept. Furthermore, the offering of reasons specific to a particular religion could be construed as not respecting the religious differences of those of other faiths.[7] And if her reasons are moral and not prudential, then she will be motivated by a principle of reciprocity to seek a common ground of reasoning with others, to offer reasons that constitute a common currency of debate in a pluralist society, that is, political reasons. For example, in the public arguments of religious opponents of abortion over time, a shift is noticeable away from particular religious appeals to arguments in terms of political values, that is, the "rights" of the fetus and the state's interest in preserving life.[8]

While deliberative democrats have expressed concerns about what they see as the overly restrictive limitations of public reason, they often embrace principles of reciprocity that are almost indistinguishable. Consider, for example, Benhabib's (1996, 71–2) description of what is involved in articulating a view "in public":

> The very procedure of articulating a view in public imposes a certain reflexivity on individual preferences and opinions. When presenting their point of view and position to others, individuals must support them by articulating good reasons in a public context to their co-deliberators. This process of articulating good reasons in public forces the individual to think of what would count as a good reason for all.

Note this description presumes that the reasons evoked must be reasons acceptable for "all," where "all" seems to imply something like citizens generally, that is, citizens with potentially diverse comprehensive views and not simply, for example, the members of one's own church or synagogue or association within civil society. As to what will count as a good reason for all citizens in regard to political matters, it seems clear that principles specific to one's comprehensive view will not be acceptable reasons to others holding different views, and the process of thinking of what would count as good reasons for all is in part, already, a process of adhering to the limitations of public reason. But in dialogue within a religious association, one would expect an appeal to shared principles specific to a given comprehensive view and not generally shareable by all citizens. What is being argued for here might be termed a conception of a limited public domain, appropriately subject to something like public reason or general principles of reciprocity, within a larger public domain composed of associations in civil society, appropriately not subject to such principles (so that it makes no difference whether one calls the internal life of such associations "nonpublic," "private," or another "region" of the "public" domain).

In fact, Benhabib (1993, 42–3) points to exactly this kind of distinction between the reasoning "internal" to a given association and that which governs dialogue with citizens at large in the context of political debate. She considers the case of a Mormon traditionalist, convinced of the moral correctness of the practice of polygamy on the basis of "the dictates of the teaching of Joseph Smith." She then asks if such a Mormon could imagine a situation in which she had to justify to others who are not Mormons the morality of such a practice. The political parallel of this thought experiment concerns the recent example of those Mormons who have begun, as Benhabib phrases it, "to go public," seeking legal protection for the practice of polygamy:

> The fundamentalist Mormons have their private religious beliefs, but in the public arena of the liberal democratic state, they are using the secular language of moral tolerance, and the recognition of the civil rights of dissenting groups and minorities—they wish to be accepted by the state as "practitioners of a different lifestyle," accepting thereby the legitimate plurality of lifestyles in the liberal state (Benhabib 1992, 65).

Here a distinction is drawn between the "public arena" and the "private religious beliefs" of Mormons. But as Benhabib undoubtedly would acknowledge, these private beliefs are in fact associational beliefs, just as the Mormons themselves are an association in civil society, the same civil society whose associations are said to be privileged by a deliberative model of democracy (Benhabib 1996, 73). Hence the public domain is here characterized in just the restricted Rawlsian sense for which I have been arguing. First, the debate on the legal status of polygamy concerns legislating on a matter of "constitutional essentials." Second, the affected group is engaged in advocacy in the public domain by pressing its case in terms of political values and political principles, that is, *it is addressing its case to citizens at large*. But one would not expect the Mormons to apply the same principles or to argue in the same terms within their associations. There one might expect them to invoke the religious authority of Joseph Smith

[7] The religious appeals of some abolitionists are often cited in discussions about the limitations of public reason (Galston 1991, 115; Holmes 1988; Sandel 1994). First, the value being evoked—the basic equality of all persons—was a political value, not only embodied in the Declaration of Independence and the Constitution but also a central component of political debate from the time of the pre-Socratics onward. The abolitionists were appealing to religious beliefs to support a known political concept. Second, the idea of the equality of persons could hardly be said to be specific to a given faith in the way that, for example, the idea of the Trinity is.

[8] See, for example, the reasons invoked by Cardinal Bernadin (1986) in an article in which he argues against a right to abortion. His argument is presented in terms of the political values of public peace, the preservation of human rights, and accepted standards of moral behavior in a community of law. Rawls (1996, lvi) cites this article as an example of an argument against abortion that does adhere to the principles of public reason.

and his interpretations of the Bible. In such a venue, invocations of that kind are *reasonable* and satisfy a principle of reciprocity *within* a given association. This is precisely Rawls's (1993, 220) point in stating that the reason of associations in civil society "is public with respect to their members, but nonpublic with respect to political society and to citizens generally."

The Significance of a Limited Public Sphere

Deliberative democrats who privilege associations within civil society may well reject any distinction between the "internal life" of associations within civil society and associations viewed as participants in a wider public political debate addressed to all citizens. In this regard, Benhabib (1996, 76) maintains: "To say that when in civil society these individuals and associations are governed by one kind of reason, a nonpublic one, but that they have to respect the limits of public reason once they enter the political arena is inadequate, *for civil society is also public*." I have already noted how Benhabib herself appears to draw just such a distinction. What I would like to consider here are the dangers of failing to draw it.

Consider, for example, some of the general principles of discourse ethics as laid out by Habermas (1990, 86), what he terms the "universal and necessary communicative presuppositions of argumentative speech." These require that every subject with the competence to speak and act be allowed to take part in the discourse; everyone is allowed to question any assertion whatever, to introduce any assertion into the discourse, and to express his attitudes, desires, and needs (p. 89). According to Benhabib (1996, 78), "the deliberative model of democracy presupposes a discourse theory of ethics," which incorporates two basic elements of Habermas's "ideal speech situation": All are "equally entitled to take part in those discourses which are to determine the norms that are to affect their lives"; and "each individual has the same symmetrical rights to various speech acts, to initiate new topics, to ask for reflection about the presuppositions of the conversation, and so on." These Benhabib terms, respectively, principles of universal moral respect and egalitarian reciprocity. As has been noted, for Benhabib (1996, 80) the "public sphere is introduced as the embodiment of discursive democracy in practice," where the public sphere is defined as being composed of all the manifold associations within civil society.

If we consider some of the general principles listed by Benhabib as underlying the validity claims of a deliberative model of democracy—participation governed by norms of equality and symmetry, equal rights to interrogate and question topics of conversation, no prima facie rules restricting the identity of the participants or the agenda—then it is clear that many of the most important associations comprising civil society do not come close to fulfilling these requirements. First, associations within civil society often have a highly selective membership, a point Benhabib (1996, 76) herself acknowledges: "Civil society and its associations are not public in the sense of always allowing universal access to all." Second, many of these associations have "hierarchical" or "authoritarian" structures of discourse that do not meet such deliberative ideals as absolute symmetry and equal opportunity to initiate discourse and problematize matters. In regard to the concept of the public sphere as an embodiment of Arendtian freedom, Dana Villa (1992, 713) has remarked: "Not only coercion and violence, but all relations of hierarchy are excluded from this sphere." Yet, the modes of discourse or conversation in a church or synagogue, for example, often involve deference to the judgment and authority of individuals occupying certain roles, such as a priest or a rabbi. Some matters are often steadfastly denied a problematical status among participants in the "conversation," for example, whether a text is the word of God or whether God exists (although, of course, these matters also may come to be seen as problematical).

Third, and perhaps most significantly, according to Benhabib (1992, 42–3), discourse ethics presupposes a "postconventionalist moral system" characterized by a "moral reflexivity"—the ability to question radically all procedures of justification, including its own, and the requirement that the validity of normative practices be demonstrated with reasons that others who are not members of one's way of life can accept or find convincing. As such, moral postconventionalism is distinguished from moral conventionalism, based neither upon the ability to offer reasons acceptable to all nor upon the radical questioning of its own justificatory procedures. For Benhabib (p. 43), moral reflexivity and moral conventionalism are not compatible, although the rights associated with communicative ethics in a deliberative democracy would certainly not interfere with the practices of such "conventional" associations (to the extent that they do not infringe upon the rights and liberties of others). But the problem here concerns locating the "moral conversation" of a deliberative democracy within civil society, which presumably means *within* as well as *between* civil associations, when civil society is itself composed of numerous associations that practice "conventional" morality. To the extent that discourse ethics privileges voluntary associations in civil society, should such associations be expected to live up to a "postconventional" ideal of discourse ethics, and should they be judged by its standards?

Perhaps only certain associations are meant to be members of the "anonymous public conversation"—those which in some ways meet the demands of a discourse ethics. Benhabib (1996, 73) says that the procedural specifications of the model of discourse ethics "privilege a plurality of modes of association in which all affected can have the right to articulate their point of view." There is some ambiguity in this specification. Does it mean that only those associations are "privileged" in civil society "in which all affected can have the right to articulate their point of view"? The associations listed as being privileged by the discourse model are said to "range from political parties, to citizens' initiatives, to social movements, to voluntary associations, to consciousness raising groups and the

like" (p. 73). This does not appear to be selective, inasmuch as "voluntary associations" would include *all* associations in civil society (with the exception of the family).

In raising the question of the propriety of applying a postconventional ideal of discourse ethics to associations in civil society, we confront a paradox. The principles of discourse ethics, as analogously is Rawls's device of an original position, are principles of *universalization*. And according to Benhabib (1992, 42), the problem with the adherent of conventional morality is that "the kinds of grounds such a person will bring into the moral conversation will not be sufficiently universalizable from the standpoint of all involved." While not excluded from the moral conversation, such persons face a "rationality deficit"; there is a point in the conversation beyond which they cannot proceed: "Because the adherents of such moralities are willing to stop the conversation and because they have to withdraw from the process of reflexive justification in order not to let their world view crumble, their position is not comprehensive and reflexive enough" (Benhabib 1992, 43). To attain sufficient reflexivity, then, means a transformation from conventional to postconventional morality. And this in turn means argumentation from universalizable grounds, which presumably means *believing* in reasons based upon universalizable grounds and generalizable interests (assuming general principles are not presented hypocritically or for prudential reasons). The implications of this are, arguably, either the end of associations in civil society or their melding into one association à la Rousseau's ([1762] 1978, 109) General Assembly, in which "the more harmony there is in the assemblies, that is, the closer opinions come to obtaining unanimous support, the more dominant as well is the general will."

The reason for this, simply put, is that what is nonuniversalizable is the basis of difference, and discrete associations are founded upon a principle of difference. Neither the interest that unites bowlers nor the beliefs that unite Catholics are universalizable in a pluralist society. Churches are now viewed as associations within civil society rather than as part of the public domain, precisely because the beliefs and practices of particular religions are held to be nonuniversalizable in a pluralist society. If reciprocity is to "become the norm among humans as concrete selves and not just as juridical agents," in the strong "postconventional" sense in which Benhabib (1992, 60) appears to intend the principle of reciprocity to apply, then all forms of conventional morality will become postconventional, and all reasons will ultimately be universalizable ones. In regard to pluralism and the public realm as a "discursive space," Villa (1992, 714) has remarked: "The public realm understood as a discursive space characterized by symmetry, nonhierarchy and reciprocity, both presupposes, and makes possible, plurality and so provides the opportunity for a politics based on mutual recognition and respect for difference." What I have argued is that the public realm, thus understood, *if located in the associations of civil society*, does not respect difference and in fact endangers plurality.

The problem is not with a principle of universalization per se, for the limitations of public reason are also a form of such a principle, but with where it should be applied. The value of a more limited conception of the public domain is that it can acknowledge the validity—and perhaps even the value—of nonpublic associations based upon nonuniversalizable interests and modes of discourse. This very feature of nonuniversalizability, I have argued, is the impetus for the formation of discrete and varied associations to begin with. The ideal of public reason applies in those circumstances in which citizens are publicly addressed *as citizens* concerning political issues, not as church members or members of a particular association. Put another way, the "association" addressed by public reason is the association of political society as a whole, what Rawls (1971, 527) has termed "the social union of social unions." The process of adhering to the limitations of public reason in such circumstances is, I have argued, not a particularly unnatural or demanding process; it is the process cited by Benhabib whereby persons try to convince "others," such as those of a different faith, by appealing to reasons that can be convincing reasons for them, that is, for "all."

It may be objected that the argument thus far has displayed a good deal of sociological naiveté. If nonpublic life is not characterized in some ways by principles of egalitarian reciprocity, then how can we expect public life to be so characterized? Certainly, this is a matter of degree. We would expect a society in which all associational life, including personal relationships, is characterized by racial division to be race conscious and racially divided in public life as well. As I have noted above, the nonpublic realm changes in regard to changing societal norms and vice versa. From a sociological perspective, they are not at all completely distinct entities or domains. For the most part, associations of all kinds are "semipermeable" bodies within the larger democratic culture. At the same time, however, a good deal of associational self-selectivity, nonreflexivity, hierarchy, and deference to authority (of a religion, a text, a wise person, a teacher, a guru, God) is also compatible with democratic self-rule and adherence to principles of egalitarian reciprocity in public life.[9] Put another way, public postconventional morality is compatible with considerable conventional morality in nonpublic associational life. In asserting this, I do not intend to extol the virtues of "conventional" morality, but it is my hope to avoid unnecessary criticism of such moralities as presently embodied in numerous and varied forms of associational life.

[9] Recent work by Verba, Schlozman, and Brady (1995, 19) demonstrates how involvement in religious associations can promote the development of various "civic skills" associated with democratic citizenship: "organizational and communications skills that are relevant for politics and thus can facilitate political activity." The authors argue that these civic skills are "more or less fungible" and can be effectively transferred from the various "nonpublic" venues in which they are developed (in this case, religious associations) to public life, enhancing political participation (p. 520).

RAWLS'S ROUSSEAUISM AND THE COLLAPSE OF THE PUBLIC-NONPUBLIC DIVIDE

Although for Rawls public reason entails that citizens forgo arguing or reasoning on the basis of their comprehensive views in the limited circumstances defined as the public domain, this by no means entails that these very same views do not have a fundamental political role to play. As noted, the very views which are to give way in public life ideally guarantee the stability of the regime by forming the basis of an overlapping consensus. The political conception of justice is "a module, an essential constituent part that fits into and can be supported by the various reasonable comprehensive doctrines that endure in the society regulated by it" (Rawls 1993, 12). Thus, the political conception is in some way related to the broader moral and ethical values of citizens' comprehensive views and is thereby given a deeper or "ultimate" justification as part of what citizens view as the "whole truth." But given that citizens affirm a plurality of comprehensive views, the bases of deeper justification will themselves be plural: "It is left to citizens individually—as part of the liberty of conscience—to settle how they think the values of the political domain are related to other values in their comprehensive doctrine" (Rawls 1993, 140).

For Rawls, however, not just any comprehensive view can be a part of an overlapping consensus, only "reasonable" views: "We look for a consensus of reasonable (as opposed to unreasonable or irrational) comprehensive doctrines. The crucial fact is not the fact of pluralism as such, but of reasonable pluralism" (Rawls 1993, 144). It is the appropriateness of this division of comprehensive views into the reasonable and the unreasonable that I now want to question. Such a classification, I argue, is a problematical step in Rawls's theory and has the effect of ultimately collapsing the public-nonpublic distinction upon which his theory critically depends. To see why this is so, consider what exactly, according to Rawls, makes a comprehensive doctrine reasonable. He appears to offer two very different sets of criteria. The first concerns what might be called general features of coherence or rationality. Thus, a reasonable doctrine

> covers the major religious, moral, and philosophical aspects of human life in a more or less consistent and coherent manner. It organizes and characterizes recognized values so that they are compatible with one another and express an intelligible view of the world. . .it normally belongs to, or draws upon, a tradition of thought and doctrine. Although stable over time, and not subject to sudden and unexplained changes, it tends to evolve slowly in the light of what from its point of view, it sees as good and sufficient reasons (Rawls 1993, 59).

The first question that may be raised is: How many religions (or for that matter philosophical) views actually fit this characterization of the reasonable? I would hazard that there never has been a religious or philosophical system without major inconsistencies, incoherences, and incompatibilities as well as a general clash of values on one level or another. Some religious doctrines emphasize faith over reason to such an extent that they codify, as an article of faith, a certain hostility toward rationality, such as Tertullian's *credo quia absurdum* (I believe because it is absurd) and Kierkegaard's "blind leap of faith." Furthermore, why are tradition and gradual evolution cited as features of the reasonable? The history of religion is punctuated by the relatively sudden appearance of new faiths: Lutheranism, for example, did not evolve gradually from Catholicism. Even in the case of science, which in some ways embodies the reasonable as an exercise of theoretical, as opposed to practical, reason, development has been punctuated by dramatic shifts in thought and doctrine, often not entirely explainable in terms of what has preceded (Kuhn 1962).

Second, and more significant, is the issue of whether a doctrine that seeks as far as possible to limit itself to the domain of the political and political values should pass judgment on what general structural features a comprehensive doctrine must have in order to count as reasonable and, hence, be part of an overlapping consensus. "Reasonable" here is not being used as a political or even moral concept, for its opposite is something like inconsistent or "irrational." Consider, for example, Rawls's (1993, 60) assertion that "in a particular case someone may, of course, affirm a reasonable doctrine in an unreasonable way, for example blindly or capriciously." Thus, Kierkegaard's blind leap of faith could be seen as an instance. Therefore, judgment is being passed from a nonpolitical perspective—a blind leap of faith endangers no one's rights—on the proper way to come to hold a religious belief, in direct violation of Rawls's insistence upon positing a domain of nonpublic reason that lies outside the purview of a strictly political conception. Such a breach in the public-nonpublic divide entails a corresponding violation of Rawls's (1993, 150) avowed hope "neither to assert nor deny, any particular religious, comprehensive, philosophical, or moral view, or its associated theory of truth and the status of values." Certainly, a belief as to how one comes to hold a true religious belief is an important component of any religion's theory of truth.

In addition to these general features, Rawls's (1993, 243) definition of a reasonable doctrine does have moral aspects: A doctrine is reasonable which can support a reasonable balance of political values. Before attempting to explicate this description, it must be noted that Rawls consistently talks about reasonable or unreasonable comprehensive doctrines as opposed to the reasonable or unreasonable *use* of a doctrine. He does characterize as unreasonable (p. 138) those who seek to enforce the doctrine *extra ecclesiam nulla salus* (beyond the church there is no salvation) as a political doctrine, in the sense of making conformity to the church in question required by law. He also asserts as vital to the idea of political liberalism that, while it may be unreasonable to use the power of the state to enforce a religious doctrine, this does not necessarily mean the doctrine is incorrect. This is all well and good, and consistent with the aims of political liberalism, but Rawls does not always adhere to this principle.

Rawls (1993, 152–3) considers the case of "rationalist believers" who seek to establish their particular religion on the grounds that it is a truth readily accessible to all through reason. To them, he says, "we need not say that their religious beliefs are not true, since to deny that religious beliefs can be publicly and fully established by reason is not to say that they are not true." Yet, in the next sentence he asserts: "Of course, *we do not believe the doctrine believers here assert*, and this is shown in what we do. Even if we do not, say, hold some form of equal liberty of conscience, our actions nevertheless imply that *we believe the concern for salvation does not require anything incompatible with that liberty*" (p. 153, emphasis added). But does our unwillingness to consider enforcing the doctrine as law (or "establishing" it) really imply that we think the doctrine is false and, what is more, false *as theological doctrine*, that is, not required for salvation?

Rawls's (1993, 152) claim that the falsity of a practice is implied by "what we do," in the specific sense of how we adjudicate, has unfortunate consequences for political liberalism's hope "to try to bypass religion and philosophy's profoundest controversies." It is important to note that there are two ways in which such controversies can be "bypassed" as a political matter. One is simply to remove from the political agenda altogether any issues which may give rise (or have given rise) to such controversies. This is rarely a viable alternative. How would one remove from the political agenda debates concerning abortion, capital punishment, doctor-assisted suicide, same-sex marriages, and a host of other issues with profoundly controversial philosophical and religious implications? The other way to bypass issues is a constraint upon reasons that can be invoked to justify public policies, or laws, or principles of justice, that is, by adhering to something like the principles of public reason.

For example, the establishment clause of the First Amendment is held to require that legislation be justified on the basis of legitimate secular (i.e., political) governmental interests (*Lemon* v. *Kurzman*, 1971). Simultaneously, the free exercise clause has long been held to bar from legal consideration the truth or falsity of religious doctrines or beliefs. This principle was given explicit statement in *United States* v. *Ballard* (1944), in which Justice Douglas's majority opinion stated that the First Amendment barred submission to the jury of "the truth or verity of respondent's religious doctrines or beliefs"; all that the jury could consider was whether the respondents believed the claims they made. Douglas commented in *Ballard* (pp. 86–7):

> Men may believe what they cannot prove. They may not be put to the proof of their religious doctrines or beliefs. [The] miracles of the New Testament, the Divinity of Christ, life after death, the power of prayer are deep in the religious convictions of many. If one could be sent to jail because a jury in a hostile environment found these teachings false, little indeed would be left of religious freedom.

In free exercise cases, if a religion is adversely affected by legislation and a free exercise claim is denied, then the burden is justified in terms of the importance of legitimate governmental interests, *not* in terms of the court's passing judgment on the falsity of the religion in question. (See, e.g., *Jacobson* v. *Massachusetts* [1905], in which the compulsory vaccination of school-aged children of Christian Scientists was sustained against a free exercise challenge on the basis of the state's compelling interest in protecting the health and safety of the community. As the Court noted in *Prince* v. *Massachusetts* [1943, 158], "the right to practice religion freely does not include liberty to expose the community . . . to communicable disease.") Conversely, if religion is "advanced," in the sense of removing a governmental burden seen as inhibiting its free exercise, the justification is presented in terms of the value of the free exercise of religion in general, not in terms of the truth of the particular religion in question. Thus, in *People* v. *Woody* (1964, 72), the court held unconstitutional the application of state criminal statutes to Navahos using peyote in religious ceremonies, on the grounds that "peyote . . . is the *sine qua non* of the defendants' faith." It did not say: "We have determined that the practice is pleasing to God." In outlawing the use of peyote for all citizens in a general nondiscriminatory manner, the state was not passing judgment on the propriety of its use *as part of a religious ceremony*; in making an exception to this law, the state was not endorsing the practice from a theological perspective.

Michael Sandel (1994) appears to question the possibility of the kind of bypassing (or "bracketing") here argued for, and he implies that adjudication affecting religious belief has in fact decided, albeit often surreptitiously, its truth or falsity. He writes in relation to abortion,

> But whether it is reasonable to bracket for political purposes the moral and religious doctrines at stake depends on which of those doctrines is true. If the doctrine of the Catholic Church is true, if human life in the relevant moral sense does begin at conception, then bracketing the moral-theological question is far less reasonable than it would be on rival moral and religious assumptions. The more confident we are that fetuses are, in the relevant moral sense, different from babies, the more confident we can be in affirming a political conception of justice that sets aside the controversy about the moral status of fetuses (Sandel 1994, 1,778).

This formulation is based upon a misunderstanding. In regard to resolving the abortion issue, Rawls does not assert that the relevant moral status of fetuses should or can be bypassed or "set aside." Rather, as far as possible, one should try to resolve the matter by public reason or in political terms (Rawls 1993, 243–4), which, as Rawls argues throughout, *are also moral terms*. The political is a domain of the moral, and a political conception of justice is also a moral conception (Rawls 1993, 13, 174–5). But how can one resolve such a question in political terms? Note that public reason includes common sense and the conclusions of science when not controversial. What does "common sense," or a kind of basic empiricism that *excludes revelation*, tell us in this regard? It tells us that a fertilized egg cell up to a certain point appears and acts nothing like a distinct human being (in fact, before a

certain point it cannot be said to act at all). From this we may infer that it is not human in the relevant and limited political-moral sense, at any rate as far as preserving basic human rights go. This is undoubtedly crude. It is certainly not meant to be any kind of decisive argument (although it may be all that we can or should go on for political purposes).

Consider what it means to say that the resolution of the controversy depends upon showing that the *Catholic doctrine* is false (or true): "If the Catholic doctrine is right. . .it is not clear why the political values of toleration and women's equality. . .should prevail. . . . It [the case for abortion] must engage rather than avoid the comprehensive moral and religious doctrines at stake" (Sandel 1994, 1,778). Catholic belief that status as a person begins at conception is an article of faith based upon a particular interpretation of the Bible (seen as a divine document), revelation, and theological doctrine. How are we to prove it wrong? Whether a fertilized egg cell or a fetus is a person is a normative question not amenable to anything like scientific refutation or verification (as opposed, e.g., to the heliocentric doctrine of the solar system, which the Catholic church initially opposed). To say that we must strive to "engage," that is, prove right or wrong, theological doctrine whenever adjudication touches upon theological matters would mean, for example, that we should determine from a theological perspective the propriety of the use of peyote as part of a religious ceremony. Yet, when Rawls asserts that our not believing the doctrine which rationalist believers assert "is shown in what we do," he seems to put in doubt the possibility of just this kind of "bypassing" and to threaten the constitutional divide between the public and private domains.

Rawls is not entirely consistent concerning whether our actions imply that we do believe such doctrines are false. But what seems textually unequivocal (or at any rate, less equivocal) is that such doctrines are classed as unreasonable. This can be seen by returning to Rawls's description of a reasonable doctrine. Rawls (1993, 243) asserts: "The only comprehensive doctrines that do not accord with public reason on a given question are those that cannot support a reasonable balance of political values on the issues that it raises." To illustrate, Rawls considers abortion and identifies three primary political values as being at issue: respect for human life, the reproduction of political society over time, and the equality of women citizens. According to Rawls, "Any reasonable balance of these three values will give a woman a duly qualified right to decide whether or not to end her pregnancy during the first trimester. . .any comprehensive doctrine that leads to a balance of political values excluding that duly qualified right to abortion in the first trimester *is to that extent unreasonable*" (p. 243, emphasis added). Furthermore, he notes that "a comprehensive doctrine is not as such unreasonable because it leads to an unreasonable conclusion in one or even in several cases. It may still be reasonable most of the time." (p. 243).

Rawls is not saying that a view proposed as a public matter which is derived from such a comprehensive doctrine would be an unreasonable *application* of that doctrine to public political matters; rather, any comprehensive doctrine that would not support such a balance of values is "to that extent" an unreasonable doctrine. This means that even when an individual does not vote on the basis of her comprehensive views, when she believes that it would be unreasonable to try to institute such views as a political matter, by the tenets of political liberalism her views can still be classed as unreasonable. Thus, a Catholic who believes as an article of faith that status as a person begins at conception, but who does not believe that such a view can reasonably be defended by public reason and therefore does not, for example, seek to overturn *Roe* v. *Wade* (1973) (assuming this to constitute the correct balance of the political values at issue), will still be held by the tenets of political liberalism to hold a comprehensive view which is *to that extent* unreasonable. Paradoxically, so characterized, she would be a reasonable person but the holder of an unreasonable doctrine. But is the doctrine itself unreasonable, and should it be classified as such by an avowedly political conception? Or is its proposed *use* an unreasonable one, that is, *its use as a political doctrine*?

Consider what it means to say that a doctrine such as *extra ecclesiam nulla salus* (beyond the church there is no salvation) is an unreasonable doctrine. In one form or another, this is precisely the doctrine held by the Catholic church and numerous other denominations at the present time. For example, according to the *New Catholic Encyclopedia*:

> The necessity of the church for salvation is illustrated in the Fathers by means of Biblical figures such as that of the ark in the deluge. . . . Origen is the first to formulate the dogmatic axiom **Extra Ecclesiam nulla salus* (EnchPatr 557). . . . If the Catholic church is necessary for salvation, it follows that no one can be saved unless one belongs to the Church in some way or other (Eminyan 1967, 995–7).

Perhaps such a doctrine, which may appear to some as morally objectionable per se, conceals what is at issue here. One must keep in mind that there are plenty of religious prescriptions about which one justifiably may say it would be unreasonable to make them into law; but one would not be justified in saying, at least as a political matter, that they are unreasonable per se, that is, as religious doctrines applied within their own appropriate (i.e., nonpublic) domain. For example, in a religiously pluralistic society, it certainly would be unreasonable to propose that Talmudic dietary restrictions become national law, but are they therefore "unreasonable," or should they be classified as such? Religions contain numerous articles of faith affecting social relations among coreligionists: *Extra ecclesiam nulla salus* is just such a tenet. Those who hold this belief may accept the illegitimacy of having it—or political principles derived from it—imposed upon or applied to society at large; they may accept that, in Locke's words, ([1689] 1983, 31) "excommunication neither does, nor can, deprive the excommunicated Person of any of those Civil Goods that he formerly possessed." But as an associational belief in the non-

public domain and a manifestation of nonpublic reason, its truth or falsity, as well as its "reasonableness," should be outside the purview of an avowedly limited political conception.

I have observed how, according to this classification, a Catholic who adheres to public reason would be a reasonable holder of an unreasonable doctrine. But she may not be a reasonable person according to political liberalism, because of the very particular way in which Rawls defines a reasonable person. That definition raises all the same difficulties associated with the definition of a reasonable comprehensive doctrine as well as the further and even more perplexing question of whether an unreasonable person can still act reasonably. According to Rawls (1993, 48–54), one basic feature of reasonable persons is their readiness to propose and willingly abide by principles and standards that are fair terms of cooperation, given assurance that others will do likewise. So in this regard, the individual mentioned above who holds an "unreasonable" doctrine but believes it would be illegitimate to impose that doctrine upon others would be a reasonable person. Rawls adds, however, that reasonable persons also must recognize what he calls the "burdens of judgment" (pp. 54–8). These are meant to explain why, under conditions of freedom and the good faith exercise of our powers of reason and judgment, we generally end up with a plurality of comprehensive views rather than convergence upon one accepted view. As defined by Rawls (pp. 55–7), the burdens of judgment include such factors as the difficulty of weighing the relevant considerations in matters of "ultimate truth," the conflicting and complex nature of relevant evidence, and the influence of the particularities of our life situation on our view of the world and of the proper balance of values.

The problem with this formulation is that most religions, including those which accept the general principles of liberty of conscience and free faith, do not accept anything like the burdens of judgment. In fact, it is arguably necessary for any religion, if it is to present itself as a sure and infallible guide to the truth, precisely to deny them. As Leif Wenar (1995, 44) has noted: "A religious doctrine—as a purportedly authoritative guide to moral requirements and/or salvation—characteristically presents itself as universally accessible to clear minds and open hearts." Catholic doctrine, for example, explicitly denies the reality of anything like the burdens of judgment. Rather, it holds that, through revelation, "those religious truths which are by their nature accessible to human reason can be known with solid certitude, and with no trace of error, even in the present state of the human race" (*Dogmatic Constitution on Divine Revelation*, article 6, in Abbott 1966, cited in Wenar 1995, 44). So the hypothetical Catholic who abides by public reason and a principle of reciprocity would not be a reasonable person, because, let us assume, that as a believing Catholic she does not accept the burdens of judgment. In this case, we have an unreasonable person acting reasonably. But if an unreasonable person, or the holder of an unreasonable doctrine, can act reasonably *for political purposes*, why classify doctrines—or persons—in this manner at all?

By such a classification, judgment is being passed upon religious doctrine per se in violation of Rawls's own division between the realms of public and nonpublic. If a theory, as well as a political regime, is committed to respecting the plurality of comprehensive views existing within society, then it appears that very little is to be gained by such a socially divisive classification as that between reasonable and unreasonable comprehensive views. Furthermore, as the abortion example shows, Rawls does not consider any balancing of political values as reasonable and appropriately so. But what appears questionable in this example is the specificity with which Rawls identifies what constitutes the correct balance on a given political question. In discussing the idea of public reason, Rawls maintains (1993, 226): "Accepting the idea of public reason and its principle of legitimacy emphatically does not mean, then, accepting a particular liberal conception of justice down to the last details of the principles defining its content. . . . Public reason does not ask us to accept the very same principles of justice." Here, however, for a comprehensive doctrine to be reasonable (in regard to the issue in question) and not "run afoul of public reason," it appears that it must in some way support the "last details" of the political principles.[10]

Why, then, does Rawls insist upon classifying comprehensive doctrines as reasonable and unreasonable? The answer, as I have noted, concerns his conception of an overlapping consensus—which ensures the stability of the regime—as a consensus only of reasonable doctrines. The implications of this are enormous. The point is no longer that all will accept the political conception as a "common ground," whatever their various comprehensive views, or will accept the reasonableness and necessity of a separation of church and state; rather, *all citizens will (ideally) hold only comprehensive views that support the political conception*. Thus, for all its commitment to taking seriously the fact of pluralism and the irreconcilable differences of diverse comprehensive views, and to separating public reason from nonpublic reason, Rawls's conception of an overlapping consensus points to a rather surprising unity, even homogeneity, of comprehensive views. Whatever their differences, all the comprehensive views held by citizens in society will (ideally) support a rather precise balance of political values. More specifically, the balance of moral and ethical values as articulated internally to each "reasonable" comprehensive view will support the appropriate balance of political values. It is this aspect of political liberalism that I call Rawls's "Rousseauism."

[10] Rawls (1996, lv–lvi) has clarified what he intended by this example in the Introduction to the paperback edition of *Political Liberalism*. He says that his point was not to argue for a right to abortion in the first trimester but simply to show what a reasonable balance of the relevant political values might look like. Yet, this in no way affects the present criticism. I am questioning the specificity with which Rawls identifies what would constitute a reasonable balance on a given issue, not the content of the balance.

In the *Social Contract*, Rousseau ([1762] 1978, 131–2) writes:

> Those who make a distinction between civil and theological intolerance are mistaken. These two intolerances are inseparable. It is impossible to live in peace with people whom one believes are damned. . . . Wherever theological intolerance exists, it is impossible for it not to have some civil effect. . . . Now that there is no longer and can never again be an exclusive national religion, one should tolerate all those religions that tolerate others insofar as their dogmas are in no way contrary to the duties of the citizen. But whoever dares to say *there is no salvation outside of the church* should be chased out of the State, unless the State is the church, and the prince is the pontiff.

Rousseau, then, denies the possibility of a stable regime if persons hold religious beliefs seen as conflicting with the virtues necessary for good citizens. Thus, in important ways, he denies the possibility of separating the public and nonpublic spheres: Only those religious doctrines are to be tolerated which are deemed to teach nothing contrary to the duties of citizens. While Rawls seems to embrace the possibility of the separation of these spheres, his idea of an overlapping consensus seems in surprising agreement with Rousseau. Only those comprehensive doctrines which support the proper balance of political values and the virtues of political liberalism will be part of an overlapping consensus; such citizens will constitute, as it were, the foundation and "true" citizens of political society, that is, its General Will.

Furthermore, Rawls (1993, 60) asserts that "reasonable persons will not think it reasonable to use political power, should they possess it, to repress comprehensive views that are not unreasonable, though different from their own." The implication is that they may think it reasonable to suppress unreasonable views and, like Rousseau, "chase them out of the state." In fact, Rawls believes "comprehensive doctrines that would suppress *if they could* liberty of conscience and freedom of thought" must be contained "like war and disease" (p. 64, emphasis added). What does "if they could" mean here? Is it counterfactual, that is, what the adherents of such views would do if they could, even if they are not attempting to do this? Or what it is assumed the adherents of such views would do if they could, because such political consequences are *implied* by, or *derivable* from, the comprehensive views they hold? The implications of this seem quite illiberal, for one must ask whether the principles of liberty of conscience and toleration are in danger of being compromised.

CONCLUSION

For Benhabib, "full" participants in the public dialogue must accept a postconventional morality. For Rawls, "full" participants in an overlapping consensus must accept a precise balance of political values and highly specific and controversial views about human reason (the burdens of judgment). For Benhabib, only followers of a postconventional morality, freed from a "rationality deficit," can stay in the conversation "all the way." For Rawls, the crucial fact is not the fact of pluralism as such, but of reasonable pluralism. The problem with both these formulations is that modern democratic society as we know it is composed of numerous associations and individuals who embrace a conventional morality, do not support a precise "reasonable" balance of political values, and reject the burdens of judgment. Yet, these same persons and groups can and do accept basic democratic principles, the separation of church and state, and liberty of conscience (though, of course, there will always be some in every democratic society who do not accept these principles). How is this possible?

Liberalism claims that this can be achieved through bifurcation of a human being into a public and nonpublic entity, into a *homme* and *citoyen*. This is and will remain a difficult and delicate project involving a constant reevaluation of boundaries, for these two identities are never entirely distinct. We are all simultaneously persons and citizens. There is a constant temptation, however, to erase this distinction with all its inevitable tensions, even among those liberal and democratic theorists who in other respects appear to accept such a bifurcation and embrace a division between the public and nonpublic domains. To overcome bifurcation (*die Aufhebung der Entzweiung*, to borrow a Hegelian phrase) would require our nonpublic values, beliefs, and modes of discourse to cohere with our public (general-universal) values, beliefs and modes of discourse. The problem is that the "project" of such cohesion fails to respect difference and ultimately endangers pluralism. It involves collapsing, in one way or another, the boundary that separates the public and private. As Shklar (1989, 25) warned, the importance of maintaining this boundary, which while "a shifting line" is "not an erasable one," must never be forgotten.

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Partisan Cues and the Media: Information Flows in the 1992 Presidential Election

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Electoral research acknowledges the growing significance of the mass media in contemporary campaigns, but scholars are divided on the nature of this influence. Using a unique database that includes both media content and public opinion, we examine the flow of partisan information from newspapers to the voters and assess the press's role in electoral politics and citizen learning. We find that the American press does not present clear and singular messages about presidential elections but, rather, multiple messages about the candidates and the campaign. In addition, perception of the information is shaped as much by an individual's political views as by the objective content. Despite the mixed messages, we find that a newspaper's editorial content is significantly related to candidate preferences in 1992. These results challenge the minimal effects interpretation of the media, because local newspapers can play a significant role in providing cues that influence voters' electoral calculus.

Most of what voters learn about the political process and contemporary events is mediated through a variety of institutional and individual information sources. Most people have not met their elected representatives, attended a government hearing, or read the legislative proposals under consideration by Congress. Nevertheless, people form impressions and evaluations of these actors and events through the information provided by intermediaries, such as the media, political organizations, and people with whom they discuss politics. The most regularly used information source, by virtually all accounts, is the mass media, especially television and the press.

The media's role as an intermediary is most evident at election time, when the media are the primary conduits for information on the campaign. Few voters attend a rally or have direct contact with the presidential candidates or their representatives. Instead, information presented in the media provides people with cues about the policy positions, qualities, and abilities of the candidates. The media also report on the progress of each campaign, including how the American public is reacting to the candidates and their messages. From this information, as well as other sources, the public forms its images of the candidates and its voting choices.

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This research was supported by a grant to the authors from the National Science Foundation (SES 91-23519); Russell Dalton and Holli Semetko received supplemental support from the Joyce Foundation for the media data collection. We want to acknowledge the foundation's support for this research and Professor Semetko's willingness to share these results. A previous version of this article was presented at the annual meetings of the American Political Science Association, San Francisco, September 1996. We would like to thank Lee Becker, Audrey Haynes, Shanto Iyengar, William Koetzle, Ruediger Schmitt-Beck, Holli Semetko, and the anonymous reviewers for their comments on this research.

How does this learning process work? Some analysts posit that the media have a direct influence on the public by reinforcing partisan predispositions or persuading voters to support particular candidates (e.g., Bartels 1993, Noelle-Neumann 1984, Zaller 1996). The predominate position in the literature, however, is that the media affect what factors to consider in judging the candidates by the way they present the topics of the campaign (Iyengar 1991, Iyengar and Kinder 1987, Patterson 1993). The media thus shape how the public thinks about the candidates rather than influence candidate preferences per se. Despite widespread acceptance of the media's important role in disseminating political information, scholarship remains divided on the central questions about the nature and effect of this information.

Our research addresses the question of media influence from a methodological perspective different from the one that has dominated scholarly research in recent years. In a tradition largely neglected since the days of the Columbia studies of voting (Berelson et al. 1954, Lazarsfeld et al. 1948; but see Patterson 1980), this article examines the direct persuasive influence of the media. Our specific interest is whether the content of press reports affects partisan preferences at election time.

It is curious that media research has not focused on the press, where variation in media content is most likely to be found. Instead, most studies have concentrated on television coverage of campaigns. Opinion surveys routinely show that television is most often cited as a source of political information, and its visual power has drawn scholars to research this medium. Yet, television news coverage is less likely to show systematic partisan bias in election content because of its history of government regulation, its origins in an advertising rather than a partisan culture, and its need to address a national viewership. Since the three networks have developed a largely impartial orientation, television seems less likely to influence partisan preferences directly. One can argue that partisan cues are

more likely to be found in newspapers because of their greater variety in ownership, differences in circulation bases, and earlier traditions of party-aligned reporting. Yet, studies of newspaper coverage of campaigns have not focused on variation in partisan content, and no comprehensive national study of newspaper coverage of a presidential election has ever been undertaken.

Our research represents the first national examination of how the print media actually presented a presidential campaign and how this information was perceived by voters and potentially influenced their preferences. We assembled a nationally representative collection of newspaper coverage of the 1992 presidential election. These data enable us to describe and analyze the political information that Americans received from their daily newspapers as well as the variation in coverage across the nation. With a parallel survey of the readers of these newspapers, we also assess how individuals perceived the political cues the press provided on the candidates and issues and whether this information influenced voting choices.[1]

Even more important, by examining the complete intermediation process for newspapers—from the content of the media to its effect on voters—we can test general theories of political learning and the nature of media influence. For instance, there is basic disagreement on the accuracy with which people perceive the political cues emanating from the media and on the factors that influence these perceptions (Neuman 1986, Zaller 1992). We can examine this linkage with our empirical evidence. In addition, the literature is unclear about how the information provided by the media is integrated into predispositions and thus influences political preferences. Our merger of objective media content and attitudinal data enables us to examine this complex process.

The findings of this study speak to several important questions in the current debate on the media's role in American electoral politics: the content of the press's coverage of presidential elections, the potential for media bias in election coverage, and the influence of newspapers on their readers. Because of the public's reliance on the media as an information source, the answers to these questions address an essential aspect of the democratic process: the role of the press as information provider.

TRACING INFORMATION FLOWS FROM THE PRESS

Early communications scholars feared that the modern mass media would be a powerful instrument of partisan persuasion and propaganda. These concerns lessened with the first empirical studies of the media's influence in elections (Berelson et al. 1954, Lazarsfeld et al. 1948). The results gave rise to the so-called minimal effects view: The media do not appear to exert a major persuasive influence on partisan preferences (Klapper 1960). The contemporary view is that media influences are primarily the cognitive effects of agenda-setting and framing how events and candidates are perceived (Iyengar and Kinder 1987, McLeod et al. 1994).

At the same time, other researchers have continued to explore the media's potential for persuasion. Robert Erikson (1976) found that a newspaper's editorial endorsement was significantly related to county-level voting patterns. John Robinson (1974) and Steven Coombs (1981) also documented a direct relationship between newspaper endorsements and the voting patterns of readers. Elisabeth Noelle-Neumann (1984) claimed that the media can have a strong influence on voting preferences when their message is clear and consistent. Larry Bartels (1993) showed that measurement error may significantly attenuate estimations of media effects, and he argued that the direct influence is much stronger than previously recognized. John Zaller (1996) provided new evidence that media content can affect policy preferences and political evaluations. In summary, there is evidence that media content can have a direct influence on public opinion and, specifically, voting preferences.

The First Stage

Our research traces the flow of political information from newspapers to the voter and then assesses the effect of this information on partisan choices. This requires that we examine the flow through several distinct stages.[2] The process begins with the information provided by the press. At this stage, we are concerned with clarity and direction of the political signals disseminated through the media. The strength of political cues in a newspaper's election reporting presumably affects the accuracy of their reception and ultimately their effect.

The nature of the information provided through the media also has fundamental implications for its potential persuasive effects. For example, Zaller (1992) and others (Noelle-Neumann 1973, Ross et al. 1976) argue that uniformly one-sided messages ("one message model") promote conformity to the message and are powerful sources of persuasion. Differentiated messages (Zaller's "two message model") are cognitively more complex and force receivers to search for cues about which messages to believe. Thus, the nature of political messages provided by the press has important consequences for the direction and degree of the public's response. By examining the content of the press's coverage of election campaigns we can determine the initial characteristics of this information process.

Surprisingly, prior research has not produced systematic information on the degree to which American

[1] Another component of this project is the cross-national replication of the basic design by research teams in Germany (Max Kaase, Hans-Dieter Klingemann, Manfred Kuechler, and Franz Urban Pappi), Great Britain (John Curtice, Anthony Heath, and Roger Jowell), Japan (Hiroshi Akuto), and Spain (Richard Gunther, Bradley Richardson, and Jose Ramon Montero).

[2] This is a simplified variant of McGuire's (1985) multistep process of information flow. We feel, however, that the three stages we examine constitute the core of the process and represent major empirical elements of media influence.

newspapers provide clear partisan cues and evaluative information to their readers. Several studies describe the coverage of the presidential candidates in the elite press and track these trends over time (e.g., Graber 1993, chapter 6; Stempel and Winhauser 1991), but they are limited in several respects. Most research focuses on only a few elite newspapers or local newspapers to lessen the burden of coding media content. Although the *New York Times* may consider that it publishes "all the news that's fit to print," it is read by only a small fraction of the American public. The typical voter reads a local newspaper with limited circulation, and these local papers may vary considerably in their partisan cues. Thus, to assess the information provided by the media, one needs to evaluate a sample of newspapers that represent the range of American media.

In addition, the coding of the information content of the press has not been fully satisfactory. Political communications research often relies on surrogates or partial measures of content. For instance, several analysts have used a newspaper's editorial endorsement as a measure of its partisan content (Coombs 1981, Erikson 1976, Robinson 1974); others have explicitly excluded editorial material (e.g., Stempel and Windhauser 1991). Studies of news content also have variously measured only headlines or articles appearing on the first page. Because of these different approaches, it is not surprising to find that prior research yields ambiguous conclusions about what partisan cues the press provides. Statistics on editorial endorsements show that fewer newspapers now explicitly endorse a presidential candidate (Stanley and Niemi 1994), and there is evidence of a general decrease of partisanship among the contemporary press (Rubin 1981). Conversely, others argue that the press has become more openly critical of politicians and has created unfavorable candidate images (Sabato 1991), although substantial empirical evidence of these effects has been lacking. Campaign organizations, often all of the parties and candidates, routinely claim that the media are projecting biased images of their candidate (see discussion in Patterson 1993, chapter 5). What is needed is a full and direct assessment of the partisan cues provided by the press so that we can generalize about the nature of election coverage.[3]

The Second Stage

The second step in the process of information flow involves readers' perceptions of the objective content of the media. How accurately do readers assess the partisan content of their daily newspapers? The primary source of reader perceptions should be the objective information provided by the media, but that is potentially diverse. There is an ebb and flow to election campaigns, so a single source may report positive news on a candidate one day and negative news the next. Even the same event is open to multiple interpretations, and these contrasting views may be expressed in a single article or separate reports. Furthermore, newspapers often print several different campaign articles in the same issue, and the partisan direction of each may vary. There is also variation between the news coverage of a campaign and what appears on the editorial pages. The interaction between news and editorial reporting may affect the clarity of the message the press conveys. We will explore how alternative sources of political cues, such as news coverage versus editorial content, shape a paper's partisan image among its readership.

Beyond the objective information, a reader's own characteristics may determine his or her perceptions of the media's message (Becker and Kosicki 1995). For example, research on information flows often shows that individuals project their predispositions onto information sources (or screen out opposing messages), unless the source provides an unavoidable message (Klapper 1960, McGuire 1985, Zaller 1992). Yet, research has found a much different process at work for media usage. Robert Vallone and his colleagues (1981) presented evidence of a "hostile media phenomenon" in the 1980 election; they found that most Americans evaluated media treatment of presidential candidates as fair and impartial, but when voters perceived bias in media coverage, they typically saw the source as opposing their preferred candidate (also see Beck 1991).

In addition, several different research approaches suggest that political sophistication or attentiveness may influence the accuracy of perceptions. For example, it is claimed (Neuman 1986, Zaller 1992) that political attention and political sophistication may increase the public's accuracy in perceiving political cues and thus enhance media effects. Research on political learning from the media seems to reinforce this point (Neuman et al. 1992, chapter 6). This literature suggests a potential interaction between political sophistication and the awareness of political cues. We will explore these possible interactions as part of the perceptual process.

The Third Stage

The last stage of the process involves the effect of media-based information on political choice. The literature stresses that individual characteristics interact with the message to determine media effects, but the nature of this interaction is disputed. For instance, there is a general belief that partisan predispositions condition media effects, but the nature of these influences is debatable (Schmitt-Beck 1996, 276). If we draw a parallel to the party contact literature, for example, then a hostile media may either mobilize or demobilize partisans. Do Democrats reading a paper that prefers Bush shift toward the Republican candidate, or does this news about the opposition motivate them to support Clinton even more? Either option is theoretically possible, but only empirical evidence can determine which is a more accurate description of reality.

Research also suggests that political sophistication

[3] There are political cues and influences of the press beyond the direct partisan cues we examine here (see, e.g., Bennett 1995, Edelman 1988, Patterson 1993).

may affect the nature of media influences, although the pattern of influence is debated. Scholars such as Zaller (1992) claim that while the politically sophisticated may be more attentive than others to political cues, their greater awareness may create a stock of political information that lessens the media's persuasive effects. Consequently, there are reasons to expect stronger media effects among the less sophisticated—if media cues are perceived in the first place. Conversely, uses and gratification theory suggests that highly interested individuals have a greater need for orientation, which facilitates their attentiveness and media influence (Weaver 1980). Both expectations are plausible, which led Philip Converse (1966) to suggest a curvilinear model of persuasive effects.

Summary

The media are a major source of political information for Americans, but the nature and influence of this information flow are unclear. The sheer number of newspapers and their history of partisanship create great potential for diversity in the message being sent to voters in different locales, but we do not yet know the clarity of the media's partisan content during a presidential election. Moreover, there are fundamental unanswered questions about how the information presented by the press is perceived by the public. How clearly do people perceive partisan cues that may emanate from the press, and what factors shape these perceptions? Finally, does the information gained through the media influence partisan choice? Research provides insights into selective parts of this process, but the whole process has not been examined in a nationally representative project. This may explain the contrasting claims that the media has strong or minimal influence.

Our goal is to examine the workings of the information process to help answer these questions. Yet, ours is also a limited goal. Newspapers are only one part of the media equation in modern campaigns. Most individuals, in fact, claim that television is their primary information source. If the media have a persuasive influence, however, then this should be most apparent for the press because of its partisan history and potential for variation, so this is the focus of our research.

DATA RESOURCES

Our study provides an unusual opportunity to look at the complete information flow from the press to the voters. The core of our analysis is the first nationally representative sample of newspaper coverage of a presidential election. We began by randomly selecting 40 counties to represent the American population (see the Appendix). For each we then coded the major daily newspaper read by county residents for a sample of days during the 1992 presidential campaign. This produced a sample of newspapers representative of those read by a cross-section of the American public (the list is included in the Appendix). Our research team analyzed the campaign coverage of these 46 newspapers between Labor Day and election day. We coded 6,537 items, including news coverage of the campaign, news analyses, opinion and editorial articles, political cartoons, and letters to the editor. We coded each for its location in the paper, presentation, and topics covered. More directly relevant to this research, we also coded each item for its overall evaluative content regarding the Bush, Clinton, and Perot campaigns.

This content analysis was done in conjunction with interviews of a nationally representative sample of people who lived in the counties served by these newspapers (see the Appendix). A total of 1,318 telephone interviews were completed after the election. Among other questions, we asked respondents what newspaper they read, how attentive they were to political news, and whether they felt their daily newspaper leaned more toward one presidential candidate than another. Finally, we collaborated with a study of television coverage of the 1992 election, and we include some preliminary data from this project.[4]

The analytic strength of our research is based on the potential content variation across the newspapers in the sample. We expected to find significant variation, since the range is from the recognized elite press (*New York Times*, *Washington Post*, and *Los Angeles Times*) to small town dailies (such as the *Paris News* in Paris, Texas) and includes newspapers with diverse political leanings. Given this range, we can determine whether readers can accurately perceive the political cues provided through the press and the factors that affect these perceptions. We also can examine whether election reporting influences voter choice and the factors affecting this calculus.

PRESS COVERAGE OF THE 1992 PRESIDENTIAL CAMPAIGN

One of the most debated aspects of the 1992 election was the role of the media in the campaign. At the Republican convention the criticisms of the media were almost as loud as the cheers for the party's nominee. Bush held up bumper stickers proclaiming "Annoy the Media: Re-elect George Bush," and there was a lively trade in T-shirts that read "Blame Me: I'm with the Media." Clinton's advisors similarly claimed that the media followed a double standard, focusing on the challenger while exempting Bush from the same kind of scrutiny. When the election was over, Perot's former press aide blamed the media for creating an unfairly negative image of his candidate. Indeed, the subtitle of the 1992 campaign might be "blame it on the media."

The 1992 campaign reflects the continuing debate on the role of the media in modern democracies. What cues does the press provide voters on the strengths and weaknesses of the contestants in an election? How much do they favor one candidate over another? What influence does this information have on electoral choices? The answers have basic implications for the

[4] For additional information on the television analyses, see Semetko 1996.

TABLE 1. The Evaluative Content of Election Coverage

	News Reports			Editorial Reports			Television Reports		
	Bush	Clinton	Perot	Bush	Clinton	Perot	Bush	Clinton	Perot
Extremely positive	0.1%	1.0%	0.1%	0.7%	0.7%	0.8%	0.0%	3.7%	0.7%
—	3.1	17.0	5.8	5.7	9.5	4.2	2.2	15.9	2.5
—	7.7	25.4	13.3	5.9	17.9	10.5	7.1	14.5	7.3
Balanced	31.0	32.5	38.8	21.0	29.1	24.9	24.5	27.8	17.8
—	32.3	11.8	20.5	33.0	23.3	27.0	28.9	10.7	13.1
—	17.7	4.5	9.3	26.2	13.1	24.9	15.0	4.0	10.9
Extremely negative	1.2	.4	.7	3.9	1.3	3.4	3.3	0.0	4.0
No evaluative content	6.8	7.5	11.6	3.6	5.1	4.4	19.0	23.4	43.6
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%
Mean score	3.39	4.44	3.82	3.19	3.84	3.13	3.29	4.51	3.43
(*N* of articles)	(3,039)	(2,823)	(1,523)	(1,094)	(989)	(503)	(453)	(428)	(275)

Note: Table entries are coders' assessment of the evaluative content of articles/stories from the perspective of each campaign. News reports include only news articles and news analyses in which the campaign was a visible part of the story; editorial reports include editorials by the newspaper and op/ed columns presented in the editorial section in which the campaign was discussed; television reports include all stories in which the campaign was visible. Mean scores are based on the 1–7 scale. The dashed lines indicate intermediate values on the 7-point scale, which were not labeled on the coding guide.

nature of electoral politics and the quality of information upon which voters must decide.

One of our central concerns is whether the press presented a strong partisan message in coverage of the 1992 campaign. We assessed the overall evaluative content of each newspaper article by judging whether it created an image that would be positively or negatively viewed by the respective campaigns.[5] Coders scored each article on a seven-point scale (1 = extremely negative, 7 = extremely positive).[6] For example, a generally positive article on Bush's well-received economic policy speech in Detroit would receive a positive score; but if an article contrasted the speech with Bush's record on the economy and his prior vacillation on economic policy, the story might receive a negative score.

Clearly, there are many elements to election reporting, as discussed in other media analyses (e.g., Bennett 1995, Patterson 1993), and the media's influence may occur in subtle ways. Nevertheless, the overall evaluative content of articles taps a central political cue that the media provide on the election: who is presented positively and who is presented negatively. Like the partisan cues of friends or social reference groups, this aspect of media coverage provides voters with guidance on their electoral choice from a known and generally credible information source. Moreover, it is exactly this aspect of media content that underlies much of the research on media effects on voting preferences (e.g., Erikson 1976, Noelle-Neumann 1984) and occupies the harshest critics of the media.

We first focused on articles that reported news about the campaign.[7] We examined news reports or news analyses in which the campaign was a significant part of the story; this excluded editorials, op/ed pieces, letters to the editor, and other nonnews campaign reports. Table 1 displays the evaluative content of 1992 election coverage. Roughly one-third of all news articles presented a balanced view; that is, the evaluative content favoring one candidate was matched by negative news (or equivalent coverage of a rival campaign) in the same story. Relatively few (roughly one-tenth) news articles contained no evaluative content; these provided straight factual information on the campaign. Articles on the Perot campaign most often lacked clear political cues for the readers.

Most news articles contained at least some evaluative content, such as whether one campaign was doing well or another was faring poorly. In 1992 the imbalance in this reporting was clear: Newspaper coverage of the Bush campaign was predominately negative. Whereas 51% of all news articles conveyed a negative image, only 11% reflected positively on Bush's campaign. By comparison, 17% of news articles on the Clinton campaign were negative, while 43% had positive evaluative content. References to Perot were lacking from most articles until October, when he reen-

[5] We created three separate variables in coding the overall evaluative content of each article in terms of the Bush, Clinton, and Perot campaigns. The instructions for the Bush variable were as follows: "Code the overall content of the article *that involves Bush, Quayle, or the Bush/Quayle campaign* in terms of its favorability or unfavorability to the Bush campaign. Evaluate the article from the perspective of the Bush campaign and assess the content of the article from this perspective. In other words, would the Bush campaign like seeing this article in print? If an article only focuses on Clinton (or other actors), then do not evaluate the Bush/Republican bias. If both candidates/campaigns are mentioned in the same article, then treat these as separate coding judgments; that is, an article might be negative to both candidates, positive to both, or a mix." (This was followed by examples of coding.)

The evaluations were coded on a seven-point scale: 1 = extremely negative, 7 = extremely positive. If the article contained no information that would lead to an evaluation of the candidates or the parties, then it was coded as "no evaluative content" (8).

[6] After the first weeks of coding, we conducted intercoder reliability checks. A set of articles were double-coded to assess the reliability of our media measures. The evaluative content measures displayed high levels of reliability for each campaign; unfortunately, the data used to calculate the specific reliability coefficients were lost in a computer malfunction.

[7] Each article was coded for its general type. News articles were reports on specific events that had occurred during the previous 48 hours. News analyses went beyond reporting the specific events; often these were reviews of the candidate's position or of campaign strategies.

tered the campaign. Among the stories on Perot with an evaluative content, the negative outnumbered the positive by three to two.

Journalists may strive for balance and objectivity in the reporting of partisan or election news, but the political cues of the press tend to be more apparent in editorial pages. The last vestige of an earlier tradition of many papers as party propagandists, editorial positions are an important part of a newspaper's political identity. Therefore, we also examined the evaluative content of editorial pages. The second panel of Table 1 presents the evaluative content of editorials and op/ed pieces that explicitly dealt with the 1992 presidential election. These data accentuate the patterns found in news reports. Both Bush and Clinton received more critical editorial treatment, but their relative ranking was similar to that for news stories. The one significant difference is that Perot was treated much more critically in the editorial statements than he was in news reports.

The third panel of Table 1 presents comparable data from television reports on the campaign, which contained less evaluative content than press reports. Roughly half of all television news reports gave balanced views of the candidates or lacked evaluative content, but the general treatment of the candidates was similar to that in the press.[8] Both media presented more favorable reports on the Clinton campaign and more critical reporting of the Bush campaign. Perot received intermediate coverage by both media, and both devoted much less attention to his candidacy. Marion Just and colleagues (1996, chapter 5) used a different method but found a similar overall ranking of the 1992 presidential candidates in their coding of tone in network news and a small set of local newspapers.

Why did the Bush campaign fare so poorly in the media (also see Just et al. 1996, chapter 5)? One explanation lies in campaign style. In a separate coding of actions on the campaign trail, we found that Bush pursued a predominately negative strategy: attacking his opponents, criticizing their politics, and belittling the opposition (e.g., his "Ozone Man" comments about Albert Gore).[9] When Bush was an actor in a story, he pursued such negative actions 33% of the time. In contrast, Clinton was more upbeat and optimistic, advocating new policies and stressing the positive. We found that candidate style was strongly correlated with the overall evaluative content of articles, even though these are conceptually separate aspects of media coverage and were independently coded. A negative approach often yielded a story with negative evaluative content, perhaps because controversy stirred up reaction, especially if journalists tried to balance negative comments with a rejoinder from the other campaign.[10] In short, Clinton ran a presidential-style campaign that benefitted his candidacy; Bush did not.

The Bush campaign further suffered because of its content. The economy was prominent among voters' concerns and campaign coverage (Dalton et al. 1995). Stories about the economy or federal budget showed the Clinton candidacy in a relatively positive light, while the Bush campaign emerged from such stories with a negative image of trying to defend its economic policies or attack its critics—this area showed the largest lead for Clinton in the relative favorability of election coverage. Another important news topic was the campaign itself, and the Bush candidacy also suffered in this regard. Struggling to define an identity for the campaign, reversing strategy, and openly trying to unify the party's diverging factions, the Bush effort hardly could avoid presenting a negative view of itself in 1992. In contrast, the Democrats were, for a change, unified and well organized. Even in the areas that might be considered strengths of the Bush campaign, such as foreign policy and candidate characteristics, Clinton's coverage was more positive than Bush's. In short, the Bush campaign was never able to define an agenda that advantaged its candidate, as it had in 1988. As other media analysts have observed, turning the Bush campaign into a positive news story in 1992 would have required a large positive bias in reporting actual events (Pomper et al. 1993, Weisberg 1994).

We also considered whether the evaluative content of election reporting reflected overt partisan bias by the press. This is an admittedly difficult judgment to make. Negative or positive news is not necessarily an indicator of media bias—it may reflect an accurate view of the relative performance of each campaign. To test for possible media bias, we separately coded the tone of coverage to see if journalists added their own evaluations to their reporting of the news.[11] They seldom did so. Almost two-thirds of all news articles were purely descriptive (61.2%); in another one-sixth (16.3%) the journalist had mixed observations about the news being reported. Barely one-sixth (17.3%) included evaluative comments from the reporter, and these were almost evenly balanced between comments

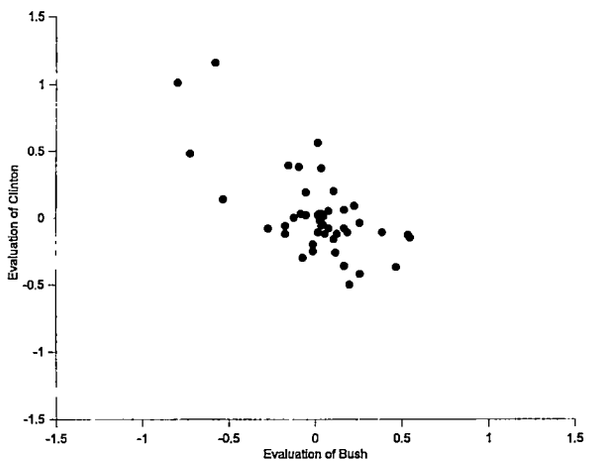
[8] The newspaper and television content data were separately coded by research groups working at the University of California, Irvine, and the University of Michigan. Both groups used the same coding protocol and instructions.

[9] The coding instructions were as follows: "This is a judgment of the actions(s) taken by the main actor. First, judge whether the actor is *taking a critical or an affirmative action*; then code the intensity of this action. Not all stories have actors who are taking actions with an evaluative dimension; thus, there are separate codes for balanced and neutral stories. To illustrate: if the essence of the article is 'Bush strongly supports a capital gains tax,' then this is an affirmative action. If the essence of the article is 'Clinton criticizes North American Free Trade Agreement,' this is a critical statement." These evaluations were coded on a seven-point scale (1 = extremely critical, 7 = extremely affirmative).

[10] Although this evidence is indirect, it tends to undercut the arguments on the effectiveness of negative campaigning (Ansolabehere and Iyengar 1995); for additional empirical evidence challenging the negative campaigning thesis, see Brians and Wattenberg 1997.

[11] This variable was derived from Semetko et al. 1991. It determines whether the journalist added identifiable material that was intended to reinforce or undermine the message of the primary actors in the article. The coding instructions were as follows: "Were there identifiable comments by the reporter that reinforced (or reflected positively on) the candidate's actions or deflated the message of the candidate (negative comments or diminishing comments), or is the reporter making simple descriptive comments or straight comments with no evaluation?"

FIGURE 1. The Evaluative Content of Press News Articles about the 1992 Election



Note: Figure entries are mean scores on the 7-point evaluative measures, expressed as deviations from grand means (Bush mean = 3.41; Clinton mean = 4.45). N = 46.

that either reinforced or undermined the candidates' statements. Thus, the overall evaluative content of campaign news did not reflect the explicit comments of journalists.

In the end, however, we are primarily concerned with whether the press presented partisan cues in 1992, rather than the source of these cues. Although the press speaks in mixed messages and varied tones, there were significant partisan cues in newspaper coverage of the 1992 presidential campaign.

Variations in Press Reports

The overall content of election coverage tells us which candidates were generally favored in press reports, but these aggregate statistics do not describe the specific information available to readers of individual newspapers. Indeed, the number and diversity of newspapers in the United States offers the potential for great variation in the evaluative content of press reporting. There is anecdotal evidence that newspapers (and journalists) can be very partisan in their coloration of the news. Other evidence suggests that newspapers have gradually and steadily lessened any explicit partisan leanings. Our sample of newspapers enables us to examine the degree of diversity in the partisan cues actually disseminated in the American media.

Figure 1 presents the averages for the evaluative content of news articles about the Bush and Clinton campaigns for each of the newspapers in our sample. To provide a common metric across separate figures, these data are expressed as deviations from the overall mean for each campaign. (In absolute terms, every newspaper in our study was more positive in its relative coverage of the Clinton campaign!). The range of evaluative content regarding both campaigns was modest. Most newspapers hovered fairly close to the overall mean (the standard deviation is approximately .30). For example, the *Washington Post*, which scored .17 above the overall mean for Bush and −.08 for Clinton, was more typical in its campaign coverage than such local papers as the *News Sentinel* of Fort Wayne, Indiana (.55, −.16), or the *Post-Standard* of Syracuse, New York (−.74, .48).[12]

There are several ways that we can measure the clarity of partisan stimuli emitted by specific newspapers. In aggregate terms, there is a strong negative correlation in the treatment of the Bush and Clinton campaigns (r = −.68); papers that favored Bush tended to be critical in their reporting on Clinton.[13] Equally important, the information presented by any single newspaper provides multiple perspectives on each campaign. This can be seen by using the newspaper as an independent variable to predict the evaluative content of news reports. The correlation between the newspaper and the evaluative content of news articles was modest for the Bush campaign (η = .25), the Clinton campaign (.25), and the Perot campaign (.21). This means that although the differences *between* newspapers in their overall reporting of each campaign were substantial, the differences in reporting *within* newspapers was also considerable. In short, although U.S. newspapers differed somewhat in their reporting of the campaign, they also contained multiple messages on each campaign for their readers.

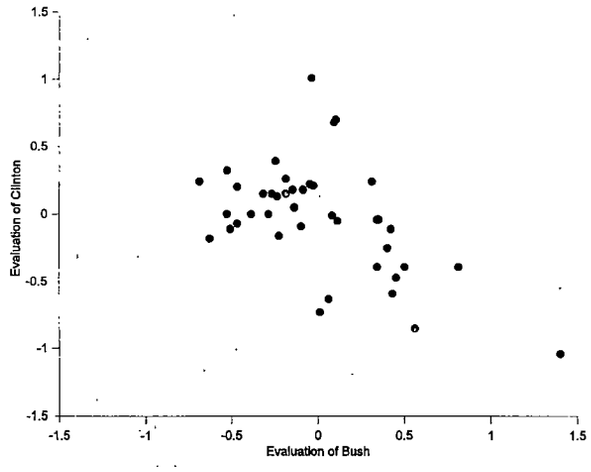
We replicated these analyses for the editorial coverage of the 1992 campaigns, that is, both editorial statements and op/ed pieces. When compared to Figure 1, Figure 2 shows that newspapers differed more in the partisan content of editorial material than in their news reports. Entirely as expected, editorials contained more evaluative content and varied more distinctly across newspapers. For instance, the standard deviation in the evaluative content of editorial material was nearly twice that of news articles. In addition, the correlation between the newspaper and the evaluative content of its editorials is somewhat stronger than for news reports: Bush campaign (η = .36), Clinton campaign (.34), and Perot campaign (.35). Yet, the correlation between coverage of the Bush and Clinton campaigns in Figure 2 is slightly weaker (−.59) than for news coverage (−.68) in Figure 1.

We contend that individual newspapers often present multiple, conflicting messages regarding the candidates. The clearest evidence comes from comparing the news and editorial coverage of the three campaigns. As an illustration, Figure 3 plots evaluative content in news coverage and editorials on the Bush candidacy. There is a very weak (r = .15) correlation between these two elements of campaign reporting. We obtained similar results for the evaluative content of reports on the Clinton (.35) and Perot (.13) cam-

[12] Since so much research is devoted to the elite press, we should note that newspapers in that group tended to be fairly balanced in their reporting of both campaigns: *New York Times* (.02, −.07), and *Los Angeles Times* (−.29, −.09). Greater variation in reporting was displayed among the local dailies.

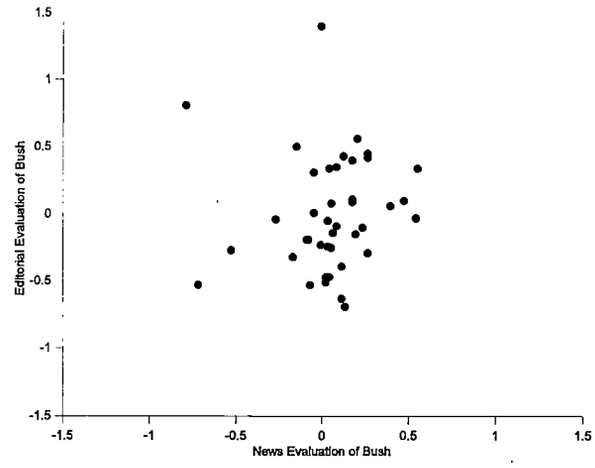
[13] The evaluative content of news coverage of the Perot campaign was positively correlated with evaluative content coverage of both the Bush (r = .26) and Clinton (r = .13) campaigns.

FIGURE 2. The Evaluative Content of Editorial Press Coverage of the 1992 Election



Note: Figure entries are mean scores on the 7-point evaluative measures, expressed as deviations from the grand means (Bush mean = 3.20; Clinton mean = 3.79). $N = 41$, not 46, since newspapers with less than 30 coded editorial articles were excluded.

FIGURE 3. Bush Coverage in Press News Reports and Editorial Coverage of the 1992 Election



Note: Figure entries are mean scores on the 7-point evaluative measures of news coverage and editorial coverage of the Bush campaign, expressed as deviations from grand means. $N = 41$.

paigns. To some extent, the weak association is surprising. Many assume that a newspaper's editorial stance strongly colors its reporting of the news, whereas our data suggest a significant degree of autonomy. This also can be seen if we compare the editorial endorsement of newspapers to their coverage of the campaign. Although fewer and fewer papers formally endorse a presidential candidate, an endorsement often is assumed to be a surrogate of the paper's political position that will be reflected in its news coverage. Our study shows not only no relationship between newspapers' formal endorsements and their news coverage of the campaign but also a very weak relationships between the endorsements and the overall content of their editorial pages.[14] For instance, the newspapers that endorsed Bush were slightly more negative in their news coverage of his campaign (55% negative) than papers that endorsed Clinton (50% negative toward Bush). Similarly, papers that endorsed Clinton were slightly less positive in news articles about their preferred candidate (42% positive) compared to papers that endorsed Bush (47%). If anything, these findings suggest that newsrooms display views independent from those of their editors.

Taken together, these results provide strong evidence of the general separation of campaign reporting and editorial opinions in America's print media. Contrary to common claims in the literature (e.g., Bagdikian 1992), newspaper ownership and editorial control apparently color the reporting of presidential campaigns less than they may have when newspapers were more openly aligned with a party or political orientation. Even if some editors and some newspapers still project a clear partisan image of election campaigns through their news reporting, this is relatively rare.

From the perspective of information flows, our findings imply that the typical American voter reads a newspaper that differs modestly from other newspapers in its overall evaluative reports on the presidential election. Compared, for example, to the classic case of the British partisan press, American newspapers are fairly neutral in their reporting on the campaign. Equally important, most newspaper readers are exposed to multiple evaluative messages about the campaign from the same newspaper, not a single and clear partisan perspective. The evaluative content of a paper's news reports was relatively independent of the information provided through editorial pages for the 1992 campaign. Indeed, several newspapers offered a positive image of a candidate on the editorial page and a contrasting view in news reports. The diversity of the message also existed within the news and editorial sections. Some news articles presented a positive image of the Bush (or Clinton) campaign, and the next section of the same article or the next news article presented a contrasting view. Indeed, contemporary American journalistic norms stress balance and objectivity in political reporting, and a common way to present the information is by intermixing positive and negative accounts. Thus, the partisan information flows derived from the press conform to Zaller's (1992) notion of multiple messages, giving voters a wealth of material and a diversity of evaluative cues from which to make their judgments.

READERS' PERCEPTIONS

The next stage in the process of political intermediation involves the public perceptions of the messages

[14] The correlations between editorial endorsement and evaluative content of news articles is insignificant for coverage of both the Bush ($r = .01$) and Clinton ($r = -.02$) campaign. Even the correlations between newspapers' endorsement of Bush and editorial content toward the Bush ($r = .08$) and Clinton ($r = -.04$) campaign are weak, albeit in the expected direction.

TABLE 2. Perceived Favored Candidate by Information Sources

Favored Candidate	Daily Newspaper	Nightly TV News	Personal Discussant
Bush	12%	4%	34%
Clinton	40	33	41
Perot	1	1	15
Mix of candidates	1	1	1
Did not favor a candidate	42	55	2
Not sure	4	4	7
Total	100%	98%	100%
(*N*)	(1,101)	(1,112)	(1,068)

Note: Table entries are the public's perception of the political leanings of various information sources from the national survey of 1,318 respondents.

presented by the press. How do ordinary voters, without a staff of coders and coding rules, see the political cues in their daily newspaper? We know from prior studies that people often perceive their newspaper as leaning toward one candidate or another, but research has seldom matched these perceptions against reality. One of the most significant features of this study is our ability to compare press campaign coverage to readers' perceptions of these same newspapers.

Following the election we conducted a telephone survey of residents in the 40 counties of our study. The respondents were asked what presidential candidate was favored by their daily newspaper, as well as by the national TV network newscast they watched most regularly and the persons with whom they regularly discussed events.[15] As shown in Table 2, the most common response described newspaper and television coverage of the campaign as not favoring any candidate. When there was a perceived bias, more people saw their local newspaper, as well as their preferred network TV news program, as leaning toward Clinton in 1992, with Bush a distant second and Perot receiving a mere trace of support.

There is also a significant contrast between respondent perceptions of the media and the people with whom they discussed important matters. Respondents often saw the media as neutral in the campaign, but they viewed almost all personal discussants as having a favorite candidate. Furthermore, the political leanings of discussants closely mirrored the 1992 election outcome, with Clinton displaying a slight lead over Bush; Perot supporters roughly matched the candidate's actual vote total.

In aggregate terms, there is at least no inconsistency between public perceptions of the press and the results described in the previous section. We found that most newspapers did not take strong partisan stands, and a large proportion of the respondents did not perceive their daily newspaper (or television news program) as preferring one candidate. The evaluative cues that were present in the press generally favored the Clinton campaign; public perceptions mirrored these leanings.

The real test of the accuracy of the public's perceptions, however, comes from comparing readers' perceptions to the content of their daily newspaper. The recognition of political cues is a complex process. In part, it depends on the clarity and visibility of the message. For instance, it is relatively easy for Britons to perceive accurately the political leanings of their highly partisan press (Curtice and Semetko 1994). American newspapers are normally temperate in campaign reporting, but there was substantial variation in the evaluative content of the newspapers we studied. Still, Zaller's (1992) distinction between single and multiple message models suggests the following hypothesis: Newspapers that present a clearer partisan message are more likely to be perceived accurately than those that give diverse images of the campaign (Noelle-Neumann 1973, Zaller 1992).

In addition, the recognition of political cues may be conditioned by the characteristics of the individual. Summing up a large research literature, Zaller (1992) formulated a "reception axiom," which suggests that levels of cognitive engagement are positively correlated with exposure to and comprehension of political stimuli (also see Neuman 1986). In other words, politically sophisticated individuals are more likely to perceive the partisan cues presented by various information sources, including their daily newspaper and daily television news program. Zaller presented impressive inferential tests of the reception axiom but never directly compared voter perceptions and the content of their information sources. As noted earlier, other scholars have questioned this axiom (e.g., Weaver 1980).

Research suggests contrasting expectations as to how individual predispositions affect recognition of (and receptivity to) political cues. Social learning theory assumes that people project their own preferences onto external information sources. For instance, unless the message is clearly dissonant, individuals tend to perceive others as holding political views similar to their own (Niemi 1974). Similarly, Wolfgang Donsbach (1990) has used cognitive dissonance theory to argue that media perceptions may reflect a person's preferences.

In contrast, Robert Vallone and his colleagues (1981, 1985) posit that a hostile media effect evokes just the opposite reaction. They found that people with strong political beliefs perceive the media as opposing their views, especially a media that attempts to present a balanced picture of the matter. Paul Beck (1991) replicated their analyses for a subset of Ohio newspapers in 1988. He found that partisans, if they saw any bias, were likely to see their daily newspaper as leaning toward the opposing candidate. This held even when Democrats and Republicans were evaluating the same newspaper.

To examine the linkage among newspaper content,

[15] The question were: "Which presidential candidate do you think the [respondent's daily paper] favored during the campaign, or didn't it favor any candidate?" "Which presidential candidate do you think [respondent's daily network TV program] favored during the election campaign, or didn't it favor any candidate?" "Which candidate do you think [respondent's first named discussion partner] supported in the presidential election this year?"

voter perceptions, and the above research hypotheses, we merged the aggregate newspaper data with the public opinion survey. Among those respondents who regularly read a newspaper, approximately 83% listed one of the papers in our content analysis study as their primary newspaper. For each reader of a specific newspaper, we created two new variables that measured whether the news and editorial content of the paper leaned toward either Bush or Clinton.[16]

We began by determining whether readers could accurately assess the partisan content of their daily newspaper. There is a surprising lack of statistical relationship between the actual news content of a newspaper and reader perceptions of its political leanings. The simple correlation between these two measures based on regular newspaper readers was only .09.[17] Reader perceptions of a paper's political leanings were only slightly more related to a newspaper's editorial content (r = .11).[18] Even endorsements of a presidential candidate were only modestly correlated with public perceptions of the partisan leanings of these papers (r = .13).[19] People judged a newspaper's bias a bit more from what was on the editorial pages than from its news coverage (even though we found that the evaluative content of editorials and news articles is relatively independent). The weakness of these relationships, however, indicates considerable slippage between the objective content of a newspaper and the message perceived by its readers. There were significant differences in evaluative content among the newspapers in our sample (see figures 1 and 2), but such differences were not clearly perceived by readers.

As noted above, one factor that may affect the accuracy of perceptions is the clarity of the political signal sent by the press. We tested this idea for both news reports and editorials. For each newspapers we computed the standard deviation of evaluation scores in news reports and editorials on the campaign.[20] Table 3 indicates that newspapers with a more consistent partisan message (lower standard deviations in their evaluation scores) are more accurately perceived by their readers. For instance, there is a .22 correlation between editorial material and reader perceptions for newspapers with lower variance, and a .02 correlation for the newspapers with the most diverse messages. Thus, the clarity of the partisan signal affects the accuracy of its reception.

TABLE 3. Perceived Candidate Leaning of Newspapers by the Variance in Content

		Variance in Evaluative Content		
	Sample	Low	Modest	High
Editorials	.11*	.22*	.13*	.02
News reports	.09*	.14*	.01	−.01

Note: Table entries are Pearson correlation coefficients between the evaluative content of editorials and news coverage and reader perceptions of the candidate leaning of their daily newspaper. The variance in evaluative content was measured by the standard deviation in editorial or news content for each paper.
*$p \leq .05$.

Personal characteristics also may affect perceptions of media content. Therefore, we expanded our model to examine the effect of partisanship and political attention on these relationships. The first column in Table 4 models readers' perceptions of the partisan leanings of their daily newspaper using three predictors: the content of editorials, the content of news reports, and the respondent's party identification. The direct effects of media content remained essentially unchanged. In addition, we found clear evidence of the hostile media effect as a function of party identification.[21] Overall, these effects were the strongest in the model (β = −.14). People with strong attachments to the Republican Party were more likely to see their paper as leaning toward Clinton, independent of the paper's actual coverage. Similarly, people with strong Democratic identifications were likely to see their

[16] We calculated the difference in the evaluative content of articles on the Bush and Clinton campaigns, derived from the data in figures 1 and 2. This was done for both news articles and editorials. This yields two variables that measure the relative evaluative content toward both campaigns, similar to the survey question which asks respondents to make these judgments. We did not include Perot because a statistically insufficient number of people felt their newspaper leaned toward Perot.

[17] We were concerned that the simple summation of news articles across the full campaign might not produce an accurate measure of each newspaper's partisan content, which would lead to weak correlations. Therefore, we created a new measure that focuses on news articles with the following characteristics: (1) published in October and November, (2) appeared in the first news section of the paper or in a special election news section, and (3) weighted by size; that is, large articles (in column inches, without any continuation pages) received more weight than short articles. This recalculated measure was strongly related to the original measure presented in the text (r = .87), and the correlation with reader perceptions was identical to the statistic presented in the text (.09).

[18] The larger correlation for editorial content is partially a function of the greater variance of this measure. In contrast, the unstandardized regression coefficients indicate that news reporting (b = .12) had a stronger effect on perceived evaluative content than did editorial content (b = .08). See Table 4 below.

[19] This weak correlation may be surprising, because we all know of specific papers whose editorial leanings seem obvious to all (or at least to all media analysts). We attribute this weak relationship to several factors. More than two-thirds of the newspapers in our sample did not formally endorse either presidential candidate, which attenuates any possible relationship. In addition, we believe the multiple messages that newspapers project blur the clarity of their partisan cues.

[20] We would like to thank an anonymous reviewer for suggesting these analyses. We computed the difference between evaluative scores for the Bush and Clinton campaign in news reports that covered both, and we then computed the standard deviation for these difference scores in each newspaper. These standard deviations were used to categorize newspapers in Table 3. For editorials, we selected newspapers that had a standard deviation less than 1.25 or greater than 2.50 as the low/high categories; for news articles we selected newspapers with a standard deviation less than 1.25 and greater than 2.00.

[21] For comparability with the other countries in the Cross-national Election Project, we used a version of the party identification question derived from the German election studies. Respondents were asked: "Many people lean toward a particular political party for a long time, although they may occasionally vote for a different party. Do you generally lean toward a particular party? (If independent): If you had to choose, do you think of yourself as closer to the Republican or the Democratic party? (If a partisan): Taken altogether, how strongly or weakly do you lean toward the [respondent's] party?"

TABLE 4. Predicting the Perceived Evaluative Content of Newspapers

	Sample		Attention to the Campaign: A Lot		Attention to the Campaign: Some		Attention to the Campaign: Little/None	
	β	b	β	b	β	b	β	b
Editorials	.12*	.08*	.08	.06	.18*	.12*	.03	.02
		(.02)		(.05)		(.03)		(.05)
News reports	.09*	.12*	.07	.09	.13*	.17*	.02	.03
		(.05)		(.09)		(.07)		(.11)
Party identification	−.14*	−.04*	−.06	−.02	−.16*	−.04*	−.26*	.07*
		(.01)		(.02)		(.01)		(.02)
Constant	—	2.09	—	2.18	—	2.11	—	1.93
Multiple *R*	.20		.11		.27		.30	
(Minimum *N*)	(792)		(243)		(364)		(184)	

Note: Table entries are standardized regression coefficients (β) in the first column and unstandardized coefficients (*b*) in the second column (with standard errors in parentheses) predicting readers' perceptions of the partisan leaning of their daily newspaper. Pairwise deletion of missing data was used in all regressions.
*$p \leq .05$.

newspaper as leaning toward Bush. This holds even when individuals were judging the same newspaper. Since partisans see the generally moderate political stance of the media as leaning away from their own more partisan political views, that may explain why the media are repeatedly and simultaneously attacked by critics on the Left and Right for their political bias.

We extended our analyses to test Zaller's reception axiom. One question in the survey asked newspaper readers how much attention they generally pay to newspaper articles about the presidential campaign. The last three columns in Table 4 analyze perceptions of the press while controlling for attention level.[22] The most striking pattern is the strengthening of the hostile media effect for partisanship at lower levels of political attentiveness. Less attentive voters are much more likely to project their partisan biases onto the press ($\beta = -.26$); for them, the media is an advocate for the opposition, and objective media content was not significantly related to their perceptions. In a limited sense, Zaller's reception axiom is supported. The least accurate perceptions were found among those who did not pay attention to newspaper coverage. Yet, these relationships were only slightly stronger among the most attentive (they were strongest among the middle group).

These results seem to challenge the claim that the press exercises a persuasive influence on the American public. It would be difficult for a newspaper to influence the specific policy opinions of its readers if even the messages on a highly visible presidential election are not clearly perceived. A persuasive message must be sent and received, and our analyses of partisan cues in the 1992 campaign provide ambiguous evidence on both points. Furthermore, the evidence in support of the hostile media hypothesis suggests why the press may receive such widespread criticism. Whether on the Left or Right, partisans see the balanced and objective press as leaning toward their opponent. Moreover, these feelings are strongest among those least attuned to the campaign, who perceive the media as hostile toward their preferred candidate.

MEDIA CONTENT AND CANDIDATE PREFERENCES

The information flow that begins with the presidential campaign and the media's coverage ultimately ends in individual decisions about a candidate. As we have seen, however, the connection between the beginning and end of this information flow is complex and often imprecise. First, newspapers project multiple messages about the campaign, so the political cues lack strength and clarity. Second, perceptions of the messages are as much a function of an individual's partisan leanings as of the message itself. Third, we come to the question of how the objective message and subjective perceptions influence partisan preferences.

To assess possible media effects in 1992, we linked our measures of press content to voters' feelings about the two major candidates.[23] We began with our two

[22] The literature is divided on whether to measure media attentiveness through exposure (such as the frequency of readership) or attentiveness to specific topics (Biocca 1988, Chaffee and Schleuder 1986). We chose the latter because it enables us to focus on those who claim to use the media as an information source. The question wording was: "During the election campaign, how much attention did you generally pay to newspaper articles about the presidential campaign? Did you pay: a lot, some, a little, or no attention?"

[23] We measured candidate image with the following question: "I'd like to get your feelings toward the major candidates for president by asking you to rate each one on a scale that runs from 0 to 10. Zero means you feel most unfavorable toward the candidate, 10 means you feel most favorable toward the candidate, and 5 means you feel neutral toward the candidate. Using any number from 0 to 10, overall how unfavorable or favorable do you feel toward Bush?"

We used candidate image for several reasons. Vote choice is a simple dichotomy, which produces potential problems of estimating a dependent variable. In addition, the three-candidate race in 1992 complicated the analysis of vote preference and would force us to exclude Perot voters from the analysis. Therefore, we computed the difference between respondent evaluations of Bush and Clinton on the ten-point feeling scale. This difference measure (positive values represent support for Clinton) is the dependent variable for our analyses.

TABLE 5. Predicting Candidate Preferences in 1992

	Total Sample		Variation in Editorial Content						Attention to the Campaign					
			Low		Modest		High		A Lot		Some		Little/None	
	β	b	β	b	β	b	β	b	β	b	β	b	β	b
Editorials	.08*	.28*	.15	.55	.08	.25	.08	.29	.04	.13	.11*	.35*	.01	.03
		(.13)		(.47)		(.15)		(.37)		(.23)		(.18)		(.27)
News reports	.02	.20	−.12	−.85	.01	.10	.03	.15	−.03	−.17	.01	.06	.07	.50
		(.23)		(.75)		(.42)		(.42)		(.36)		(.37)		(.57)
Newspaper perceptions	.00	.00	−.05	−.22	−.02	−.09	.17	.83	−.06	−.24	.02	.08	.05	.25
		(.17)		(.39)		(.22)		(.46)		(.27)		(.27)		(.37)
Party identification	.38*	.48*	.33*	.41*	.37*	.47*	.47*	.59*	.46*	.56*	.42*	.55*	.28*	.34*
		(.04)		(.11)		(.06)		(.12)		(.08)		(.07)		(.10)
County vote	.03	.01	.05	.01	.04	.01	−.07	−.01	.10	.03	−.06	−.02	.10	.03
		(.01)		(.03)		(.02)		(.02)		(.02)		(.02)		(.03)
Intercept		2.93		2.72		2.98		3.10		3.73		4.27		1.43
Multiple *R*	.40		.39		.39		.50		.50		.42		.32	
(Minimum *N*)	(654)		(119)		(428)		(98)		(198)		(302)		(154)	

Note: Table entries are standardized regression (β) coefficients in the first column and unstandardized coefficients (*b*) in the second column (with standard errors in parentheses) predicting candidate preferences based on the difference between the ten-point scales evaluating Bush and Clinton.
*$p \leq .05$.

objective measures of the content of the respondent's main daily newspaper: the relative favorability of news reports and editorial material (editorials and op/ed columns) toward the Bush and Clinton campaigns. We also included the respondent's perception of the candidate leanings of their newspaper to see if perceptions mediated the effect of the paper's evaluative content.

In addition, we wanted to control for the possibility that the correlation between content and party preferences is a function of predispositions or other spurious factors. We have discussed how predispositions may influence both awareness of messages and their persuasive influence. One potential source of these effects is the party identification of the respondent. Partisans may selectively perceive the political world and may be less influenced by information that supports competing points of view. Another source of spuriousness may arise from the local political context. If newspapers try to reflect the local standings of the candidates or their readers' preferences in reporting the news, then a correlation between media content and candidate preferences would be spurious. To control for these effects, we included both partisanship and county voting patterns as control variables.[24]

As expected, party identification had a very strong influence on candidate image in 1992 (β = .38), shown in Table 5. In addition, the political cues provided in editorials had a significant positive effect (β = .08) on candidate image even while controlling for partisanship. It appears that voters used editorials as a reference point in making their electoral decisions. Some readers who saw the editorial content of their paper leaning toward Clinton apparently factored this into the candidate choice, just as they might weigh the partisan cues provided by other information intermediaries. Indeed, these editorial cues may have had special force in 1992; the Clinton candidacy did especially well if we assume that most newspapers normally favor Republicans and incumbents. For instance, when the strongly conservative Orange County *Register* openly asked whether Bush should have run for reelection, this sent a clear political signal of the candidate's weakness.

The regression coefficients for media effects admittedly appear quite modest, which could be viewed as justification for the "minimum effects" interpretation of the media. One should recognize, however, that even a modest effect can have significant electoral consequences when the range of editorial content is substantial.[25] Figure 4 displays the relationship between editorials and candidate preferences derived from the unstandardized, total sample equation in Table 5. The center line presents the relationship for the total sample; to place this in context, we also chart the relationship for Democratic and Republican identifiers. A small relationship can produce large differences. For instance, the Clinton/Bush difference in editorial content of the *Detroit News* was relatively favorable toward Bush (−1.97), and the editorial pages of the *Detroit Free Press* were more positive toward Clinton (.46). Using these two papers as an example, the differences in their editorial content could produce a .65 point shift in the mean candidate images (on a ± 9 scale) among their respective readers, which is roughly the effect of partisan differences between In-

[24] County vote is Clinton's percentage of the vote in the three-way race.

[25] In addition, research by Bartels (1993) on the effect of measurement error also applies to our analyses. The imperfect reliability of the media content measure should attenuate media effects. When coupled with measurement error in the survey-based variables, the "true" influence of the media would be substantially larger. Because data are unavailable to estimate these measurement effects empirically, their precise influence can only be surmised.

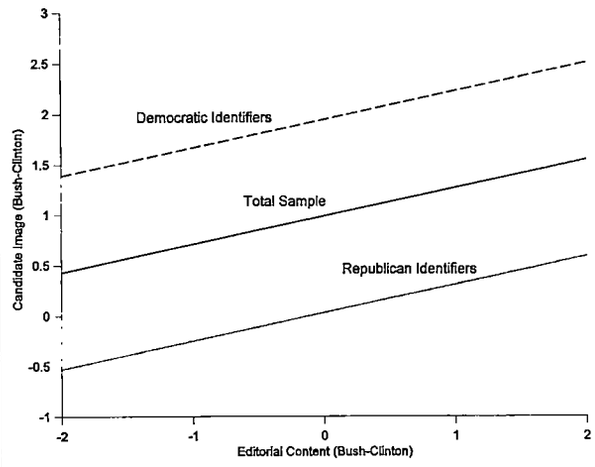
dependents and Republicans.[26] Most newspapers did not display this magnitude of difference in their political cues, but when distinct political cues exist, they can influence political images. Even compared to the large effects for partisanship, these media effects are more than minimal.

The variables that do not exert an influence in Table 5 are also interesting. The political cues in news reports about the campaign did not have a significant independent effect on candidate images. We attribute this to the weak and multiple messages that flow from news reports on the campaign. To the extent that the American media try to be balanced and objective in reporting election news, the effects *should* be weak. In addition, perceptions of a newspaper's partisan leanings have no influence on candidate images once partisanship and the objective content of the press are controlled. Again, this suggests that media perceptions are largely a projection of political values (in this case partisanship) and thus have little independent influence on political opinions.

Beyond these overall analyses, we wanted to test our hypotheses that characteristics of the message (the clarity of the signal) and characteristics of the individual (the attentiveness to the media) may affect the cue-giving influence of the media. We first tested the hypothesis that editorial content will have a stronger effect when cues are distinct as opposed to diverse.[27] The second panel of Table 5 repeats the basic model while controlling for the variation in a paper's editorial content. In each submodel the effects of partisanship were dominant; when media cues were most variable, partisanship and voter perceptions of media cues were at their strongest (column three). Editorial cues were strongest for readers of papers with more consistent partisan cues (b = .55) than for newspapers whose party cues were highly variable (b = .29). Due to small subsamples and small effects, the differences between these two coefficients were not statistically significant.

The influence of media cues also may be a function of an individual's attention to the media. The last panel in Table 5 controls for the level of media attentiveness. The effect of partisanship was systematically related to attentiveness; the most attentive display the strongest partisan effects. There was also a tendency for editorial effects to be stronger among the moderate (b = .35) and highly attentive (b = .13) and weakest among the least attentive (.03), but this pattern was neither monotonic nor strong. In addition, news report content, perceptions of the newspaper's leaning, and county voting all lacked a significant relationship with candidate images. Attentiveness has only a modest influence on media effects.

FIGURE 4. The Relationship between Editorial Content and Candidate Preference



Note: This figure plots the relationship between editorial content and predicted candidate support based on the total sample equation in Table 5.

THE MEDIA AND AMERICAN ELECTIONS

Our research provides a unique perspective not only on the media's role in national election campaigns but also on the persuasive effects the press may exercise by providing evaluative cues about the candidates to voters. We were not concerned with potential cognitive effects of the press, such as through priming or agenda setting, although these are important aspects to study. In 1992 the content of the evaluative information in the press was predominately critical of Bush and more flattering to Clinton, although we attribute this largely to the content and dynamics of the campaigns and less to any conscious media bias. Media coverage of the 1992 campaign primarily reflected the failures of the Bush campaign, which the media did not create.

Beyond the sum of political information provided by all media in 1992, we found that newspaper readers were exposed to a diversity of political messages from the press. The average newspaper presented a mixture of positive and negative news about each campaign. Moreover, we uncovered a surprising independence in political content between news reports and editorial assessments of the campaigns. Contemporary journalistic norms encourage such diversity of views and

[26] For the interested reader, we present below the comparable multiple regression analysis to Table 5, using dichotomous vote choice (excluding Perot voters and nonvoters) as the dependent variable.

	β	b	Standard error
Editorials	.10*	.05*	(.02)
News reports	.06	.06	(.02)
Party identification	.73*	.14*	(.01)
Perception	−.04	−.03	(.02)
County vote	.02	.00	(.00)
Constant	—	1.27	
Multiple R	.76		
(Minimum N)	(561)		

*$p \leq .05$.

The effect of editorial content is slightly stronger when predicting vote choice. If the difference in editorial content of the *Detroit Free Press* and *Detroit News* were linked to vote choice, then this would represent a 12% difference in predicted vote.

[27] We focused on the clarity of editorial rather than news reporting cues because editorial cues are empirically more important in structuring candidate images. The operationalization of the variation measure uses the same procedures as for Table 3.

moderation in overall political content; the days of a highly partisan American press are past. In summary, the press did not speak with a single clear voice in 1992, but with several voices and in muted tones.

The attempt of most newspapers to be fair in their coverage of the campaign ironically may explain why the media have received so much criticism from both partisan camps and why this is likely to continue. We found clear evidence that adherents of a political party tend to perceive their daily newspaper as leaning toward the other party—regardless of the paper's actual stance—even though most readers perceive their paper as relatively unbiased. This hostile media effect means that many partisans see balanced treatment of the candidates as bias toward the other side.

At the end of this process, the partisan cues provided by the press had a modest independent effect on candidate preferences. The content of editorials directly influenced candidate preferences; for example, newspapers that favored Bush on their editorial pages slightly increased support for Bush among their readers. Significantly, the effect was found for editorial content, which delivered partisan messages that were strongest and clearest, and not for news reports on the campaign. But the effect was modest, perhaps because even the editorial page sends multiple messages.

These results have several implications for the role of the modern press in democracy. The separation of news and editorial content and the diversity of information provided speak very positively about how the American press performs its role as a provider of information. The typical American voter has access to a large amount of material and a diverse set of political viewpoints through the local newspaper. Added to this is the information available from television, social groups, friends, and other intermediaries. Certainly, the wealth of information readily available to the average American voter now far exceeds the experience of previous generations.

The evidence that newspaper content can shift public images of the presidential candidate also suggests that the press performs a persuasive role as well as an information function. More than just framing events, the press provides political cues that may significantly influence the opinions of readers. Still, we stop short of interpreting these effects as completely attributable to the media. Even on the editorial pages, the press is analyzing campaign events that may vary in emphasis across locales because of interest in issues and partisan context differ. In other words, we find evidence of information effects, but it is not clear whether this is the media's independent influence or the effect of the campaign as transmitted by the press. The existence of these relationships, however, should revive interest in the potential persuasive power of the media.

The evidence in support of the hostile media hypothesis also may explain why it seems nearly everyone agrees that the media is biased—they just disagree on the direction of this bias. Partisans appear more sensitive to dissonant information in the media than they are to information that reflects their views. This makes the media unique in comparison to other information sources, such as personal discussants and social groups, which individuals see as reinforcing their own beliefs. It is likely that these effects have strengthened in recent decades, since most cities are now served by a single newspaper that gravitates to a neutral political position. The hostile media effect presents a special challenge for newspapers and television. It is possible for the media to be both more neutral in their reporting of events and increasingly criticized by partisans for their (perceived) bias. Thus, the media's continuing credibility depends on providing balanced coverage of the news, yet still retaining the confidence of a possibly skeptical readership.

Finally, the public's limited ability to perceive accurately the partisan cues of the press and the limited influence of these cues on candidate images may reflect the basic nature of the press's message. The information flow through the press conforms to what Zaller would describe as a "multiple message" medium. Multiple and often conflicting messages necessarily diminish the potential persuasiveness of this information flow, especially in comparison to the more partisan press in other democracies. The strongest evidence supporting this conclusion comes from findings about the editorial pages, where explicit political cues are more apparent than in news reports and are more accurately perceived by readers. Ironically, as the American press has become more professionalized and has moved closer to the ideals of information providers in a democracy, its persuasive influence on voter preferences has probably diminished.

APPENDIX

We conducted a national telephone survey using the CATI (computer assisted telephone interviewing) facilities of the Center for Survey Research at Indiana University. We selected counties as our primary sampling units. All the counties in the United States were stratified by population size, education, and change in population since the prior Census; this produced 20 strata that each contained 5% of the population. A replicate design was employed in which two counties were independently chosen from each cell with replacement. Los Angeles County was randomly selected in both samples, so there were actually 39 different counties.

The telephone list was provided by Survey Sampling using random-digit dialing procedures. Within each household, residents were listed by age, and then a respondent was randomly selected. Extensive callbacks were made. The survey yielded a random sample of 1,318 voters, plus a supplemental sample of 271 spouses and 841 personal discussants for these main respondents (see Huckfeldt et al. 1995). This total N produced approximately 33 respondents per county; the number seems small, but it provides a reasonable basis for estimating sample statistics for counties (rather than individual results for specific counties).

The interviews averaged a bit more than an hour; they began during the week after the election and were completed by the end of January. The response rate was 48%, calculated as the ratio of completions to the sum of completions, refusals, and partial completions. Alternatively, if we include subjects who were never available after multiple callbacks, the response rate is 45%. There is a sampling bias in the

TABLE A-1. Sample Points and Newspapers

County	Largest City	Newspaper	Circulation (000s)
—	—	*USA Today*	
Los Angeles, CA	Los Angeles	*LA Times*	1,196
Marin, CA	San Rafael	*San Francisco Chronicle*	563
		Marin Independent Journal	41
Orange, CA	Santa Ana	*LA Times-OC*	
		Orange County Register	353
San Joaquin, CA	Stockton	*Stockton Record*	54
Sutter, CA	Yuba City	*Appeal Democrat*	227
Hartford, CT	Hartford	*Hartford Courant*	228
Hernando, FL	Brooksfield	*St. Petersburg Times*	353
		Tampa Tribune	290
Gadsen, FL	Quincy	*Tallahassee Democrat*	260
Bibb, GA	Macon	*Macon Telegraph*	274
Allen, IN	Ft. Wayne	*Journal Gazette*	263
		News Sentinel	256
Lake, IN	Gary	*Gary Post-Tribune*	70
Johnson, KS	Olathe	*Kansas City Star*	287
Prince George, MD	College Park	*Washington Post*	780
Washington, MD	Hagerstown	*Herald Mail*	239
		Morning Herald	222
Bristol, MA	New Bedford	*Standard Times*	244
Kent, MI	Grand Rapids	*Grand Rapids Press*	144
Wayne, MI	Detroit	*Detroit Free Press*	981
		Detroit News	500
Aitkin, MN	Aitkin	*Brainerd Daily Dispatch*	17
Simpson, MS	Menderhall	*Clarion-Ledger*	102
Jackson, MO	Kansas City	*Kansas City Star*	287
Hunterdon, NJ	Flemington	*Star-Ledger (Newark)*	476
Bernadillo, NM	Albuquerque	*Albuquerque Journal*	121
New York, NY	New York	*New York Times*	1,108
		New York Daily News	1,097
Onondaga, NY	Syracuse	*Post Standard*	289
Mecklenburg, NC	Charlotte	*Charlotte Observer*	236
Ashtabula, OH	Ashtabula	*Star-Beacon*	19
Clarke, OH	Springfield	*Springfield News-Sun*	36
Hamilton, OH	Cincinnati	*Cincinnati Enquirer*	199
		Cincinnati Post	104
Columbia, PA	Bloomsberg	*Press-Enterprise*	22
Lancaster, PA	Lancaster	*Intelligencer Journal*	44
Perry, PA	Bloomfield	*Harrisburg Patriot*	
Marion, SC	Marion	*State (Columbia)*	145
Minnchaha, SD	Sioux Falls	*Argus Leader*	58
Lamar, TX	Paris	*Paris News*	12
Tom Greer, TX	San Angelo	*Standard Times*	33
Chitterden, VT	Burlington	*Burlington Free Press*	
York, VA	Williamsburg	*Daily Press*	
Pierce, WA	Tacoma	*Morning News-Tribune*	117
Marathon, WI	Wausau	*Wausau Daily Herald*	25

direction of the politically active; the sample overestimates voter turnout in comparison to Census figures. The vote shares average within 1.6% of the official vote totals, however, which is more accurate than the NES postelection sample. Our sample was more educated than the national adult population (though less than the population of voters), as is typical of telephone surveys. It roughly matches the population in gender and racial distribution. The survey is deposited with the Inter-university Consortium for Political and Social Research in Ann Arbor (Beck et al. 1995).

For each of these counties, we collected the major local newspaper, a list of which is presented in Table A-1. For each newspaper we coded every third day during this period and every Sunday; a maximum of 27 days were coded for each paper, depending on its circulation pattern. For each sampled day we coded all articles that dealt with the campaign in the first half of the news section. In addition, we coded any articles that appeared under a separate election or campaign heading later in the news section; we also coded all campaign articles that appeared on the editorial pages. A total of 6,537 separate articles were coded.

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Minority Representation in Multimember Districts

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Given the recent court rulings against racial gerrymandering, the effect of multimember district elections on minority representation is an important issue. We present a model of voting in double-member district elections with two majority candidates and one minority candidate and consider the voting equilibria under straight and cumulative voting. In straight voting, while an equilibrium always exists in which the two majority candidates are expected to win the two seats, minority candidates may be elected. In cumulative voting, minority candidate wins are also possible in equilibrium but are less likely when minority voters prefer one majority candidate over another. We then present experimental evidence showing that minority candidates win significantly more seats in cumulative than in straight voting elections. When minority voters perceive a substantial difference between the majority candidates, however, they are more likely to split their votes between the minority and majority candidates, winning fewer seats.

A fundamental issue in modern democracies is the extent to which the electoral system facilitates representation of multiple interests. In particular, the degree to which the diversity of both the interests and the characteristics of the electorate are "represented" by its legislators is a basic concern when choosing or evaluating electoral rules. Political scientists distinguish between two forms of representation. *Descriptive representation* refers to the demographic characteristics of the electorate, while *substantive representation* refers to the policy interests of the electorate (Pitkin 1967, Swain 1993). Although many commentators argue that the latter is most important, the two forms of representation are linked in significant ways. In particular, if features of the political system prevent one demographic group from achieving descriptive representation, then it may be highly unlikely that the group will be able to achieve substantive representation of its interests. Thus, the effect of electoral rules and other political institutions on both the characteristics and the policy positions of elected officials cannot be ignored.

Since the passage of the 1965 Voting Rights Act, scholars and practitioners in the United States have debated how to achieve representation—both descriptive and substantive—of racial and ethnic minorities. Early court cases and electoral reforms before and shortly after the Voting Rights Act concentrated on increasing minority voter participation through removing barriers to registration and access to the ballot. Empirical evidence shows that these efforts successfully increased minority registration and voting rates.[1]

Nevertheless, the number of minorities elected to federal, state, and local legislatures remains far lower than the minority portion of the population might warrant. African Americans and Latinos comprised 21.1% of the American voting-age population in 1996, but only 10.6% of the members of Congress elected that year were from these groups, and similar discrepancies persist in state and local legislatures. Beginning with the Supreme Court decision in *Allen* v. *State Board of Education* 1969, reform efforts shifted from increasing participation to removing other barriers to minority representation. In *Allen*, the Court argued that the preclearance provisions in Section 5 of the Voting Rights Act should also apply to factors which might "dilute" the force of minority votes that were appropriately cast and counted. Most subsequent debates and court cases have focused on how electoral systems dilute or enhance minority voting strength and representation even after the Voting Rights Act removed barriers to minority participation.

One area of particular concern in these debates has been the ability of minority voters to achieve representation in legislative bodies. The primary focus is on how electoral laws and procedures affect the ability of minority voters to elect minority candidates. In the United States, members of Congress are chosen in single-member district elections.[2] Members of local

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Earlier versions of this article were presented at the 1995 annual meetings of the Public Choice Society; the 1997 Political Methodology summer meetings; and the 1997 annual meeting of the American Political Science Association. The Office of the Vice President for Research at the University of Iowa provided funding for the experiments. We thank Greg Adams, Chris Fastnow, Kristin Kanthak, and Dena Levy for their assistance with the experiments. We also acknowledge the helpful comments and suggestions of Chris Achen, Gary Cox, Mark Fey, Dean Lacy, and Samuel Merrill. All remaining errors are, of course, the authors' responsibility.

[1] By 1967 minority voter registration rates had risen more than 50% in all the states covered by the 1965 Voting Rights Act (see Grofman, Handley, and Niemi 1992). Filer, Kenny, and Morton (1991) show that by 1980, after controlling for demographic and other relevant variables, minority turnout rates in presidential elections had become roughly equivalent to those of nonminority voters.

[2] This has not always been the case. As Cox (1984) notes, the use of multimember districts stems from our English heritage: "The modal type of district in England from the thirteenth through most of the nineteenth century was double-member, with the next most common types being single- and triple-member. Early records from the colonies indicate that representatives to colonial legislatures were returned from the same mix of districts, although there was a

and state legislatures are typically chosen in single-member or multimember district elections.[3] Both types of elections are generally "winner-take-all," that is, the top vote receiver (in single-member districts) or the top x vote receivers (in districts with x members elected) are chosen. In some cases the winner must also secure a majority of the votes cast or face the closest opponent(s) in a runoff election.[4] The standard procedure used in most U.S. elections—both single-member and multimember—is straight voting. Each voter has one vote per office and can cast up to one vote per candidate. In a single-member district election, then, each voter can cast one vote for a single candidate or abstain. In a double-member district election, each voter can cast one vote for each of two candidates, one vote for only one candidate, or abstain.

Many in the scholarly and legal communities argue that, under straight voting, single-member districts allow minority voters to elect minority representatives more easily than do multimember districts. To support this argument, proponents point to the following example. Consider a double-member district election under straight voting. Suppose one candidate supported by a nonwhite minority of voters in the district faces two candidates supported by the white majority bloc. Each voter has two votes. If voting is sincere and strictly racial, then we would expect whites to split their votes between the white candidates and nonwhites to cast only one vote for the nonwhite candidate.[5] The two white candidates will receive the most votes (i.e., they will tie), and the minority candidate will come in third. If instead the district is divided into two single-member districts, such that one has a majority of nonwhite voters, and voters again vote along racial lines, then the minority candidate will be elected. By this logic, minority votes are less diluted in single-member districts than in multimember districts.

In addition to hypothetical examples, proponents of single-member districts point to empirical evidence showing that local governments in the post-Reconstruction South moved to multimember districts in order to dilute African-American voting strength (see Kousser 1992). For the voting population in 1988, Welch (1990) demonstrates that a gap exists between the number of minorities elected under at-large districts versus single-member districts. Similarly, two candidates from one party may "squeeze out" a candidate from a minority party in double-member district elections. Citing evidence from multimember district elections in Michigan, Ohio, and England, Cox (1984) reports that the empirical evidence of such domination is mixed.

While multimember districts have not been declared unconstitutional per se, Section 2 of the 1982 Amendments to the Voting Rights Act does allow for challenges to these systems if they "result in the denial or abridgment of the right of any citizen on the grounds of one's race or membership in certain language minorities" (Bott 1990, 207). Furthermore, many of the court cases since *Allen* have concerned the extent to which multimember districts dilute minority voting strength at the state and local levels. As Justice Thomas points out in his concurrence in *Holder* v. *Hall* (1994, 7):

> Perhaps the most prominent feature of the philosophy that has emerged in vote dilution decisions since *Allen* has been the Court's preference for single-member districting schemes, both as a benchmark for measuring undiluted minority voting strength and as a remedial mechanism for guaranteeing minorities undiluted voting power. See, e. g., *Growe* v. *Emison*, 507 U.S. __, __ (1993) (slip op., at 14); *Gingles*, *supra*, at 50, n. 17 (declaring that the "single-member district is generally the appropriate standard against which to measure minority group potential to elect"); *Mobile* v. *Bolden*, 446 U.S. 55, 66, n. 12 (1980) (plurality opinion) (noting that single-member districts should be preferred in court-ordered remedial schemes); *Connor* v. *Finch*, 431 U.S. 407, 415 (1977) (same). Indeed, commentators surveying the history of voting rights litigation have concluded that it has been the objective of voting rights plaintiffs to use the Act to attack multi-member districting schemes and to replace them with single-member districting systems drawn with majority-minority districts to ensure minority control of seats.... The obvious advantage the Court has perceived in single-member districts, of course, is their tendency to enhance the ability of any numerical minority in the electorate to gain control of seats in a representative body. See *Gingles*, 478 U.S., at 5051.[6]

Recent developments raise serious questions about the extent to which districting can be used to promote minority representation, particularly at the federal level. In congressional redistricting following the 1990 Census, a number of states, including Florida, Georgia, Louisiana, North Carolina, and Texas, engaged in deliberate racial gerrymandering. These plans linked geographically dispersed groups of voters by race in order to maximize the number of districts in which minorities comprise a majority of the electorate. A number of new minority representatives were elected from these districts, but the gerrymandering was subsequently challenged and in several cases overturned. The Supreme Court ruled that these districts violated

tendency also to use larger multi-member districts" (pp. 724–5). Reflecting this mixed heritage, the U.S. Constitution was silent on whether congressional delegations should be elected on a general ticket from each state as a whole or under a districting scheme and left that matter to be resolved by the states or by Congress. Not until 1842 did Congress decide that representatives should be elected from single-member districts (Congressional Act of June 25, 1842. Chapter 47, 5. Stat. 491). U.S. senators are elected from double-member districts with staggered elections.

[3] For figures on the use of multimember districts in the United States, see Cox 1984, Jewell 1970, and Klain 1955.

[4] See Gerber, Morton, and Rietz 1996 for a discussion of majority requirements.

[5] Recall that under straight voting, voters cannot cumulate their votes for a single candidate. The assumption of sincere and strictly racial voting is critical for this example. In our theoretical and experimental analyses in this paper, we consider the consequences of strategic voting under straight and cumulative voting.

[6] In *Holder* v. *Hall* (1994) the Supreme Court ruled against plaintiffs who wished to replace a single elected local administrator with a five-member board elected from single-member districts, which suggests that the Court's bias against multimember districts may be decreasing. Note that the full references for *Grove* v. *Emison* were not available at the time Justice Thomas wrote his opinion. The complete citation is *Grove* v. *Emison*, 507 U.S. 25, 37–42 (1993).

the constitutional rights of white voters by using race as the predominant factor in drawing district lines.[7]

Racial gerrymandering raises a number of issues. Many researchers have begun to recognize its limits as a means of increasing minority representation since it depends on residence choices of minority and majority voters. In fact, it may actually encourage residential racial segregation. Moreover, racial gerrymandering can significantly change the ideological composition of voters in other districts. For example, by concentrating racial and ethnic minorities into one or a few districts, the remaining (majority) districts lack ideologically liberal minority voters and therefore may elect more conservative representatives (see Grofman and Davidson 1992; Cameron, Epstein, and O'Halloran 1996). For all these reasons, racial gerrymandering is clearly limited as a method for enhancing the ability of voters to elect members of Congress who represent both the characteristics and the interests of minority and majority voters.

With multimember districts seen as potentially vote diluting and racial gerrymandering viewed as equally problematic, attaining the goal of increased minority representation seems impossible, short of proportional representation. Yet, a considerably less radical alternative than this is advocated by Guinier (1994): "cumulative" voting in multimember districts. For example, in a double-member district under cumulative voting, voters can cast their two votes as follows: twice for one candidate, once each for two candidates, once for one candidate only, or abstain. All voters have the same number of votes as in the straight voting case (two in this example), but they have more options as to how they can distribute their votes among the candidates.[8]

Proponents argue that cumulative voting it is an effective and equitable method of achieving minority representation. "Like-minded voters can vote as a solid bloc or, instead, form strategic cross-racial coalitions to gain mutual benefits" (Guinier 1994, 14). Specifically, minority groups can cast racially motivated votes, cumulating them on one minority candidate, while white voters split their votes between two or more white candidates. Alternatively, nonwhite voters can form coalitions with white voters if they desire. Thus, without relying on racial gerrymandering or proportional representation, cumulative voting allows minority voters to maximize their representation, either descriptive or substantive, depending upon their unique political circumstances.[9]

Most claims about the effects of cumulative voting are purely speculative, since the procedure is rarely used, and studies of its theoretical effects are even more rare. There are a few exceptions, most notably the work of Merrill (1981) and Cox (1984), discussed below. Some corporations currently use cumulative voting to elect members to their boards of directors. Several small municipal and county governments in the United States also use cumulative voting, as did the State of Illinois before 1982.[10]

Cumulative voting coupled with multimember districts thus represents a potential means for increasing minority representation in legislatures such as the U.S. Congress. Hence, it is important to evaluate carefully the merits of this voting system to identify the conditions under which it indeed increases the likelihood of minority candidates winning office. In this article we examine theoretically and empirically both cumulative voting and straight voting in multimember district elections. Previous theoretical analyses have approached parts of this issue. A number of researchers have examined optimal voter strategies under numerous voting procedures using a decision-theoretic approach. One example is Merrill (1981), who found that, in multimember elections, cumulating for one candidate is an undominated strategy.[11] Cox (1984) considered voting strategies under straight voting and showed that it may be optimal in double-member district elections for voters to "plump" their votes by voting for only one candidate (their favorite) rather than for two candidates.[12] Yet, neither researcher derives equilibrium outcomes and the full slate of equilibrium voting strategies for the different electoral systems, and neither explores the comparative statics of the equilibrium predictions. Instead, they focus strictly on the decision problem of the individual voter.

Other researchers have explored equilibrium outcomes under different electoral systems. Myerson and Weber (1993) and Saari (1994) present equilibrium analyses of straight and/or cumulative voting, but they examine only single-member elections. We show that the tendencies to plump votes in straight voting and to cumulate votes in cumulative voting are related directly to the conditional probabilities of which candidates, in the event of a tie, are likely to tie for the *last* seat rather than first place. Thus, the results from single-member district elections do not generalize to multimember district elections, since analyses of the former cannot reveal the full range of equilibrium possibilities in the latter.

Finally, Cox (1990, 1994) combines analyses of individual choice and equilibrium outcomes under several alternative electoral systems. Cox (1990) examines equilibrium candidate positions under cumulative voting but assumes that voters vote sincerely for their first

[7] See, for example, *Shaw* v. *Reno* (1993) on the Florida districts, and *Bush* v. *Vera* (1996) on the Texas districts.

[8] Since voters have only one vote in single-member district elections, cumulative voting and straight voting in single-member district elections are theoretically identical.

[9] The effects of cumulative voting can be viewed as lowering the threshold of exclusion to minority representation when all voters cumulate for their most preferred candidates. See Rae 1971.

[10] See Glazer, Glazer, and Grofman (1984) for an explanation of cumulative shareholder voting. Adams (1996) and Sawyer and MacRae (1962) analyze the Illinois system. Cole, Taebel, and Engstrom (1990) report on the use of cumulative voting in the City of Alamogordo, New Mexico, and Still (1990) discusses the application of cumulative voting in a number of Alabama county commissions and school boards.

[11] This result is consistent with the more general theoretical results we present below, although in some of the equilibria in our model, voters also may optimally mix between cumulating and splitting their votes.

[12] We also derive this result in our more general model.

preference and that policy is unidimensional. In our analysis, we allow for a multidimensional voting space and permit voters to behave strategically.[13] Cox (1994) investigates strategic voting equilibria in single nontransferable vote (SNTV) systems, which resemble cumulative voting systems, but voters *must* cumulate all their votes on a single candidate. Thus, our results can be seen as an extension of both the early work of Merrill and Cox and the later equilibrium analyses of Cox, Myerson and Weber, and Saari.[14]

In the following section we present our theoretical model, which focuses on three-candidate elections in double-member districts.[15] To capture the concept of minority candidates in our model, we assume a voter preference configuration such that a majority of voters prefer two of the candidates (i.e., the majority candidates) to the third, while a minority of voters prefer the third candidate (i.e., the minority candidate) as their first choice. We consider the voting equilibria in multimember districts under straight voting and then under cumulative voting. The next section presents experimental evidence on multimember district elections under both voting systems. The final section considers the implications of our analyses for minority representation in contemporary U.S. elections.

THEORETICAL ANALYSIS OF VOTING IN MULTIMEMBER DISTRICTS

Candidates, Voter Types, and Preference Distribution

We assume three candidates, D, R, and S, and three types of voters, d, r, and s, who most prefer the respective candidates. Type d and r voters next prefer candidates R and D, respectively. We normalize to 1 the utility a voter receives if her most preferred candidate wins a seat, 0 if her least preferred candidate wins. Type d voters are assumed to receive u_d utility if their second preference, R, wins a seat, $0 \leq u_d \leq 1$. Type r voters are assumed to receive u_r utility if their second preference, D, wins a seat, $0 \leq u_r \leq 1$. Type s voters are divided into two sets: s_R voters, whose second preference is R, and s_D voters, whose second preference is D. Type s_i voters receive u_{si} utility from their second preference, $0 \leq u_{si} \leq 1$. We also assume that $1 - u_{si} \geq u_j$. That is, the decrease in utility for minority voters from their second preference (either D or R) replacing their first preference (S) is greater than the gain in utility to majority voters for replacing their last preference (S) with their second preference (either D or R, respectively).

TABLE 1. Generic Utility Schedule

Voter Type	D	R	S	% of Voters
d	1	u_d	0	n_d
r	u_r	1	0	n_r
s_D	u_{sD}	0	1	n_{sD}
s_R	0	u_{sR}	1	n_{sR}
		$1 - u_{si} > u_j$		

Table 1 presents a summary of the utility schedule. The utilities can be interpreted as follows. Types d and r are majority voters who are divided over which majority candidate, D or R, they prefer. The majority voters receive a high utility (normalized to 1) if their most preferred candidate wins, a lower positive utility if the other majority candidate wins, and the lowest utility (normalized to 0) if the minority candidate wins. We assume additive utility. Thus, since two candidates are elected, majority voters of type j receive a maximum utility of $1 + u_j$ if the two majority candidates win, a utility of 1 if their favorite majority candidate plus the minority candidate win, and a utility of u_j if the other majority candidate plus the minority candidate win.

The minority voters (type s) similarly receive a high utility if the minority candidate wins (normalized to 1) and the lowest utility (normalized to 0) if their least preferred majority candidate wins. Again, since two candidates are elected, minority voters of type s_i can receive a maximum of $1 + u_{si}$ if the minority candidate and their most preferred majority candidate win, a utility of 1 if the minority candidate and their least preferred majority candidate win, and a utility of u_{si} if the two majority candidates win. We also assume that the minority voters comprise a plurality of the electorate but not a majority. We normalize the total number of voters to 1. Thus, we assume that there are n_d, n_r, n_{sD}, and n_{sR} numbers of d, r, s_D, and s_R voters, respectively, such that $1/2 < n_d + n_r < 2/3$. We chose the three-candidate, double-member configuration because we felt it was the most empirically relevant (as noted by Cox 1984). Our configuration also allows us to test the argument, presented earlier, that multimember districts with straight voting are biased against the election of minority candidates.[16]

[13] While we do not explicitly model the policy space of the candidate positions, our formulation allows straightforward translation of voter preference functions over candidates into an issue space of two or more dimensions.

[14] Sawyer and MacRae (1962) present an early theoretical analysis of cumulative voting and an empirical investigation of cumulative voting in Illinois. Adams (1996) presents a more recent empirical analysis testing propositions about candidate positions based on the assumption of sincere voting. In both cases, voter behavior is "black boxed," and the theoretical analyses concern the parties' decisions in terms of number of candidates or location of policy positions. These researchers assume all voters cumulate for their most preferred candidates, which we show may not always be the equilibrium strategy for all voters.

[15] Our choice of three-candidate elections is based on Cox's (1994) analysis, which shows that strategic voting in SNTV systems will lead to an equilibrium in which the number of candidates equals to one plus the number of seats in the district. A special case of cumulative voting, SNTV systems force voters to cumulate votes on one candidate. We later consider how expanding the number of candidates would affect the analysis.

[16] An alternative configuration which yields similar results assumes that minority voters are less than a plurality but that a significant portion of majority voters have as their second preference the minority candidate.

Straight and Cumulative Voting Games

In each election under straight voting, a voter can either abstain or cast one vote each for up to two candidates. Hence, voters can cast one vote each for two, one, or zero candidates. Let (v_D, v_R, v_S) designate a voter's vote vector, where v_i equals the number of votes for candidate i. Then, in straight voting, voters are allowed to cast the following vote vectors: (1,1,0), (1,0,1), (0,1,1), (1,0,0), (0,1,0), (0,0,1), (0,0,0). In each election under cumulative voting, a voter can either abstain or cast up to two votes for any combination of candidates. Again, voters can cast one vote each for two, one, or zero candidates. They also can cumulate their two votes and cast them for a single candidate. In cumulative voting, voters are allowed to cast the following vote vectors: (2,0,0), (0,2,0), (0,0,2), (1,1,0), (1,0,1), (0,1,1), (1,0,0), (0,1,0), (0,0,1), (0,0,0).

In each election, the two candidates that receive the most votes are the winners. Voters receive the sum of utilities corresponding to the winning candidates and their voter type. If a tie for second place occurs between two or more candidates, we assume the winner(s) is selected randomly.

Voting Equilibria

We now present a characterization of the voting equilibria based on the model developed by Myerson and Weber (1993). A voter can affect the election outcome only if two or more candidates receive vote totals that are within one vote and would just qualify them for the last seat. A voter's vote may affect a close two-way race for the first seat, but his vote will not change his utility from the outcome, as both candidates would be elected regardless of how the close race for the first seat is resolved. How a voter perceives the likelihood of various "close races" for the last seat should therefore play a role in her vote choice. These perceptions may arise from a variety of sources, including uncertainty about electorate demographics, candidate positions, voter preferences for candidates, turnout level, and how other voters will vote given all available information. In the experiments we discuss later, we remove many of the sources of this uncertainty. We give all voters information on the number of voters of various "types." We show how each voter type ranks the candidates by showing the monetary payoffs each will receive conditional on each candidate winning. Yet, voters do not know the utility functions of other voters over these payoffs, the uncertainty they perceive, or how they will vote conditional on these factors. This preference and strategic uncertainty can create considerable doubt about the possible election outcome.

We define a near tie (for the last seat) as one in which the votes controlled by a single voter can affect the election outcome. In large elections, the probability of a tie for the last seat is generally extremely small. If voters believe that this probability is zero, then they are indifferent between all vote vectors, and voter behavior is predicted to be random. We therefore assume that voters perceive the probability of a tie for the last seat is positive, albeit very tiny. We further assume:

1. Near-ties are perceived to be much more likely between two candidates than between three or more candidates.
2. The probability that a particular ballot moves one candidate past another for the last seat is perceived to be proportional to the difference in votes cast on the ballot for the two candidates.
3. Voters seek to maximize their expected utility gain from the outcome of the election.

The first assumption means that voters form subjective expectations about tie probabilities. They view three-way ties as sufficiently unlikely that they can disregard the possibility, which greatly simplifies their vote choices. This does not mean that voters compute the actual probabilities of a tie (see, e.g., Fey 1997, Ledyard 1984, Palfrey 1989); rather, they estimate it from their expectations about the electoral outcomes. In this sense our voting model, like that of Myerson and Weber (1993), does not fully specify the process through which voters form their perceptions of tie probabilities. The model would be complicated to the point of intractability if we were to endogenize voter perceptions assuming rational and strategic calculations of the cross-effects of all voter choices on their own choices and the probabilities.

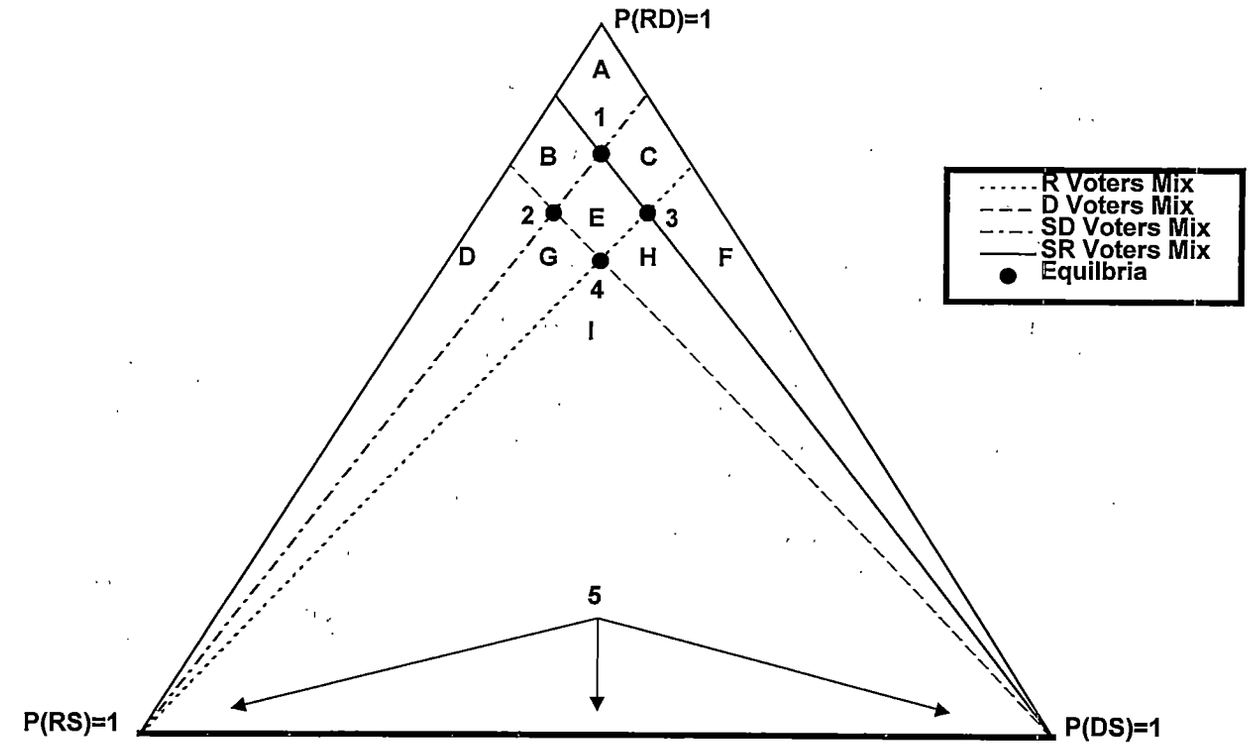
The second assumption implies that voters recognize that casting the same number of votes for two candidates in a close race does nothing to their relative standing and, therefore, does not influence the election. Casting one more vote for the preferred of two candidates in a close race allows some probability of influencing the election in the direction preferred by the voter. Casting two more votes for this candidate further increases the probability of influencing the election as desired. This assumption states that voters believe the increase in probability is proportional to the increase in the vote difference they cast for the candidates. That is, a two-vote difference is twice as likely to affect the outcome as a one-vote difference.

Let $K = \{1, 2, \ldots, k\}$ be the set of candidates and p_{ij} be the ij-pivot probability (perceived by a voter) of the event that candidates i and j will be tied for the second seat in an election. Then, given our three assumptions, a voter who assigns utility U_i to the election of candidate i and perceives the likelihood of a near-tie between candidates i and j to be p_{ij} will cast the vote vector $(v_1, \ldots, v_k)$, which maximizes

$$\sum_{i \in K} \sum_{j \neq i} p_{ij}(U_i - U_j)(v_i - v_j) = \sum_{i \in K} v_i \sum_{j \neq i} p_{ij}(U_i - U_j). \tag{1}$$

As noted above, the perceived probability that two candidates are likely to tie for the last seat is very small. Following Myerson and Weber (1993), we can rescale the vector of perceived tie probabilities to make its components sum to one. In this fashion, the pivot probabilities become perceived "conditional" probabilities. That is, conditional on a tie for the last seat, these

FIGURE 1. Equilibria under Straight Voting with Minority Voter Preferences over D and R



rescaled probabilities are the perceived likelihood that the tie will be between candidates i and j. Voters rationally allocate their votes given these perceived conditional probabilities. At a *voting equilibrium*, the voters' perceptions of the relative chances of various close races support voting behavior that leads to an outcome justifying the original perceptions. Note that voters do not vote monolithically. Therefore, even though the distribution of types is fixed and known, as in Myerson and Weber (1993), voters choose vote vectors without knowing for certain the choices of the other voters, including voters of their own type. An equilibrium exists when their expectations about the electoral outcome are justified by the outcome.

Straight Voting Equilibria. In straight voting elections, voters have two undominated vote vectors: vote only for their most preferred candidate or vote for both their first and second preferences. For example, type s_D voters may maximize expected utility by casting vote vectors (0,0,1) or (1,0,1). Voting for their least preferred candidate, R, or abstaining is always dominated by voting either only for S or for both S and D.[17] Intuitively, if a tie for second place seems likely, voters will vote just for their first preference when they perceive that candidate is most likely to tie with their second preference. They will vote for both when they perceive that their second preference is likely to tie for second with their third preference. Voters will mix between these two strategies when the expected utilities from each vote vector are equal.

Figure 1 illustrates the possible voting equilibria under straight voting. The equilibria are presented as points in the two-simplex of the rescaled perceived conditional pivot probabilities for second-place ties, p_{RD}, p_{RS}, and p_{DS}. We know that $p_{RD} + p_{RS} + p_{DS} = 1$.[18] At the left vertex $p_{RS} = 1$ and $p_{RD} = p_{DS} = 0$; at the top vertex $p_{RD} = 1$ and $p_{DS} = p_{RS} = 0$; and at the right vertex $p_{DS} = 1$ and $p_{RD} = p_{RS} = 0$. Along the left boundary of the simplex, $p_{RS} + p_{RD} = 1$ and $p_{DS} = 0$; along the right boundary of the simplex, $p_{DS} + p_{RD} = 1$ and $p_{RS} = 0$, and along the bottom edge of the simplex, $p_{RS} + p_{DS} = 1$ and $p_{RD} = 0$. At points inside the simplex all the conditional probabilities are positive. Thus, for example, as we move from the left vertex up and to the right, p_{RS} decreases, and p_{DS} and p_{RD} increase.

We can divide the simplex into nine regions according to the optimal voter strategies. In regions A, B, and D, type s_D voters perceive that a tie for second place between their second preference (D) and first preference (S) is less likely than a tie for second place between their second and third preference or between their first and third preference. That is, $p_{RS}, p_{RD} > p_{DS}$. Thus, in these regions type s_D voters vote for both their first and second preference, casting vote vector (1,0,1).

[17] Abstention is a dominated strategy since voting is costless.

[18] We can represent the equilibria in this fashion since the voters' objective functions are homogeneous of degree 1 in the pivot probabilities.

TABLE 2. Vote Vectors (v_D, v_R, v_S) Consistent with the Straight Voting and Cumulative Voting Equilibria

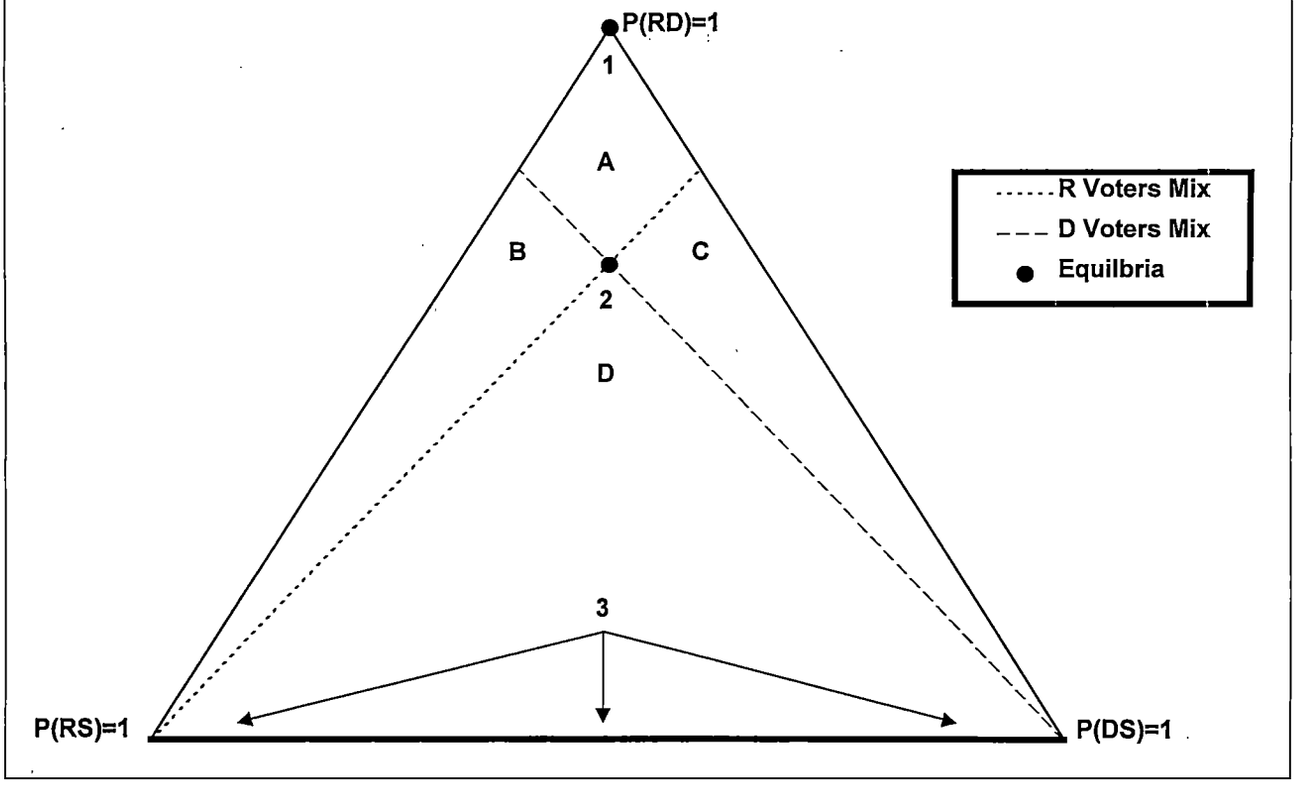
Voter Types	Equilibrium Outcome under Straight Voting				
When $u_{si} > 0$					
	$D \approx R \approx S$	$D \approx R \approx S$	$D \approx R \approx S$	$D \approx R \approx S$	$D \approx R \approx S$
d	(1,0,0)	(1,1,0)&(1,0,0)	(1,0,0)	(1,1,0)&(1,0,0)	(1,1,0)
r	(0,1,0)	(0,1,0)	(1,1,0)&(0,1,0)	(1,1,0)&(0,1,0)	(1,1,0)
s_D	(1,0,1)&(0,0,1)	(1,0,1)&(0,0,1)	(0,0,1)	(0,0,1)	(0,0,1)
s_R	(0,1,1)&(0,0,1)	(0,0,1)	(0,1,1)&(0,0,1)	(0,0,1)	(0,0,1)
When $u_{si} = 0$					
	$S \gg D \approx R$		$D \approx R \approx S$		$D \approx R \gg S$
d	(1,0,0)		(1,1,0)&(1,0,0)		(1,1,0)
r	(0,1,0)		(1,1,0)&(0,1,0)		(1,1,0)
s	(0,0,1)		(0,0,1)		(0,0,1)
Voter Types	**Equilibrium Outcome under Cummulative Voting**				
When $u_{si} > 0$					
	$D \approx R \gg S$		$R \approx S \gg D$		$D \approx R \approx S$
d	(2,0,0)		(2,0,0)		(2,0,0)
r	(0,2,0)		(0,2,0)		(0,2,0)
s_D	(2,0,0)&(0,0,2)		(0,0,2)		(2,0,0)&(0,0,2)
s_R	(0,0,2)		(0,2,0)&(0,0,2)		(0,2,0)&(0,0,2)
When $u_{si} = 0$					
			$S \gg D \approx R$		
d			(2,0,0)		
r			(0,2,0)		
s			(0,0,2)		

In regions C, E, F, G, H, and I, type s_D voters perceive a significant probability that their first and second preference will tie for second place if a tie occurs (i.e., p_{DS} has increased) and thus only vote for their first preference, casting vote vector (0,0,1). Along the boundary of these two sets of regions, voters of type s_D will mix between these two pure strategies. Where the boundary of this region touches the right edge, $p_{RD} = 1 - u_{sD}$, $p_{DS} = u_{sD}$, and $p_{RS} = 0$. Similarly, type s_R voters will cast vote vector (0,1,1) in regions A, C, and F; vote vector (0,0,1) in regions B, D, E, G, H, and I; and mix between these two pure strategies along the boundary between these two sets of regions, where $p_{RD} = 1 - u_{sr}$, $p_{RS} = u_{sr}$, and $p_{DS} = 0$. Type d voters will cast vote vector (1,0,0) in regions A, B, C, E, F, and H; vote vector (1,1,0) in regions D, G, and I; and mix along the boundary, where $p_{RD} = u_d$, $p_{RS} = 1 - u_d$, and $p_{DS} = 0$. Finally, type r voters will cast vote vector (0,1,0) in regions A, B, C, D, E, and G; vote vector (1,1,0) in regions F, H, and I; and mix along the boundary, where $p_{RD} = u_r$, $p_{DS} = 1 - u_r$, and $p_{RS} = 0$.

We can identify five possible equilibria in straight voting, labeled points 1, 2, 3, 4, and 5 in Figure 1 and summarized in Table 2. In the first four of these equilibria, at least two voter types use mixed voting strategies, mixing between voting solely for their first preference and voting for both their first and second preference. At point 1, s_D and s_R voters mix between voting for just their first preference and voting for both their first and second preference, while d and r voters vote only for their first preference. At point 2, s_D and d voters mix, while r and s_R voters vote only for their first preference. At point 3, s_R and r voters mix, while d and s_D voters vote only for their first preference. At point 4, d and r voters mix, while s_D and s_R voters vote only for their first preference. In all four of these equilibria, voters place positive probabilities on all two-way close races for second place, and the equilibrium outcome is a close three-way race among the candidates. The notation $\approx$ represents a close race. Thus, in these four possible equilibria, the outcome is represented as $D \approx R \approx S$; that is, all three candidates are in a close race for first place, and a close race for second place can occur between any two of them. The pivot probabilities in these four equilibria are not all equal, however, and as $u_{si} \rightarrow 0$, then $p_{RD} \rightarrow 1$ in all four of these equilibria (i.e., the expectation is close to certainty that the minority candidate will win a seat, and any close race for the second seat will be between the two majority candidates). In the fifth and final equilibrium, all voters use pure strategies. Minority voters vote only for the minority candidate, and majority voters vote for both majority candidates; that is, the two majority candidates are in a close race for first, and the minority candidate is expected to receive the fewest votes. Thus, any close race for second place will be between the minority candidate and one of the majority candidates but not between the two majority candidates. The expected equilibrium outcome is represented as $D \approx R \gg S$, where $\gg$ represents a strict ranking.

The location and likelihood of these equilibria depend on assumptions about the relative preferences that voters have for their second choice. Suppose we assume that all minority voters are indifferent between the majority candidates and rank both as their second/third preference with utility 0. Figure 2 presents the simplex when $u_{si} = 0$. Minority voters' only undominated strategy is to vote only for their first preference,

FIGURE 2. Equilibria under Straight Voting with No Minority Voter Preferences over D and R



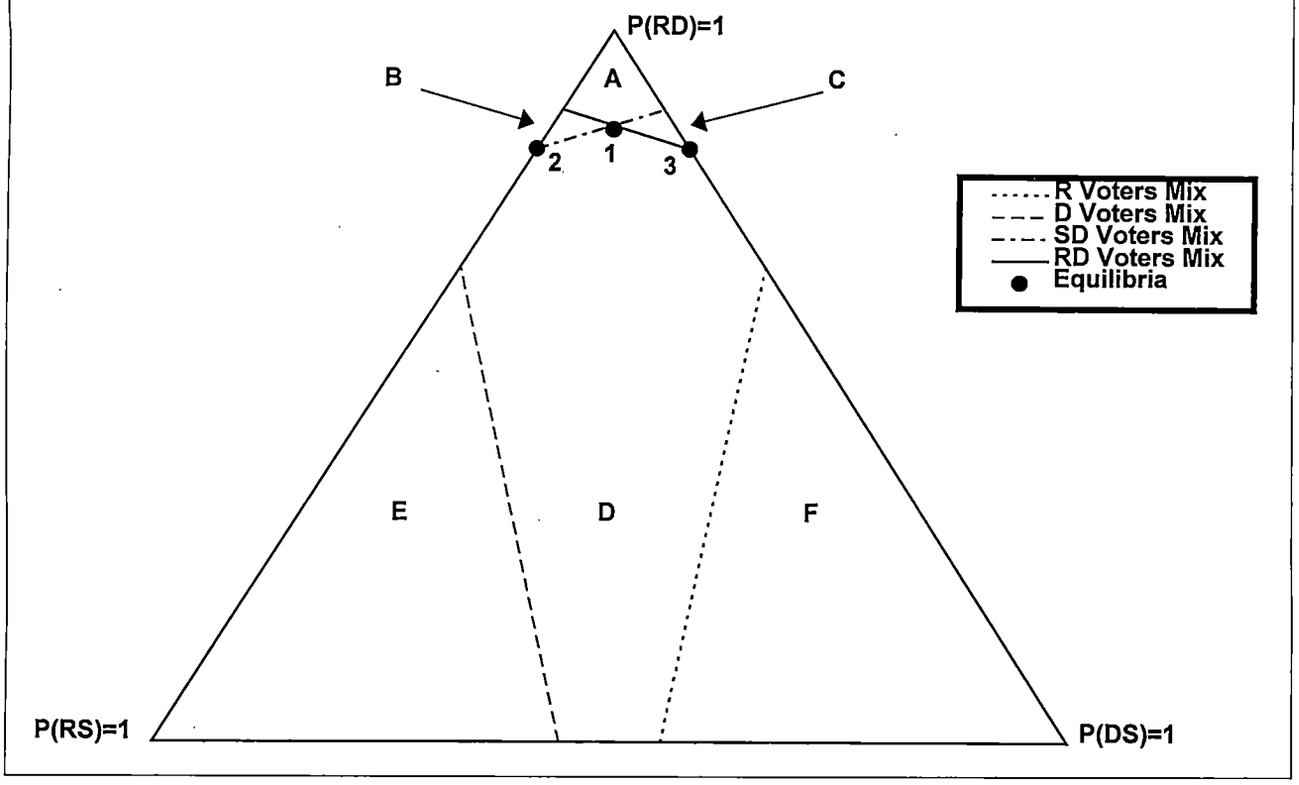
the minority candidate. If voters perceive that $p_{RD} = 1$ (if a tie for second place occurs, it is expected to be between the two majority candidates, and we are at the top vertex of the simplex), then an equilibrium exists in which majority voters also vote only for their first preference, and the equilibrium outcome is expected to be $S \gg D \approx R$.

The important conclusion from this discussion is that there are a number of straight voting equilibria in which the minority candidate is expected to be in a close three-way race and thus has a positive (and high) probability of being selected for one of the seats in the multimember district. Moreover, if minority voters perceive little difference between the two majority candidates ($u_{sD} = 0$ and $u_{sR} = 0$), then it is possible in straight voting for the minority candidate to be expected to receive the most votes and win one of the two seats. Thus, it is quite possible for minority candidates to be elected from double-member districts under straight voting, even when minority voters also prefer one majority candidate over the other. And when minority voters see no difference between the majority candidates, an equilibrium exists in which the minority candidate wins for sure. We summarize these results in proposition 1.

PROPOSITION 1: *Under straight voting, the voting game results in voting equilibria that are consistent with two expected electoral outcomes:* D≈R≫S *and* D≈R≈S. *If minority voters are indifferent between the two majority candidates, then an additional equilibrium exists in which the minority candidate is the expected first-place winner:* S≫D≈R.

Cumulative Voting Equilibria. In cumulative voting elections, voters have the additional option of cumulating their two votes on a single candidate and thus choose among more possible vote vectors. It is straightforward to show (as in Merrill 1981) that an undominated strategy for all voters involves cumulating their votes for a single candidate. Consider, for example, the possible voting strategies of r voters. Clearly, vote vector (0,2,0) dominates (0,1,0), and (2,0,0) dominates (1,0,0). The question remains whether (0,2,0) and/or (2,0,0) dominate (1,1,0). From equation 1, the expected utility for an r voter from (0,2,0) equals $2p_{RD}(1 - u_r) + 2p_{RS}(1 - 0)$; the expected utility of vote vector (1,1,0) equals $p_{RS}(1 - 0) + p_{DS}(u_r - 0)$; and the expected utility of (2,0,0) equals $2p_{RD}(1 - u_r) + 2p_{DS}(u_r - 0)$. For the expected utility of (1,1,0) to exceed the expected utility of (0,2,0), $p_{DS} > (1/u_r)[2(1 - u_r) + p_{RS}]$. For the expected utility of (1,1,0) also to exceed the expected utility of (2,0,0), however, $p_{DS} < (1/u_r)[2(1 - u_r) + p_{RS}]$. Since the two conditions cannot hold simultaneously, either the expected utility from splitting votes is less than the expected utility from cumulating votes for one of the voter's top two preferences, or the voter is indifferent among all three possible strategies. By a similar logic, we can show that the other types of voters will either cumulate their votes for one of their top two candidates or randomize

FIGURE 3. Equilibria under Cumulative Voting with Minority Voter Preferences over D and R



among all three voting strategies. Note, however, that splitting votes between a voter's top two preferences is only a weakly dominated strategy and can be optimal in mixed strategy equilibria. Therefore, while cumulating is always undominated, splitting is also undominated for some voters in many of the voting equilibria we describe below.

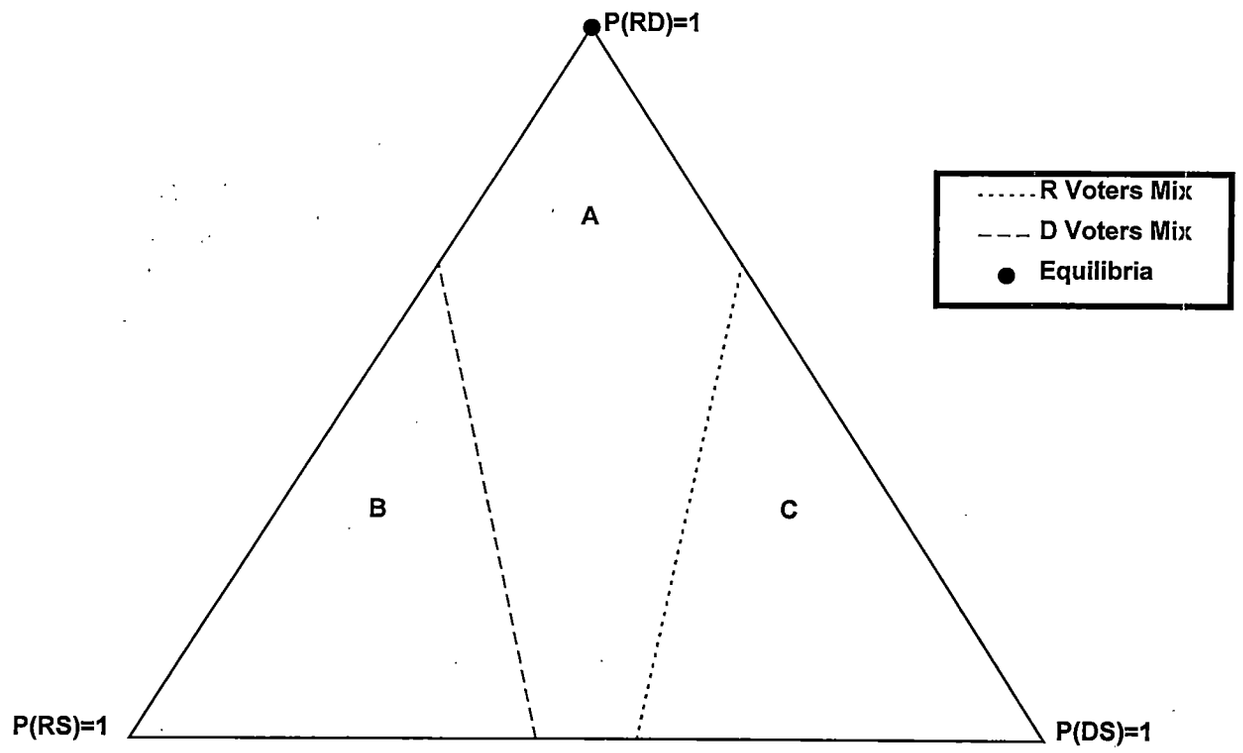
We need to consider the possible equilibria in which voters either cumulate votes for one of their two top preferences or use mixed strategies. Figure 3 presents a pivot probability simplex for cumulative voting. In regions A and C, s_R voters cumulate their votes for their second preference (R), since they perceive that R and D are likely to be in a close race for second place. In regions B, D, E, and F, they cumulate their votes for candidate S, and on the boundary they mix between the three voting strategies (0,0,2), (0,2,0), and (0,1,1). In regions A and B, s_D voters cumulate their votes for D; in regions C, D, E, and F, they cumulate their votes for S; and on the boundary they mix. Type r voters cumulate their votes for R in regions A, B, C, D, and E; cumulate for D in region F; and mix on the boundary. Type d voters cumulate their votes for D in regions A, B, C, D, and F; cumulate for R in region E; and mix on the boundary.

There are three possible equilibria in the cumulative voting game, points 1, 2, and 3 in Figure 3 (see also the summary in Table 2). At point 1, s_D and s_R voters mix between their pure strategies, and d and r voters cumulate for their first preference. At point 2, s_D voters mix, s_R voters cumulate for the minority candidate, and majority voters cumulate for their first preference. At point 3, s_R voters mix, and s_D, r, and d voters cumulate. At point 1, all the conditional pivot probabilities are positive, and the equilibrium outcome is expected to be a close three-way race, $D \approx R \approx S$. At point 2, $p_{DS} = 0$, $p_{RS} = u_{sD}(1 - u_{sD})$, $p_{RD} = 1 - [u_{sD}(1 - u_{sD})]$, and the equilibrium outcome is expected to be $D \approx S \gg R$. That is, because the conditional probabilities that both D and S will be in a close race with R for second place are positive if a close race for second occurs, minority voters of type s_R mix. At point 3, $p_{RS} = 0$, $p_{DS} = u_{\{sR}(1 - u_{\{sR\}})$, $p_{RD} = 1 - [u_{sR}(1 - u_{sR})]$, and the equilibrium outcome is expected to be $R \approx S \gg D$. Note that as $u_{si} \rightarrow 0$, then $p_{RD} \rightarrow 1$ in all three equilibria, and voters perceive a very high probability that the minority candidate will win a seat.

If minority voters are indifferent between the two majority candidates, that is, $u_{si} = 0$, $\forall i$, then there is only one unique voting equilibrium with the expected outcome $S \gg D \approx R$ in which the minority candidate wins a seat for sure. This case is illustrated in Figure 4.

In summary, with cumulative voting in multimember districts, the minority candidate is likely to be in either first or second place in all the possible equilibria. As with straight voting, the less difference minority voters perceive between the two majority candidates, the more likely are equilibria to exist in which the minority candidate has a high probability of winning. The cumulative voting results are summarized in the following proposition.

FIGURE 4. Equilibria under Cumulative Voting with No Minority Voter Preferences over D and R



PROPOSITION 2: *In cumulative voting there are three possible equilibria that correspond to electoral outcomes* D≈R≈S, D≈S≫R, *and* R≈S≫D. *If minority voters are indifferent between the two majority candidates, then there is only one unique equilibrium that results in electoral outcome* S≫D≈R.

Our theoretical analysis is limited to double-member district elections with three candidates. As in the design of the experiments, we chose this configuration because it makes the exposition clear and simple, reflects the most likely configuration when candidate entry is endogenous (Cox 1994), and presents the case in which minority voters are most likely to use both voting systems to advantage the minority candidate. An interesting extension would be to increase the number of candidates. While a full analysis of the possible equilibria is beyond the scope of this article, we can speculate on some of the possibilities. For example, suppose there are two minority candidates instead of one, and minority voters view each equally. Minority voters now face a coordination problem. Equilibria still exist in which one of the minority candidates can win in both voting games, but that requires minority voters to coordinate their votes on one of the two minority candidates, and additional equilibria are likely to exist in which minority voters do not coordinate. If minority voters differ in their preferences for the minority candidates (even if just slightly), then the coordination problem becomes more difficult. Similar coordination problems also would arise for majority voters if there were more than one *D* or *R* candidate. Experimental evidence shows, however, that in coordination problems in simple plurality rule, single-winner voting games, voters willingly coordinate using polls or campaign expenditures (for the experimental evidence, see Forsythe, Myerson, Rietz, and Weber 1993, 1996; Myerson, Rietz, and Weber 1996; for a theoretical analysis, see Fey 1997).

THE EXPERIMENTS

We developed experiments to test our theoretical predictions about voting behavior and election outcomes under straight and cumulative voting in multimember elections. The experimental method is necessary because data from actual elections of this type are rare. Due to some recent court decisions, cumulative voting has been adopted in a few small local elections. Cole, Taebel, and Engstrom (1990) and Still (1990) examined voter survey and aggregate voting data in local U.S. elections with cumulative voting and found some evidence that it increased the election of minorities. Nevertheless, cumulative voting is not often used, and most of the available data tend to be aggregated.

Several studies analyze aggregate election data from multimember elections. Cox (1984), for example, examined outcomes in double-member district elections with straight voting, particularly historical situations in which there were two parties but one was more dominant. Cox found that in Indiana and Michigan the majority party typically won both seats, while in Ohio

and England there were more split outcomes. While these mixed empirical results are consistent with our theoretical predictions (i.e., both may be equilibria, depending upon the perceived probability of a second-place tie), data aggregation may obscure the factors at work. Cox (1984) also investigated voting behavior in multimember district elections in England. He estimated the probability of a tie from the ex post election outcomes and then analyzed actual vote vectors from the election records. He found that voters who perceive a high probability that their most preferred candidate is in a close race for second place will vote for only their first choice. When they perceive a high probability that their second choice is in a close race for second place (but not with their preferred candidate), they vote for both their first and second choice. In both examples, Cox arrives at results consistent with our theoretical analyses of straight voting, but the aggregate data do not allow him to link these outcomes to specific individual-level behavior.

Another reason for using the experimental approach is that it allows us to compare the two electoral systems while holding voter preferences constant. In the experiments, we tied each subject's payment for participation in the study to the election outcomes, as described below. Different groups of players received different payoffs for each outcome. Their voting decisions could affect the amount of payment by as much as $20 (the experiment took approximately two hours). Thus, we could induce voter preferences and compare the different voting systems under a given configuration of preferences.

The experimental approach also allows us to consider how cumulative voting outcomes may differ according to the extent of minority voter preference for the majority candidates. That is, the theory predicts that if minority voters prefer one majority candidate over the other, then they are less likely to cumulate their votes on the minority candidate, who thus has a lower probability of winning a seat. By inducing minority voters' preferences, we can compare their voting behavior and the electoral outcomes holding the voting system constant.

Finally, the experiments help identify many of the equilibria we theorize are most likely to occur when voters make consequential decisions. The ability to test which equilibria actually occur represents a major advantage of laboratory elections over research using naturally occurring elections.

Experimental Design

In our experiments, we selected the case of minority voters indifferent between the majority candidates for several reasons. First, this configuration most closely reflects the substantive problem of minority representation. Assuming racially motivated voting, minority voters care more about the characteristics that differentiate between the majority and minority candidates than between the majority candidates. Second, this particular preference configuration allows us to compare the extent to which each electoral system facilitates minority representation. Only when minority voters are indifferent between the majority candidates does the $S \gg D \approx R$ equilibrium outcome exist under both straight and cumulative voting. Comparing the two electoral systems thus allows us to test under which system this outcome occurs more often. Our theory predicts, however, that minority candidates still have a positive probability of winning under both straight and cumulative voting when minority voters are not indifferent between the two majority candidates, so our experimental results are not knife-edged.

We also consider the case in which minority voters prefer D to R under cumulative voting. As discussed previously, when minority voters have a preference for a majority candidate, the outcome $S \gg D \approx R$ is no longer an equilibrium in cumulative voting. The only equilibrium predicted in this case is the one in which minority voters mix between vote vectors (1,0,1) and (0,0,2) and majority voters all cumulate for their most preferred candidate; the expected electoral outcome is then $D \approx S \gg R$. Figure 5 illustrates the simplex for the case in which all minority voters prefer D to R.

The experiments consisted of three sessions of two-winner, three-candidate computerized elections. One session involved straight voting, while the other two used cumulative voting. In the straight session and cumulative I, minority voters are indifferent about the majority candidates; in cumulative II, minority voters prefer D to R. The sessions were conducted at the University of Iowa and drew from a pool of approximately 450 subjects. They were recruited from the population of students attending MBA and undergraduate classes in the colleges of Business Administration and Liberal Arts.

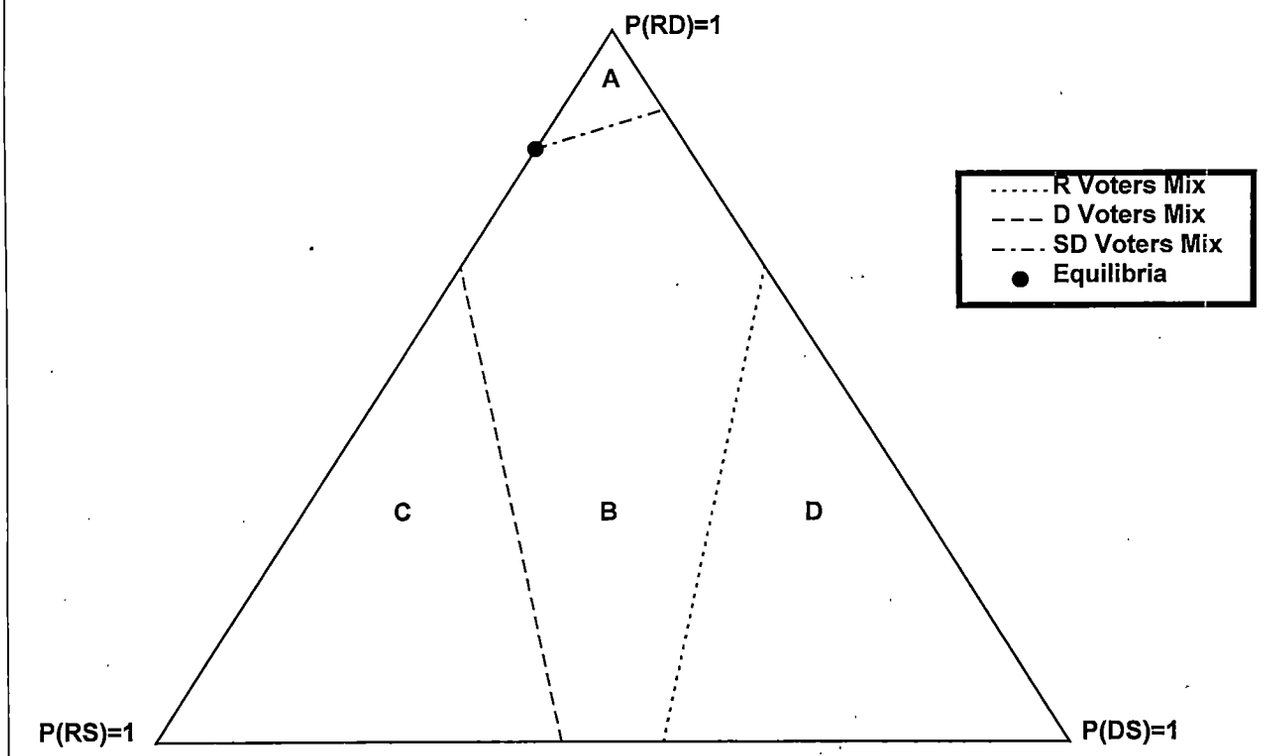
Upon arrival, the subjects were seated at computer terminals in a large classroom and given printed instructions (see Appendix). Dividers concealed each subject's screen from the view of others. The instructions were read aloud and questions were answered in public in order to make all instructional information common knowledge. In the straight session and cumulative I, 28 subjects participated in each session. In cumulative II, 22 subjects participated.[19]

Each subject was given a voter identification number and assigned to an initial voting group consisting of half the subjects in that session. The groups were then divided into three "types," distinguished by the cash payoffs they received conditional upon each candidate winning a seat. Voters knew their own type plus the distribution of voter types in the groups, but they did not know the identity of other voters in their group. Table 3 reports the payoffs used in the experiments.

Each group participated in one election using the voting rule for that session. After each election, sub-

[19] Cumulative II had fewer subjects due to unexpected constraints. We also ran a version of cumulative II for seven periods with 28 subjects before a computer failure and the withdrawal of one subject. We then ran a version of cumulative II for eight repeated elections, with the remaining 27 subjects divided into two groups and ran eight elections (16 total). While these results are not reported here, they show comparable patterns, including nearly equal percentages of outcomes and voting choices, to the cumulative II session.

FIGURE 5. Equilibria under Cumulative Voting with Minority Voter Preference for D Only



jects were randomly reassigned to new groups and new types. They then used new payoff schedules with randomly rearranged and relabeled rows and columns. This allowed us to observe a number of different groups in each cohort while minimizing any repeated game effects that might carry over from one group to the next. In each session, we conducted fifty elections. Thus, each subject belonged to 25 different voting groups and participated in 25 elections. This yielded 150 elections for a total of 1,950 voter responses. Within a group, each individual viewed the same complete payoff schedule, except that her own type was highlighted on her computer screen (see Appendix). In this way, each knew her own payoff, the payoffs to the other voter types in the group, and the number of subjects of each type. Subjects did not know the specific assignment of types of other subjects in the room or who was in their group.

The candidates were named Orange, Green, and Blue to avoid any connotations associated with D, R, and S. For reporting purposes, the voter types and responses have been transformed so that the first listed majority candidate is referred to as D. The outcomes of second-place ties were determined randomly. To do this, we placed colored balls corresponding to the "names" of the tied candidates in a bucket and asked one of the subjects to draw out a ball without looking. That candidate was declared the winner of the tie. After each election, subjects were informed (on their screen) of the vote totals received by each candidate, the winners, and their own payoff.

TABLE 3. Experiment Payoff Schedule

	Election Winners			
Voter Types	D	R	S	# of Voters
Straight & Cumulative I				
d	\$0.60	\$0.45	\$0.10	4
r	\$0.45	\$0.60	\$0.10	4
s	\$0.20	\$0.20	\$0.70	6
Cumulative II				
d	\$0.60	\$0.45	\$0.10	3
r	\$0.45	\$0.60	\$0.10	3
s_D	\$0.35	\$0.20	\$0.70	4

Experimental Predictions

Given the experimental design, our theoretical analysis makes the following predictions about straight versus cumulative voting.

In straight voting we expect to find three possible equilibrium outcomes.

1. $D \approx R \gg S$. Voters expect that $p_{DR} = 0$, p_{DS}, and $p_{RS} > 0$. That is, voters perceive that if a close race for second place occurs, it will be between either D and S or R and S, but not between D and R. Therefore, d and r voters find it optimal to vote for both majority candidates and cast the vote vector (1,1,0). Type s voters vote only for the minority candidate and cast the vote vector (0,0,1). Thus, D and R are expected to receive the highest (equal) vote shares, S is expected to receive fewer votes, and the expectation is justified.

TABLE 4. Fraction of Elections Won by Each Candidate

	Election Winners			2-way Tie for	3-way Tie for
	D	*R*	*S*	2nd Place	2nd Place
Straight (50 Sessions)					
Percentage	96.33	97.33	6.33	6	2
Number	48 1/6	48 2/3	3 1/6	3	1
Cumulative I (50 sessions)					
Percentage	52	52	96	12	0
Number	26	26	48	6	0
Cumulative II (50 sessions)					
Percentage	70	40	90	10	0
Number	35	20	45	5	0

Note: Wins are scored as follows: outright win = 1, two-way tie = 1/2, three-way tie = 2/3.

2. $D \approx R \approx S$. Voters expect that p_{DR}, p_{DS}, and $p_{RS} > 0$. That is, it is possible for any of the three candidates to be in a two-way tie for second place, if a close race for second place occurs. Majority voters find it optimal to mix between voting for their first preference and their top two preferred candidates, that is, *d* voters mix between (1,0,0) and (1,1,0), while *r* voters mix between (0,1,0) and (1,1,0). Type *s* voters vote only for the minority candidate and cast vote vector (0,0,1).
3. $S \gg D \approx R$. Voters expect that $p_{DS} = p_{RS} = 0$, and $p_{DR} = 1$. That is, they expect that if a close race for second place occurs, it is between candidates *D* and *R*. Therefore, both the minority and majority voters find it optimal to plump their votes, voting only for their preferred candidate. Type *d* voters choose vote vector (1,0,0), type *r* voters choose (0,1,0), and type *s* voters choose (0,0,1).

In cumulative I we expect to find only one equilibrium outcome: $S \gg D \approx R$. Voters expect, as with the third equilibrium under straight voting, that $p_{DS} = p_{RS} = 0$, and $p_{DR} = 1$. That is, they expect that if a close race for second place occurs, it will be between *D* and *R*. Both the minority and majority voters find it optimal to cumulate their votes, voting only for their preferred candidate. Type *d* voters choose vote vector (2,0,0), type *r* voters choose (0,2,0), and type *s* voters choose (0,0,2).

In cumulative II we expect to find only one equilibrium outcome: $D \approx S \gg R$. Voters expect that $p_{DS} = 0$, $p_{RS} > 0$, and $p_{DR} > 0$. That is, they expect that if a close race for second place occurs, it will be between *D* and *R* or *R* and *S*, but not *D* and *S*. Type *d* voters choose vote vector (2,0,0), type *r* voters choose (0,2,0), and type *s* voters find it optimal to randomize among vote vectors (1,0,1), (2,0,0), and (0,0,2).

Experimental Results

Election Wins. We first consider the number of wins by each candidate type. Table 4 presents these results. We find that the minority candidate wins a seat in very few straight voting elections (6.33%) and wins a seat in most cumulative voting elections (96% in cumulative I and 90% in cumulative II). These differences are highly significant.[20] The results support the hypothesis that a minority candidate win in multimember elections is much more likely under cumulative than straight voting. As predicted, minority candidates won fewer seats in cumulative II than in cumulative I. A difference in means test shows that the difference in the share of seats won by minority candidates in cumulative I and cumulative II is significant at the .10 level (one-tailed test, $t = 1.28$), despite the fact that the relative proportion of minority voters was larger in cumulative II. Also, the two majority candidates won nearly the same fraction of elections under straight voting and exactly the same fraction under cumulative I. As expected, *D* won more seats than *R* in cumulative II (*D* won a seat 70% of the time, while *R* won only 40% of the time, a difference significant at the .02 level, $t = 2.44$, two-tailed test). These aggregate electoral outcomes suggest that in straight voting the equilibrium outcome $D \approx R \gg S$ is most likely. The predicted unique equilibrium outcomes of $S \gg D \approx R$ and $S \approx D \gg R$ in cumulative I and II, respectively, are also supported empirically by these results.

Voting Behavior. To understand the behavior that generates the electoral outcomes, it is important to examine the individual vote strategies in detail. Table 5 reports the vote vectors used by the voters in all the elections. Bold-face type denotes vote vectors that are not strictly dominated. In the straight voting session, 89.5% of *d* voters and 87% of *r* voters cast the vote vector (1,1,0). This behavior is consistent with the equilibrium outcome $D \approx R \gg S$ and reflects one of the strategies in the mixed strategy equilibrium for $D \approx R \approx S$. Very rarely, however, did the majority voters vote only for their preferred candidate (0.5% for *d* voters, 3% for *r* voters), although this was also an equilibrium strategy, consistent with equilibrium outcome $S \gg D \approx R$ and one of the strategies in the mixed strategy equilibrium for $D \approx R \approx S$. Thus, majority voters appeared hesitant to plump their votes and generally

[20] We test the difference in the share of seats won by the minority candidates in straight versus cumulative I and straight versus cumulative II elections. Testing against the null hypothesis that mean (1) − mean (2) = 0, the two-tailed test results in $t = -24.02$, $p = .00$, for straight versus cumulative I, and $z = -16.92$, $p = .00$, for straight versus cumulative II.

TABLE 5. Percentage of Vote Vectors Cast (v_D,v_R,v_S)

Straight Voting	*d* voters	*r* voters	*s* voters
(0,0,0)	1.5%	0.5%	5.7%
(1,0,0)	**0.5**	0.5	0.7
(0,1,0)	0.0	**3.0**	0.0
(0,0,1)	4.0	6.0	**54.0**
(1,1,0)	**89.5**	**87.0**	16.3
(1,0,1)	3.0	2.0	14.0
(0,1,1)	1.5	1.0	9.3
Total	100.0	100.0	100.0
N	200	200	300
Cumulative I			
(0,0,0)	3.0	2.0	0.7
(1,0,0)	1.0	0.5	0.7
(0,1,0)	0.5	0.0	1.3
(0,0,1)	0.5	0.0	1.7
(1,1,0)	**29.0**	**30.0**	2.3
(1,0,1)	3.5	4.5	8.3
(0,1,1)	4.5	4.5	9.7
(2,0,0)	**52.0**	**4.0**	1.3
(0,2,0)	**4.5**	**52.5**	0.7
(0,0,2)	1.5	2.0	**73.3**
Total	100.0	100.0	100.0
N	200	200	300
Cumulative II			
(0,0,0)	0.7	0.0	0.0
(1,0,0)	0.0	0.0	0.0
(0,1,0)	0.0	0.0	0.0
(0,0,1)	0.0	0.0	0.0
(1,1,0)	**30.0**	**26.0**	5.2
(1,0,1)	7.3	3.3	**26.8**
(0,1,1)	1.3	2.7	1.2
(2,0,0)	**51.3**	**9.3**	**2.8**
(0,2,0)	**8.7**	**57.3**	0.0
(0,0,2)	0.7	1.3	**64.0**
Total	100.0	100.0	100.0
N	150	150	250

Note: Boldface type indicates undominated vote vectors.

adopted strategies consistent with the equilibrium in which the two majority candidates are expected to win. This behavior is consistent with the belief that likely ties for second place will be between one of the majority candidates and the minority candidate, not between the two majority candidates.

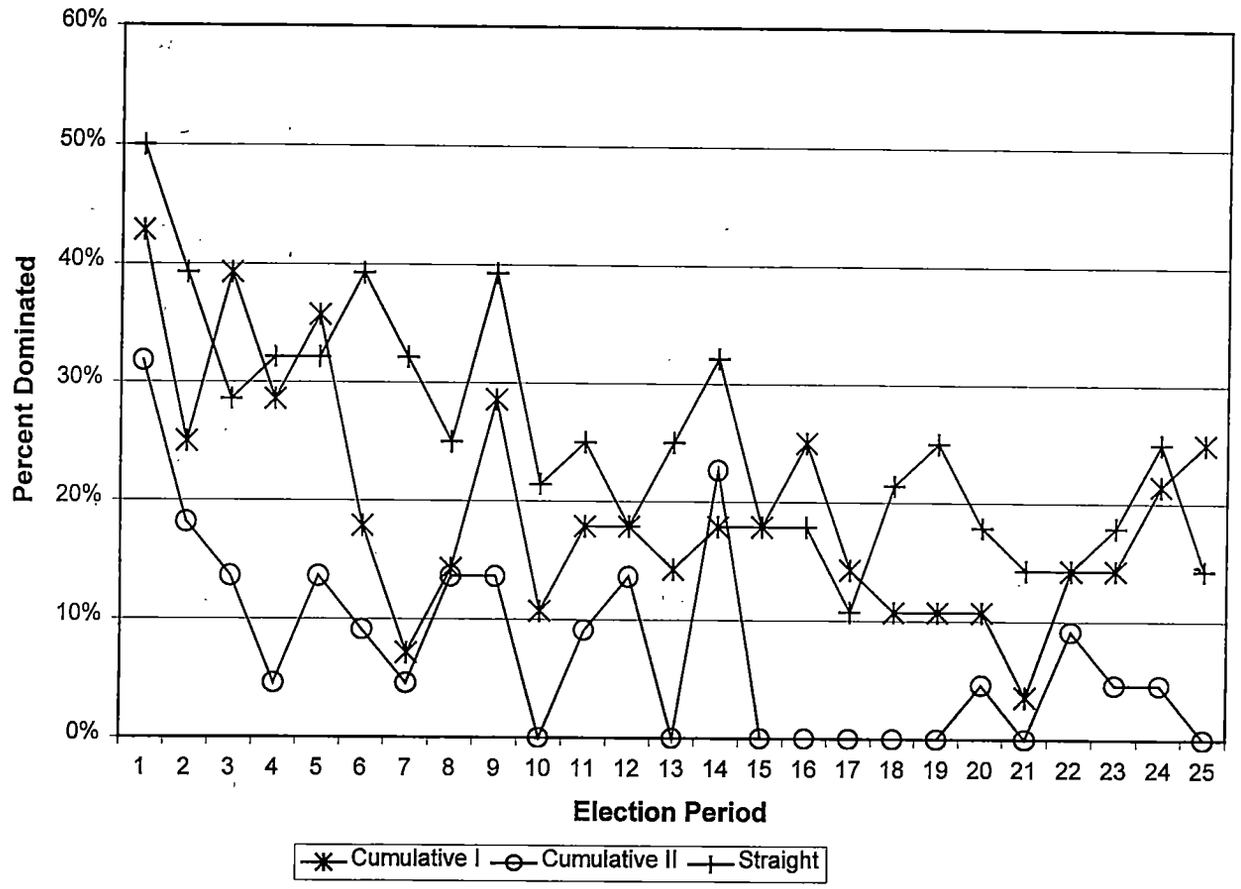
Minority voters in the straight elections, by contrast, used only one of their votes in a majority of cases. As reported in the last column of Table 5, minority voters cast the vote vector (0,0,1) 54% of the time, which is significantly greater than the plumping that occurred among majority voters ($t = -15.05, p = .00$, comparing the share of *d* and *s* voters using an unpaired difference in means test, H_0 = equal means; $t = -13.91, p = .00$, comparing the share of *r* and *s* voters). This strategy was their dominant strategy in all the possible equilibria under straight voting. Still, in a surprisingly large number of elections, they also cast votes for one of the majority candidates, and in 16.3% of the cases they cast two votes for the majority candidates. While the instructions are very clear that subjects are allowed to vote for only one candidate, voters may initially find this uncomfortable if they are accustomed to voting systems in which the expectation is that using all of one's votes is preferred. We analyzed the use of dominated vote vectors by election period to determine whether these mistakes were more likely in early elections, reflecting voter "learning." We found that voters used dominated vote vectors significantly less often as the number of election periods increased, as shown in Figure 6.

In the cumulative voting elections, the tendency to use both votes was again evident. Summing across the three voter types, only 4% of the total vote vectors cast in cumulative I and 0.2% in cumulative II consisted of less than two votes (that is, vote vectors [0,0,0], [1,0,0], [0,1,0], and [0,0,1]). Majority voters cast equilibrium-consistent vote vectors, consistent with the only equilibrium outcome of $S \gg D \approx R$, a majority of the time (52% in cumulative I and 51.3% in cumulative II for *d* voters, 52.5% in I and 57.3% in II for *r* voters). Yet, the second most common vote vector was to split votes for the two majority candidates, vote vector (1,1,0), which was cast by *d* voters in 29% of the elections in cumulative I and 30% of the elections in cumulative II and by *r* voters in 30% of the elections in cumulative I and 26% of the elections in cumulative II. While not an equilibrium-consistent strategy for the majority voters, splitting votes between the two majority candidates is only weakly dominated. In other words, voters may be indifferent between this strategy and cumulating, although they will never strictly prefer it. This vote splitting reflects some unwillingness of majority voters to cumulate their votes. That these percentages are (statistically) equal across the two cumulative voting treatments suggests that they reflect the overall tendency of majority voters to choose to split.

In cumulative I, minority voters cumulated their votes on the minority candidate significantly more often (73.3%) than the majority voters cumulated on their first preference ($t = 5.00, p = .00$, comparing the share of *d* and *s* voters, two-tailed unpaired difference in means test; $t = 4.89, p = .19$, comparing the share of *r* and *s* voters). Minority voters also showed a tendency to split their votes between their first preference and one of the majority candidates, casting vote vectors (1,0,1) or (0,1,1) 18% of the time. The tendency of minority voters not to cumulate their votes in cumulative I may partially reflect the lack of familiarity with a cumulative voting system. Nevertheless, the fact that most minority voters did cumulate shows that they recognized the optimality of this strategy. As Figure 6 shows, the use of dominated vote vectors significantly declined with the number of election periods in cumulative voting elections, as it did in straight voting elections. We interpret this trend as reflecting voter learning.

As predicted, minority voters in cumulative II showed a lower tendency to cumulate their votes on their preferred candidate (64%) than in cumulative I. This difference is significant at the .02 level ($t = 2.37$, two-tailed unpaired difference of means test). Moreover, minority voters in the cumulative II treatment voted significantly more often for their first and second preference (they cast the vote vector [1,0,1] in 26.8% of

FIGURE 6. Percentage of Dominated Vote Vectors Cast by Period and Session



the elections) than did minority voters in cumulative I, who cast this vote vector only 8.3% of the time (t = 5.95, p = .00). Similarly, voters in cumulative II chose vote vector (0,1,1) 1.2% of the time, while in cumulative I minority voters chose this vote vector in 9.7% of the elections (t = 4.29, p = .00). Given the fact that the minority voters constituted very close to a majority of the electorate in these sessions, only a few needed to split their votes between their top two candidates in order for D to have an advantage. Our analysis thus shows that when minority voters have a preference for one majority candidate over another, they tend to cumulate their votes less often, splitting their votes between their first and second preferences.

As a consequence, minority candidates won fewer elections in cumulative II, even though the relative number of minority voters was greater than in cumulative I. If minority voters mix between cumulating and splitting their votes, then there is a lower probability of the minority candidate winning, since there is a positive probability that the minority candidate will be in a close race for second place.

CONCLUSION

We make several contributions in this article. First, we develop and analyze a model of multimember district elections under straight and cumulative voting when a significant minority prefers one of the candidates. We find that in straight voting one predicted equilibrium outcome is for the two majority candidates to win seats and for the minority candidate to finish third, $D \approx R \gg S$. In this equilibrium, the majority voters vote for both majority candidates, and the minority voters vote only for the minority candidate, even when they prefer one of the majority candidates over the other. This outcome often is the basis for arguing that multimember districts disadvantage minority candidates and minority voters.

Yet, we also found a number of straight voting equilibria in which the minority candidate may win a seat. When minority voters have a preference between the majority candidates, these equilibria usually involve two types of voters using mixed strategies, and the outcome is a close three-way race, $D \approx R \approx S$. Finally, when minority voters are indifferent between the two majority candidates, there is an equilibrium outcome, $S \gg D \approx R$, in which all the voters plump their votes, and the minority candidate is the likely winner. In this equilibrium the majority voters expect that, if there is a close race for second place, it will be between the two majority candidates, so they only vote for their first preference.

In cumulative voting we also found a number of

equilibria. The theory predicts that voters in these elections should cumulate their votes or use a mixed strategy, that is, split their votes between their first and second choice and cumulate for either. We find that when minority voters prefer one majority candidate over another, only mixed strategy equilibria exist. Thus, cumulating votes is not the only predicted equilibrium strategy under cumulative voting, and vote splitting under cumulative voting may be an optimal strategy. In these elections, the expected outcome is a close three-way race or a close race for the first seat between the minority candidate and one of the majority candidates. In both types of equilibria, the minority candidate has a positive probability of winning a seat in the multimember district, although the outcome is not certain. When minority voters are indifferent between the two majority candidates, there is only one unique equilibrium: All voters cumulate for their first preference, and the minority candidate is likely to receive the most votes. If a close race for second place occurs, it is between the two majority candidates. Hence, cumulative voting does advantage minority candidates in multimember district elections since, unlike straight voting, in all the possible equilibria the minority candidate has a positive probability of winning.

The second major contribution is to test the theoretical predictions. We used an experimental design that reflects a preference configuration most likely (theoretically) to result in a win for minority candidates in both straight and cumulative elections. That is, in this preference configuration equilibria exist in both types of elections in which the minority candidate wins for sure. We believe that this design provides strong support for the hypothesis that minority candidates are advantaged in cumulative voting over straight voting elections.

We found very few wins for minority candidates in the straight voting elections, even in the preference configuration that can theoretically result in the equilibrium outcome of a sure win for the minority candidate. In examining the individual voter strategies, it appears that in most cases the majority voters used the equilibrium strategy consistent with the minority candidate coming in third (not plumping their votes), and the few minority wins were likely the consequence of a small number of voter errors. In the cumulative voting elections, we found a significantly larger number of minority candidate wins. While majority voters did not cumulate their votes as frequently as predicted and often split their votes (which is a weakly dominated strategy), minority voters did cumulate most often. Thus, minority candidates won a large number of elections. The results, therefore, suggest that cumulative voting can lead to a larger number of wins by minority candidates than can straight voting.

We also considered cumulative voting when minority voters have a preference for one majority candidate over another. We found that, as expected, minority candidates win fewer seats than when minority voters are indifferent between the two majority candidates. A significant number of times, minority voters split their two votes between the minority candidate and their preferred majority candidate rather than cumulate on the minority candidate. These results show that when minority voters have a preference for one majority candidate, cumulative voting can lead to fewer minority candidate wins and less cumulating of votes by minority voters.

The implications for minority representation are straightforward. Research on single-member district elections reveals that majority voters can generally coordinate on such signals as polls or campaign strength to defeat minority candidates (Forsythe, Myerson, Rietz, and Weber 1993, 1996; Myerson, Rietz, and Weber 1996). We show that minority candidates will also generally lose under straight voting in multimember districts.

Yet, a sufficiently large and coordinated minority can elect favored candidates to some of the seats in multimember districts under cumulative voting, particularly if minority voters are indifferent between the majority candidates. In fact, the combination of multimember districts, cumulative voting, and minority voters indifferent about majority candidates virtually assured minority representation in the experimental elections. Thus, for achieving minority representation, we conjecture that this combination can serve as a viable alternative to deliberate (and apparently unconstitutional) racial gerrymandering. The combination is not a guarantee, however, if minority voters have a preference for one of the majority candidates, in which case minority voters are less likely to cumulate on the minority candidate, and minority wins are less likely.

APPENDIX: INSTRUCTIONS FOR STRAIGHT VOTING

General

This experiment is part of a study of voting procedures. The instructions are simple and if you follow them carefully and make good decisions, you can make a considerable amount of money which will be paid to you in cash at the end of the experiment.

The experiment will consist of a series of separate decision making periods. In each period you will have the opportunity to vote in an election with three candidates. The candidates are named Orange ("O"), Green ("G") and Blue ("B"). You must vote according to the rules discussed below. The votes cast will determine the two (2) winning candidates in each election. In the next period, the process will be repeated, with the exception that the identities of some of the members in your voting group will change. Your payoff in each period will depend upon your payoff schedule for the election and on which candidates win the election. (We will describe payoff schedules and the procedure for determining payoffs in more detail later.)

Voting Groups. Initially, each participant will be assigned randomly to one of two groups of voters labeled group "A" and group "B." (When you have been assigned to a voting group, your participant identification number followed by your voting group's letter will appear at the top center of your computer screen.) In each period, two separate and totally independent elections will take place, each involving one of the two groups of voters. Your payoff will depend only on

your decisions and those of the others in your group. The decisions made by the other group of voters will have no effect on your payoffs.

After each period, we will change the voting groups. When this happens, all participants will again be randomly assigned to one of two new groups. After each re-assignment, the members of the group you are in and the individual payoff schedules will generally not be the same as they were previously.

Payoff Rules

In each period, the payoff you receive will be determined by which two candidates win your voting group's election. For each group you are in, a payoff schedule will automatically appear in the upper left corner of your computer screen. There are three types of voters in each group. Voter types differ by their payoffs. The payoff schedule shows your voter type, how payoffs will be determined for your voter type, how payoffs are determined for other voter types and the number of voters of each type. As an example, suppose that you are initially assigned to a group with the payoff schedule displayed below:

Payoff from Winner			
O	G	B	# of Voters
$0.15	$0.35	$0.10	6
$0.35	$0.05	$0.20	6
$0.10	$0.00	$0.50	4

Your payoffs are determined by those listed in the highlighted row. Thus, your payoffs will be determined by the second row of the payoff schedule. You can also see that there are 5 other voters besides yourself with the same payoffs as you. There are 6 voters with payoffs corresponding to the first row of the payoff schedule and 4 voters with payoffs corresponding to the third row of the payoff schedule. Your payoffs each period are determined by your row in the "payoff from winner" section. In the election the top two vote receivers will both be declared winners. You will receive the payoffs listed in your row under the winning candidates. Thus, if you had this payoff schedule, you would receive the following payoffs: $0.35 if the orange candidate (O) is a winner; $0.05 if the green candidate (G) is a winner and $0.20 if the blue candidate (B) is a winner. Since two candidates will be declared winners, you will receive the sum of the payoffs for the two winners. According to this payoff schedule, you will receive three possible sums of payoffs. You could receive: $0.55 if Orange and Blue are the two winners; $0.40 if Orange and Green are the two winners and $0.25 if Green and Blue are the two winners. Thus, for each winner you receive the "payoff from winner" listed under the winning candidate. Remember that this is only an example and does not correspond to the actual payoff schedules used in this experiment.

Voting Rules

Each period, when the election is held, you must decide whether to abstain (not vote for any candidate) or cast a vote in your group's election. If you do decide to vote, you must do so according to the following rule (which applies to all voters in your group):

[Straight Voting: VOTING RULE: You may cast either
1 vote for each of two candidates, or
1 vote for one of the candidates.
You may abstain by casting 0 votes for all three candidates.]

[Cumulative Voting: VOTING RULE: You may cast either
2 votes for one of the candidates, or
1 vote for each of two candidates, or
1 vote for one of the candidates.
You may abstain by casting 0 votes for all three candidates.]

(This rule will also be given in the "VOTING RULE BOX" which will automatically appear on your screen.)

Each period, before the election is held, a "BALLOT BOX" will automatically appear on your computer screen announcing the election. In the BALLOT BOX, the three candidates are listed separately. There is also a number below each candidate. At any given time, these numbers show the votes you would be casting for each candidate should you submit your ballot at that time. After all participants have been notified of the upcoming election, it will be held and you will be allowed to enter votes in the BALLOT BOX and submit your ballot.

When the election is held, a cursor will appear in your BALLOT BOX and you will be allowed to change the number of votes for each candidate on your ballot. To change the number of votes for a candidate, use the ← and → arrow keys to highlight the number below the candidate. Then use the ↑ and ↓ arrow keys to increase or decrease the number of votes you are giving to that candidate. As you change the number of votes for each candidate, the message on the right side of your BALLOT BOX will change. This message tells you whether your ballot is currently (1) valid according to the above rule, (2) not valid according to the above rule, or (3) an abstention.

Prior to submitting your ballot, make sure it is valid. If you submit an invalid ballot, you will receive a message in your BALLOT BOX stating that your ballot is invalid. You will have to change your ballot before re-submitting it. To submit your ballot, press the "Enter" key. If your ballot is valid, you will be asked to confirm your submission. If your ballot is satisfactory, press "y." If not, press "n" and you will be allowed to change the votes on your ballot. Even if you choose to abstain, you must submit a ballot. To abstain, enter zeros (0's) under each candidate and press "Enter." Then confirm this ballot by pressing the "y" key.

After all voters have submitted ballots, the computer will total the votes for each candidate. The votes cast by one voting group will have no effect on the other group's election. For each group, the two candidates with the highest number of votes in an election will be declared the winners of that election. If there is a tie for first place, then these two candidates are both declared the winners. If there is a tie for second place, we will randomly select which of the two second place finishers will be declared a winner. Specifically, we have a tie-breaking bucket and we have three colored balls: an orange one, a green one and a blue one. Balls corresponding to the tied candidates will be put in the bucket and one of you will be asked to randomly draw a ball from the bucket. The candidate whose name is the same as the color of the selected ball will be declared one of winners. If there is a three-way tie, we will put all three balls in the bucket and one of you will be asked to randomly draw two balls from the bucket. The two candidates drawn will be declared the two winners in that election. After the winners of the election have been announced, the computer will determine your payoffs and notify you of the results.

Notification and Recording Rules

After each election, you will be notified of the outcome in the following manner. After the election for your voting group, a "HISTORY BOX" will appear on the right side of your screen. It gives the votes you cast and the total votes cast by your group for each candidate. You may check your recorded vote in the election by comparing it to your previous ballot (which will remain in your BALLOT BOX). The HISTORY BOX also highlights the number of votes for the winning candidates in yellow and gives your payoff for that election. After voting groups are re-assigned, you will begin with a new HISTORY BOX. Thus, your HISTORY BOX only contains information from your current group.

You have also been given several Record Sheets that are similar to HISTORY BOXes. To have a permanent record of the information that will appear in your HISTORY BOXes, you should fill out a line in a Record Sheet for each election. First, record your ID number and group from the top center of your computer screen. Then you should record the event (election), how you voted and the outcome of the election in the spaces provided. You should also circle the vote total of the winning candidates and record your payoff for each election.

When the experiment is completed, the computer will sum your earnings from each election and place the total on your screen. You can confirm this number by summing your earnings recorded on your record sheets. Please place this amount on your receipt. The experimenter will pay you this amount in cash.

If you have any questions during the experiment, ask the experimenter and he or she will answer them for you. Other than these questions, you must keep silent until the experiment is completed. If you break silence while the experiment is in progress, you will be given one warning. If you break silence again, you will be asked to leave the experiment and you will forfeit your earnings.

Are there any questions?

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Structure, Behavior, and Voter Turnout in the United States

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Electoral participation in the United States is examined to provide a clearer account of the effect of the registration requirement on individual voting behavior. Pooling NES data from 1980, 1984, and 1988, I first model, with traditional and selection bias techniques, the full electorate to distinguish among three groups: nonregistrants, registered nonvoters, and voters. Analyses limited to recent movers are then reported to understand more fully the forces associated with the actual decision calculi of registering and voting. The influences of many factors commonly accepted as important determinants of voting are disentangled, and their effect at each stage is ascertained. Factors yielding inconsistent effects in previous research or believed to be unimportant—such as race, gender, attitudes toward the candidates, and trust in government—are shown to deserve closer scrutiny by electoral scholars.

More than seventy years ago, in distinguishing among types of electoral nonparticipants, Merriam and Gosnell (1924, 252) observed: "Entirely different reasons were emphasized by those who were not registered than by those who were registered but did not vote in the particular election." Although more recent discussions have demonstrated important distinctions among groups of nonvoters, some have little to say about the relationship between the institutional structure of electoral participation and individual behavior (Conway and Hughes 1993, Marchant-Shapiro 1994, Ragsdale and Rusk 1993). Merriam and Gosnell's seminal work, however, implies that the structural context in which electoral decisions take place may play an important role in citizen's calculi. This article presents a systematic analysis of these forces to gain further insight into electoral participation.

Many studies have acknowledged that registration and administrative forces may create barriers to participation, but these factors are often used in single models of voter turnout. For both theoretic and statistical reasons, this approach may mask or distort some important relationships. While there are distinct traditions in the electoral behavior literature, there has been much convergence in their extensions, and "hybrid" models are now relatively common (Aldrich and Simon 1986, Dalton and Wattenberg 1993, Dennis 1991). The framework used here extends the hybrid approach by more fully incorporating the short-lived (in its independent state) tradition of the legal-institutional model presented by Rusk (1970, 1971, 1974) and Converse (1972, 1974). "Basically, the theory postulates that legal-institutional properties of the electoral system—ballot and registration systems, voting systems (e.g., plurality, proportional representation), suffrage requirements, and the like—have important effects in *influencing and shaping voting behavior*" (Rusk 1974, 1044, emphasis added). Despite the compelling logic of this conceptualization, it is often considered as an alternative rather than as an approach that might be integrated with individual-level forces. It was largely absorbed by other approaches that added contextual and legal factors, such as registration, as items for which models should control. Although the majority of turnout models argue for putting first things first (i.e., registration), those that incorporate registration and administrative forces have generally treated these simply as factors to be controlled. This is a reasonable approach in aggregate comparisons, such as across nations (Jackman 1987; Powell 1980, 1986) or within the United States between cities or states (Kelley, Ayres, and Bowen 1967; Kim, Petrocik, and Enokson 1975). Even at the aggregate level, however, recent works have incorporated the two-stage process more directly (Cox and Munger 1989, 1990; Fort 1995). For individual-level behavior, the proper incorporation of the institutional structure in our models is even more important to understand the nature of behavior.[1]

REGISTRATION IN THE UNITED STATES

While registration initially may seem to be little more than "democratic housekeeping" (Spafford 1991), a number of competing purposes exist, such as preventing electoral fraud, ensuring that all who are eligible

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A previous version of this article was prepared for the annual meetings of the Midwest Political Science Association, Chicago, IL, April 14–6, 1994. This research was supported by National Science Foundation Dissertation Improvement Grant SBR-9308081. I am indebted to the ICPSR and the University of Michigan Center for Political Studies for making the NES data available. I thank Stanley Feldman and Wendy Hansen for their technical assistance and Richard Brody, Gary Cox, Tali Mendelberg, Kathleen McGraw, Helmut Norpoth, Barbara Norrander, Christopher Wlezien, and Raymond Wolfinger for their helpful comments on earlier drafts. As always, any errors in analysis or interpretation are entirely my own.

[1] Elsewhere, I have developed a four-level taxonomy of relationships between aggregate level contextual and institutional forces and individual-level behavior (Timpone 1994, Timpone and Mealy 1996). In this taxonomy, relationships may be simple and straightforward with either no relationship or a general additive influence that does not alter the underlying behavioral dynamics involved. In such situations, comparative systems could be examined in single models controlling for the different systemic factors of interest. Situations may be more complex, however. At the third level, contextual forces may influence macro and individual-level relationships differently. At the fourth level, the one hypothesized to reflect the influence of registration requirements, aggregate forces fundamentally alter the individual-level task and calculus.

are included on the list, supporting other functions of the system, and lending credibility and legitimacy to the election (Courtney 1991, Kimberling 1991). In balancing these, the United States has customarily given a higher priority to minimizing corruption, at the expense of fuller registration. This, along with the tradition of individualism and the distrust of an intrusive government, led to the general acceptance of the two-step process of participation, whereby an individual has the responsibility to register and only then can go to the polls. While scholars of electoral participation in the United States readily acknowledge this structure, its implications are often not fully appreciated. Fuller consideration of the two-step process leads inevitably to questions about traditional approaches to modeling voter turnout in the United States. Although registration and voting are fundamentally linked acts, and the burden of registration is far less than it has been in the past, they still differ in basic ways. Before passage of the National Voter Registration Act, Rosenstone and Hansen (1993, 136) explained: "Registering to vote is often much more difficult than voting itself, requiring a longer journey at a less convenient hour to complete a more complicated procedure before the peak of the campaign." Thus, the institutional requirement of first registering and then voting creates a process in which the two stages differ in place, time, and kind.

Place

When people decide to cast a ballot on election day, they go to the polling place in their home precinct. This is rarely the same location for registration. Before passage of the National Voter Registration Act of 1993, many states allowed mail registration and deputized registrars, but no universal registration procedure existed. While the need for an onerous trip has been eased over time, registering or obtaining mail registration forms traditionally required a separate trip, even in several states with election-day registration systems. This should not be ignored, since even relatively minor differences in the distance to polling places in cities significantly affects voter turnout (Schlichting and Tuckel 1994).

Time

The temporal span between the tasks of registering and voting has been found to be the strongest deterrent associated with registration (Wolfinger and Rosenstone 1980, Teixeira 1992). One reason for this was noted nearly a century and a half ago, when Tocqueville ([1850] 1969, 135) observed that "as the election draws near, intrigues grow more active and agitation is more lively and wider spread." He considered the time of a presidential election to be like a moment of national crisis. Efforts to enlist support, general campaigning, and even media emphasis increase over time and peak on election day. In the service of curbing fraud and making the administrative task easier, most states close their registration books weeks before the polls open. In 1992, the average closing date was more than three weeks in advance, forcing those not registered in most states to make the initial commitment to vote even before the first of the presidential debates.

Kind

Not only does one need to register significantly in advance of election day in most states, often at locations less convenient than polling places, but also the nature of the registration act is fundamentally different from casting a ballot. Rather than a political decision, a registration form is generally an administrative document. While not overly difficult in recent times, registration remains a fundamentally bureaucratic task with which some people are uncomfortable. President Clinton declared in his remarks at the signing of the National Voter Registration Act of 1993: "Voting should be about discerning the will of the majority, not about testing the administrative capacity of a citizen." This argument underscores the point that registration remains a task fundamentally different from that of actually casting a ballot. And the nature of the task, along with the temporal period when it must be performed, may alter the psychological determinants of each, since "registration usually does not provide the emotional gratification that voting does" (Glass, Squire, and Wolfinger 1984, 52).

This discussion of differences in the tasks of registration and voting should not be interpreted as a claim that either is necessarily onerous. The point remains, however, that they are distinct acts, and an individual must first register and only then can go to the polls in most states. Combining this well-accepted fact with the argument that the two steps differ in regard to place, time, and kind points to a need for closer scrutiny of how this institutional requirement affects individual-level behavior.

ANALYSIS

The ideas set forth above lead to the conclusion that registration, for a variety of theoretical reasons, plays a more complex role in electoral behavior than is sometimes acknowledged. Moreover, this conclusion has statistical implications. The most common approach in turnout models combines the two groups of nonvoters (i.e., nonregistered and registered nonvoters) and compares this combined group to voters, thus collapsing the two-stage process into one (Abramson and Aldrich 1982; Ashenfelter and Kelley 1975; Campbell et al. 1960; Cassel and Luskin 1988; Downs 1957; Ferejohn and Fiorina 1974; Kim, Petrocik, and Enokson 1975; Riker and Ordeshook 1968; Rosenstone and Hansen 1993; Rosenstone and Wolfinger 1978; Teixeira 1987, 1992; Wolfinger and Rosenstone 1980). While these works have greatly extended our understanding of individual-level turnout, explicitly modeling the two-stage nature of the process may yield further insights into participatory behavior. For instance, truly independent factors that affect only one part of the decision calculus (e.g., voting at the polls) will be attenuated to a degree reflected by the size of the group not affected

(e.g., nonregistrants).[2] If these factors are correlated with others in the model, then the problem becomes less predictable and more troublesome. Not only will some forces appear to have different effects on behavior than actually exists, but also the substantively important role of the registration process and behavioral characteristics cannot be separated, clouding our understanding.

The traditional approach is also problematic for understanding behavior if the underlying nature of the relationship between a factor and electoral behavior differs between the two decisions. Some factors not only may affect just one stage of electoral participation but also may influence the two stages in opposite directions. Some people, for instance, may be less likely to vote in general; yet, once they overcome the initial barrier and register, these individuals may be more likely to vote.

More appropriate specifications of the two-step process of voter turnout in the United States reflect the procedure of first registering and then voting among the subsample of the electorate that is registered. Several studies of individual-level behavior have adopted this approach in their analyses (Erikson 1981, Jackson 1996, Mitchell and Wlezien 1995, Uhlaner 1989).[3] Although these specify the two models of registration and turnout of those registered, they treat them as independent models. This is potentially problematic because the universe for the registration equation is the entire electorate, while that for the turnout model only includes those registered, which is clearly not a random subsample of the entire electorate. Problems can occur if the errors in the two equations are correlated. If this is the case, then the error in the second equation—turnout among the registered—will not have an expected value equal to zero, violating standard statistical assumptions and leading to biased results. The appropriate method for examining whether this problem exists is selection bias techniques (Achen 1986, Breen 1996, Dubin and Rivers 1989/1990, Greene 1993, Heckman 1979, King 1989, Maddala 1983).[4]

Dubin and Rivers (1989/1990) developed the technique, used in the analyses below, that explicitly models selection bias in probit models. This approach uses a full information maximum likelihood procedure to estimate the registration stage for the full electorate and then estimates the turnout stage for those registered, correcting for any correlation in the errors between the two stages. Thus, the registration stage of the selection bias model will be practically identical to an individual model of registration. The degree to which the second stage—turnout among the registered—differs from an independent model of turnout that uses this subsample depends on the magnitude of the correlation of the errors. A correlation of zero is equivalent to running the two independent models, while correlations diverging from zero lead to differences based on corrections of the second-stage estimates. The approach addresses both the nonnormality of the errors due to the dichotomous nature of the dependent variables and the nature of the bias between the equations.

In sum, a two-step approach to the analysis of turnout is the most appropriate for theoretical reasons, and a selection bias model is needed to examine and deal with the relationship between the errors. In deciding what factors to include in the models, I use the hybrid approach mentioned earlier, which integrates the classic models of voter turnout. The items included have been examined previously, but the critical issue addressed here is the manner in which the two-stage process operates in the electorate. The dependent variables are the validated *Registration* and *Turnout* measures from the 1980, 1984, and 1988 National Election Study.[5] Five general groups of factors are

[2] While there is disagreement over group size, registered nonvoters cannot be ignored if one is concerned with nonparticipation. According to state election statistics in 1988, registrants account for 42% of nonvoters, compared to 22% in Census data. Regardless of the data source, the group of registered nonvoters in each is too large a proportion of nonparticipants to ignore, as is often the case, and the size of both groups of nonvoters has increased over time. The merits and problems of each data source are debated elsewhere (Bennett 1990a, 1990b; Gans 1990; Piven and Cloward 1988, 1989, 1990).

[3] Ferejohn and Fiorina's (1975) defense of their minimax regret model of voting also made this distinction. They did not explore the issue of registration, but their turnout model only examined those registered. Ignoring the issue of registration, however, leads to the same potential problem from a nonrandom sample seen in the other studies in which the two steps are treated independently. Also, while the work of Mitchell and Wlezien (1995) is the most systematic attempt thus far to study the influence of administrative barriers in both registration and turnout models, their models of turnout use the traditional approach of combining the two groups of nonvoters. In addition to these works, Nagler (1991) demonstrates the appropriate manner for testing for the interaction of such features as education and registration laws, but he also uses the single model approach. Thus, while some previous work has made the distinction between the two stages, the fuller specification and paired analyses presented here allow greater insight into the behavioral mechanisms.

[4] While I argue that selection bias modeling is the appropriate technique for dealing with the problems in the second stage, different models can be employed that reflect distinct underlying processes. The choice rests on theoretic assumptions about the nature of the relationships. The selection bias model is the logical extension of the two-model approach used in the past to incorporate the nature of the process more thoroughly into models of participation (Erikson 1981, Jackson 1996, Mitchell and Wlezien 1995, Uhlaner 1989). The discussion of registration in this article, which distinguishes it from going to the polls in terms of place, time, and kind, not only strengthens the argument for moving beyond the unitary models of turnout but also reinforces the selection bias model as the appropriate likelihood function. Specifications such as an ordered probit ignore the uniqueness of the two tasks and the nonordinal nature of the categories, since some nonregistrants might be more likely to vote on election day, if they were eligible, than registered nonvoters. I believe the difference between these nonregistrants and those who would still fall below the threshold in this case is one of degree rather than kind, maintaining the appropriateness of a two-stage model. While other specifications can deal with the multicategory groups in the electorate and their interrelationship, the selection bias model most directly maps onto the process as theorized in the literature and is capable of dealing with the statistical consequences.

[5] Although past work has demonstrated that self-report and validated measures lead to few substantial differences in studies of turnout, one must be wary when making finer distinctions between the groups, as is done here. In order to avoid the possibility that factors related to the probability of misreporting are included in the model and lead to biases, validated registration and vote measures are used. Unfortunately, the NES decided not to continue validation

included in the analyses: demographics, items that tap social connectedness, general and election-specific political attitudes, and administrative barriers (variable coding is presented in Appendix A). Some of the specific factors included here have appeared only weakly or inconsistently related to participation in past research. This suggests that the finer distinctions made by incorporating the actual structural process involved yields a clearer image of relationships than in previous work, due to the potential attenuation caused by combining groups that may differ behaviorally.

Although some have argued that demographic factors should be avoided in models that attempt to understand behavior, these factors are still useful proxies for items not measured and remain important controls in behavioral models (Achen 1992, Timpone n.d.). General demographics included in this analysis are *Region* (southern versus nonsouthern states), *Age*, *Race*, *Education*, *Income*, and *Length of Residence in One's Home*.[6] Due to the background nature of demographic items, they are expected to have a greater influence on registration, because of their tie to more general predispositions regarding participation, than do those centering on the specific election.

A number of researchers have argued that some demographic factors reflect individual ties to the community (Knack 1993; Merriam and Gosnell 1924; Miller 1992; Pomper and Sernekos 1989, 1991; Rosenstone and Hansen 1993; Straits 1990; Teixeira 1992; Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980). Recent work has demonstrated empirically that a number of these factors reflect an individual's integration into the social milieu (Timpone n.d.), such as frequency of *Church Attendance*, *Membership in Formal Groups*, *Marital Status*, *Home Ownership*, and *Length of Residence in One's Town*. Whether social integration reflects personal self-interest or social norms, the items tapping it are expected to be related to more general political predispositions and to have a greater effect on the act of registration than on turnout at the polls.

Political attitudes linked to longer term predispositions are expected to be distinct from those specific to the election at hand. The more lasting attitudes may be responsive to short-term factors but are likely to be far more stable than those tied to election-specific choices. The long-term predispositions are *External Efficacy* (views of system responsiveness), *Internal Efficacy* (an individual's belief about his or her own political abilities), *Party Differential* (views of whether one party is better able to handle the individual's subjectively perceived most important problem), the *Strength of Party Identification*, and level of *Trust in Government*. The election-specific attitudes are the perceived difference between the major party candidates and whether both candidates are perceived favorably. The general long-term predispositions are expected to be more closely related to registration, whereas the election-specific attitudes are expected to exert a stronger influence on the decision to vote among those registered.

The final set of factors included to distinguish among the electoral groups reflect the administrative barriers faced by individuals in their state. These two factors are the closing date to register and the number of years before individuals are purged from the books. These items were compiled for each election and merged with the individual-level data. In contrast to previous models, here it is conjectured that they should only influence the probability that individuals are registered, not whether they go to the polls once they are registered.[7]

Electoral Participation in the Full Electorate

The two-step selection bias model of the entire electorate is presented in Table 1, along with the traditional single model of turnout that compares all nonvoters to voters (a discussion of the selection bias model is provided in Appendix B). In addition to this comparison, it is useful to compare the independent models of registration and turnout among the registered. As noted, the registration model is almost identical to the selection bias first stage and is thus not presented; the probit model and effect estimates for an independent model of turnout among the registered are provided in the Appendix B table.[8]

While some forces may be overlooked by the traditional operationalization, even those items considered important in past electoral models may not have provided a full understanding of how behavior is related to the two-stage process in the United States. While the results of the single model are sometimes interpreted as demonstrating various forces related to the acts of registering and turning out at the polls, without more sensitive analyses these can be no more than conjecture. The results shown in Table 1 demonstrate, however, that many of the items commonly included in electoral participation models only influence one of the

with the 1992 election, and studies validated before 1980 did not correct for those who were registered out of their district and may still have participated. Thus, the NES studies from 1980, 1984, and 1988 were pooled for analysis in this work. Dummy variables in models comparing 1984 and 1988 to 1980 were insignificant and were eliminated in the final models.

[6] While length of residence in one's home is often discussed as an item tapping one's integration into the social milieu, recent work demonstrates it is a relatively weak indicator of social connectedness that continues to exert a strong effect on turnout, even after social connectedness is more directly controlled (Timpone n.d.).

[7] While the closing date of registration will only affect those who need to register, the number of years before a state purges its records can be related to both the likelihood of being registered and the decision to vote once registered (Mitchell and Wlezien 1995). As Erikson (1981, 274) states, "one reason that people vote may be to stay registered." Thus, the number of years before purging would have a negative effect on going to the polls. Adding the purging variable to the turnout stage of the selection bias model for the full electorate had a negative but insignificant influence on turning out (first difference effect of .01). For comparability between models for the full electorate and recent movers, and due to this weak substantive effect, it is not included in the models presented here.

[8] While the parameter estimates and substantive effects are almost identical for an independent registration model and the registration stage of the selection bias model, there are minor variations in the standard errors estimated from the independent probit and the bootstrapped selection bias model. Length of time in town is significant at the .05 level in the independent model but not in the selection bias model ($p = .09$).

TABLE 1. Pooled Turnout Models for Full Electorate, 1980–88

Variable	Single Model Turnout	Selection Bias Model Registration	Selection Bias Model Turnout
Intercept	−2.1467** (.1838)	−1.9678** (.2118)	.1824 (.6056)
Administrative Barriers			
Closing date	−.0066* (.0029)	−.0078* (.0031)	—
Purge records	.0171* (.0086)	.0295** (.0090)	—
Demographics			
South	−.3788** (.0543)	−.3274** (.0567)	−.2302* (.0944)
Age	.0130** (.0019)	.0127** (.0022)	.0064 (.0033)
Age-squared	−.0002* (.0001)	−.0001 (.0001)	−.0002 (.0001)
Education	.0973** (.0108)	.1031** (.0126)	.0366 (.0202)
Race (black)	−.0843 (.0803)	.1041 (.0891)	−.2910** (.1122)
Gender (female)	−.0433 (.0496)	−.0641 (.0598)	.0059 (.0724)
Income	.0035* (.0014)	.0046** (.0017)	.0003 (.0021)
Time in home	.0129** (.0031)	.0136** (.0036)	.0070 (.0042)
Social Connectedness			
Church attendance	.5765** (.0678)	.4511** (.0735)	.4739** (.1204)
Group membership	.1477** (.0552)	.1512** (.0580)	.0628 (.0861)
Marital status	.1842** (.0535)	.0827 (.0601)	.2390** (.0725)
Time in home	.0010 (.0018)	.0039 (.0021)	−.0038 (.0023)
Home ownership	.2731** (.0591)	.3210** (.0595)	.0115 (.0973)
Political Attitudes: General			
External efficacy	.4884** (.0882)	.5139** (.0949)	.2075 (.1372)
Internal efficacy	.1721** (.0557)	.1473* (.0632)	.1231 (.0795)
Party differential	.1047* (.0496)	.1193* (.0503)	.0171 (.0693)
Strength of party identification	.1607** (.0263)	.1689** (.0268)	.0585 (.0469)
Trust in government	−.1081 (.1090)	−.1841 (.1216)	.0413 (.1503)
Political Attitudes: Election Specific			
Candidate differential	.0044** (.0011)	.0036** (.0012)	.0033* (.0016)
Candidate satisfaction	−.1032 (.0624)	−.0459 (.0663)	−.1361 (.0839)
RHO		−.3550 (.3937)	
n	3598	3598	
LLF initial	−2493.9	−4326.6	
LLF final	−1896.2	−2588.4	

Note: The dependent variables in these models are *Validated Registration* and *Validated Vote*. The full sample size of 3,598 is composed of 954 nonregistrants, 343 registered nonvoters, and 2,301 voters. The administrative barriers were not included in the second stage of the selection bias model. $^{*}p < .05$, $^{**}p < .01$. Standard errors are in parentheses (bootstrapped estimates for the selection bias models).

two stages. In fact, many of the conclusions resulting from traditional turnout models primarily reflect the relationships distinguishing between registrants and nonregistrants (Erikson 1981, Uhlaner 1989). By collapsing opposing influences in the two stages, it is not possible to disentangle where forces are exerted, and substantive relationships can be masked. This is clearly evident with race, which is neither substantively nor statistically significant in the traditional unitary model; once the process is disentangled, however, important relationships are revealed. Thus, if one is interested in fully understanding individual-level behavior, adequate specification of the structural process is clearly necessary.

Table 1 not only shows the usefulness of separating the acts of registering and going to the polls but also provides important information about the relationship between the two stages. The correlation between the two equations in the selection bias model is −.355, which is substantively meaningful, though modest and not statistically significant.[9] A negative correlation suggests that nonregistrants would be *more* likely to go to the polls, if they were eligible to do so, than those who are registered but did not vote.

In order to understand electoral behavior in the two stages, it is necessary to examine the effects of the forces in the model. Although the estimation procedure is more complex, the parameters can be treated as standard probit estimates. Because probit models are not linear and additive, the coefficients presented in Table 1 cannot be substantively compared, since the influence of each variable is dependent on the values of the others. Setting all other variables to their mean value (or base level for dichotomous variables) allows comparison of the effect of each variable on the probability of registering and voting (King 1989).

Table 2, which presents the relative effects of the

[9] While the fully specified selection bias models presented in the text are consistent with those run with more constraints, the correlation's standard errors were more influenced by specification. While the correlation coefficients were insignificant in these fully specified models, in models with more constraints and fewer variables, they were of similar substantive magnitude (although slightly larger in some constrained models) but statistically significant. This further highlights the importance of conducting the selection bias estimation in less fully specified models.

TABLE 2. Influence of Forces on Probability of Participation from Models for Full Electorate

Variable	Single Model Turnout	Selection Bias Model Registration	Selection Bias Model Turnout
Administrative Barriers			
Closing date	−.0867	−.0912	—
Purge records	.0659	.1004	—
Demographics			
South[a]	−.1498	−.1204	−.0482
Age[b]	.2568	.2380	.0601
Education	.3443	.3299	.0642
Race (black)[a]	−.0328	.0345	−.0631
Gender (female)[a]	−.0168	−.0223	.0011
Income	.0961	.1114	.0043
Time in home	.1539	.1396	.0383
Social Connectedness			
Church attendance	.2185	.1522	.0856
Group membership[a]	.0556	.0493	.0110
Marital status[a]	.0689	.0276	.0373
Time in town	.0222	.0741	−.0415
Home ownership[a]	.1003	.0988	.0021
Political Attitudes: General			
External efficacy	.1869	.1750	.0380
Internal efficacy	.0656	.0494	.0218
Party differential[a]	.0397	.0393	.0031
Strength of party identification	.1864	.1768	.0328
Trust in government	−.0308	−.0468	.0055
Political Attitudes: Election Specific			
Candidate differential	.1468	.1052	.0510
Candidate satisfaction[a]	−.0402	−.0159	−.0270

Note: Values in the table represent the change in probability of participation due to varying the value of each variable while holding the others constant. The administrative barriers were not included in the second stage of the selection bias model.
[a]Represents a dichotomous independent variable.
[b]The value of the age effect comes from the joint effect of changing both age variables in the model used to tap possible nonlinear effects.

variables, allows the forces in the models to be compared. Values of two standard deviations below and above the mean are used for comparability of the general range of the influence for each variable.[10] The results presented in Table 2 show that the variables in the models generally distinguish between registrants and nonregistrants to a greater degree than between registered nonvoters and voters.[11] This is especially clear with a number of items emphasized in past models of turnout. Two factors considered important determinants of turnout, including its decline in recent decades, have been the strength of an individual's party identification and beliefs about the responsiveness of the system reflected in the measure of external efficacy (Abramson and Aldrich 1982). As seen in tables 1 and 2, both forces are only significant, statistically and substantively, in the model for registration, not in the actual decision to go to the polls on election day. This is also the case with the other long-term political attitudes, as was expected.

For the most part, the registration model for the full electorate parallels the findings of the traditional single model. Clearly, as in the single model of turnout, the most influential force in distinguishing between registrants and nonregistrants is education. The next most influential is age. A number of other items also exert an important influence: region, church attendance, income, both ownership of and length of residence in one's home, and long-term political attitudes. Finally, the administrative items of closing date of registration and purging records are also significant determinants of registration, as would be expected.

The second stage of the selection bias model, distinguishing between registered nonvoters and voters, provides greater insight into the nature of electoral participation than does past work. As can be seen in Table 2, the forces influential at this stage generally have a smaller effect than those in the first stage, but the relationships are clearer than those revealed in previous research. The weaker effects are not surprising given the nonlinear estimation technique and the gen-

[10] Several variables had skewed distributions, leading the value at two standard deviations from the mean to exceed the variable range. In these cases the value was set at the range endpoint.

[11] Table 2 presents the first-difference estimates, which provide the probability effects of changing the values of variables. While these are intuitive, caution must be used when comparing effects across the stages. This is due to the nonlinear nature of the likelihood functions and the fact that the base probabilities differ (i.e., the mean probability of a person voting once registered is higher than being registered). Using a derivative approach avoids this problem but is less intuitive than first differences (King 1989). While most influences are smaller in the second stage using derivative methods as well as first differences, the gaps are sometimes smaller, and in the case of church attendance they reverse. Thus, at the probability of .5, using the derivative approach, the influence of church attendance is greater in the turnout stage than in the registration stage (for both the full electorate and recent movers), and the effect of candidate differential is comparable in both stages for the full electorate.

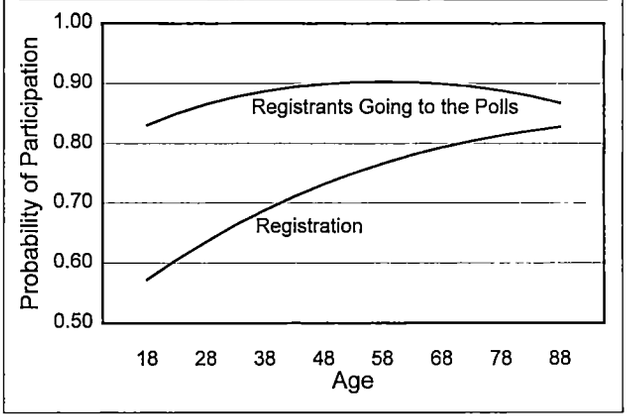
erally high likelihood of turnout among registrants. One of the more influential items at this stage is race, which, as already noted, does not even approach standard levels of statistical significance in the single model. It has been argued that racial differences in turnout no longer exist once other factors are controlled, but this conclusion appears to be incorrect. The analyses presented here suggest that, during the 1980s, African Americans were less likely than others to go to the polls once they were registered, which supports the view that they lacked desirable choices in the general elections of this period (Tate 1993).

Other significant forces are candidate differential, region, marital status, and church attendance. Marital status operates differently from the other demographic and social connectedness factors in that it only influences the turnout stage. This is consistent with the idea that marriage has a "contagion" effect: Less interested and unlikely voters are spurred to the polls by a more involved spouse, since married couples tend to vote or abstain together (Stoker and Jennings 1995, Straits 1990).

Also notable in the turnout model is the smaller effect of education. Its influence on participation is overwhelmingly focused on the registration stage. Once the systematic relationship between the two stages is controlled, education as a factor in turnout is comparable to such factors as race and church attendance but fails to reach traditional levels of significance (p = .070).

It is also evident that the traditional unified approach may enhance relationships that run in the same direction in each stage but fail to attain significance in the finer analysis. This can be seen with the nonlinear relationship between age and participation. While age is among the factors with the largest substantive influence in both stages, the nonlinear term does not reach conventional levels of significance in either stage, whereas it is significant in the unitary model.[12] The connection between age and participation has been well investigated, and the source of the relationship has sparked some controversy (Miller and Shanks 1996, Teixeira 1992, Wolfinger and Rosenstone 1980). Figure 1, which plots the probability of participation by age for each stage of the selection bias model, shows that while age is monotonically related to being registered (over the normal life span), the nonlinear relationship is driven by the probability of going to the polls among registrants. In addition, the critical point occurs at a lower age in the finer analysis (58 years), once the effect of registration is separated, than in the traditional unitary model (75 years).

FIGURE 1. Influence of Age on Participation in the Full Electorate



Electoral Participation among Recent Movers

To this point it is clear that a number of forces distinguish among the three groups in the electorate, but the idea that this reflects decision calculi is a less tenable assumption. A problem with interpreting the above models as reflections of the underlying decision process is that, while the turnout decision is proximal to the surveys, the act of registering is likely to have occurred at some point in the (possibly distant) past. This is highlighted by the monotonically increasing likelihood of being registered over the life span, seen in Figure 1. Other research has shown that the causality between the behavioral forces in these models and participation is not simply unidirectional. Previous research focuses on the potential influence of participation on such attitudes as strength of party identification, beliefs about system responsiveness, and political interest (Brady, Verba, and Schlozman 1995; Converse 1969; Finkel 1985, 1987; Jennings and Markus 1984). While reciprocity between behavior and attitudes may be a "minor" dilemma when examining a decision proximal to measures of the "independent" factors, it becomes a serious concern when examining a behavior that may have occurred years or even decades before. Thus, closer scrutiny would be required for factors that may systematically change after one has registered, such as political interest, campaign attention, and a perceived difference between candidates.

To obtain a clearer picture of the relationship between the two-step institutional structure and individual voting behavior, the models estimated above were repeated with only the subsample of those who had moved in the past two years.[13] This period avoids the potential problem of people registering before the

[12] The parameter for the age variable is significantly different from zero at the .001 level in the registration stage of the selection bias model, but it just misses conventional significance in the second stage (p = .054). As Figure 1 shows, the nonlinear relationship in the unitary model of turnout is mainly driven by the relationship in the second stage of the selection bias model (the parameter on age-squared in the second stage also approaches conventional levels of statistical significance at p = .099). Both the age and age-squared relationships are statistically significant in the independent model presented in Appendix B.

[13] While all individuals who moved may not have reregistered, and some may have voted in their past precinct, this approach gives a cleaner portrait than the previous model of the determinants of both behavioral calculi. It also is the best proxy available for registration proximal to the survey as opposed to the more distant past. This is true despite the fact that movers, like survey respondents, are not a random subsample of the electorate, but these third- and fourth-stage selection bias issues are beyond the scope of this article (see Brehm 1993).

TABLE 3. Pooled Turnout Models for Recent Movers, 1980–88

Variable	Single Model Turnout	Selection Bias Model Registration	Selection Bias Model Turnout
Intercept	−2.8420** (.3253)	−2.4625** (.3950)	−.4136 (1.1724)
Administrative Barriers			
Closing date	−.0128** (.0047)	−.0116** (.0042)	—
Purge records	.0228 (.0155)	.0300 (.0169)	—
Demographics			
South	−.3671** (.0935)	−.2786** (.0979)	−.2949 (.1526)
Age	.0197** (.0050)	.0149** (.0054)	.0192 (.0101)
Age-squared	−.0002 (.0002)	−.0001 (.0002)	−.0004 (.0003)
Education	.1453** (.0200)	.1358** (.0251)	.0706 (.0486)
Race (black)	.0271 (.1387)	.2320 (.1439)	−.3191 (.2185)
Gender (female)	−.0226 (.0848)	−.1800* (.0873)	.2837* (.1355)
Income	.0002 (.0024)	.0024 (.0031)	−.0021 (.0038)
Time in home	.1629** (.0533)	.2088** (.0668)	−.0349 (.0962)
Social Connectedness			
Church attendance	.5754** (.1198)	.4670** (.1169)	.5164** (.1979)
Group membership	.1856 (.0999)	.1486 (.1149)	.1827 (.1367)
Marital status	.2108* (.0904)	.1112 (.1111)	.2506 (.1353)
Time in town	.0008 (.0034)	.0051 (.0034)	−.0051 (.0052)
Home ownership	.2515** (.0935)	.2572* (.1031)	.0991 (.1505)
Political Attitudes: General			
External efficacy	.5864** (.1536)	.5682** (.1588)	.2298 (.2533)
Internal efficacy	.1979* (.0912)	.2043* (.0993)	.0749 (.1386)
Party differential	.0356 (.0851)	.1044 (.1022)	−.1279 (.1448)
Strength of party identification	.0777 (.0468)	.1080* (.0459)	−.0160 (.0778)
Trust in government	−.3882* (.1873)	−.3554* (.1788)	−.2835 (.2726)
Political Attitudes: Election Specific			
Candidate differential	.0062** (.0018)	.0031 (.0019)	.0086** (.0028)
Candidate satisfaction	.0238 (.1118)	.0144 (.1337)	.0093 (.1695)
RHO		−.222 (.5259)	
n	1166	1166	
LLF initial	−808.21	−1301.73	
LLF final	−647.49	−921.14	

Note: The dependent variables in these models are *Validated Registration* and *Validated Vote*. The full sample size of 1,166 is composed of 454 nonregistrants, 124 registered nonvoters, and 588 voters. The administrative barriers were not included in the second stage of the selection bias model. $^{*}p < .05$, $^{**}p < .01$. Standard errors are in parentheses (bootstrapped estimates for the selection bias model).

previous midterm election, thus not reflecting the specific election analyzed, and past work has determined that two years is the critical period for determining the influence of mobility on participation (Rosenstone and Wolfinger 1978; Squire, Wolfinger, and Glass 1987; Wolfinger and Rosenstone 1980). The single turnout and selection bias models for these recent movers are presented in Table 3.

As in the case with the entire electorate, separating the stages enhances our understanding of behavior.[14] This can be seen with gender, which is another example of a variable that behaves in the opposite direction in each stage. Thus, the null findings of the single model of turnout are the result of countervailing forces canceling each other out. When the two steps are taken into account, women who have moved in the past two years are less likely to register than men in the same situation; once women pass this threshold, however, they are more likely to go to the polls.

As with the model for the entire electorate, the parameter estimates cannot simply be compared to understand their influence. Estimates of effect were created for the recent mover models in the manner discussed previously, and these are presented in Table 4.

Overall, the registration model for recent movers is generally consistent with that for the entire electorate. There is substantial overlap in the findings of the two models, although several items gain greater influence among those needing to register at a time more proximal to the survey. Education, for instance, re-

[14] The correlation between the errors in the two models is also statistically insignificant and is substantively smaller for those individuals for whom both tasks are more immediate concerns than for the full electorate, −.222 compared to −.355, respectively. The general magnitude of the correlation among recent movers was even smaller in models with more constraints and never attained statistical significance. In all cases, the magnitude of the correlation was smaller among recent movers than for the full electorate. Thus, there may be less systematic variation among the unmeasured factors for those who have moved recently, and systematic differences may grow as individuals continue to participate or abstain over time. Rather than systematic alteration of the measured items, the differences could be due to the force of habituation of past decisions, consistent with the view of the inertial impetus of continued participation or abstention as self-reinforcing behavior (Brody 1980, Rosenstone and Hansen 1993).

TABLE 4. Influence of Forces on Probability of Participation from Models for Recent Movers

Variable	Single Model Turnout	Selection Bias Model Registration	Selection Bias Model Turnout
Administrative Barriers			
Closing date	−.1791	−.1584	—
Purge records	.0900	.1153	—
Demographics			
South[a]	−.1392	−.1101	−.0867
Age[b]	.2823	.2224	.1742
Education	.4640	.4370	.1633
Race (black)[a]	.0107	.0863	−.0946
Gender (female)[a]	−.0089	−.0708	.0640
Income	.0060	.0639	−.0376
Time in home	.1285	.1602	−.0181
Social Connectedness			
Church attendance	.2259	.1769	.1283
Group membership[a]	.0739	.0561	.0434
Marital status[a]	.0839	.0423	.0575
Time in town	.0121	.0719	−.0510
Home ownership[a]	.1000	.0952	.0245
Political Attitudes: General			
External efficacy	.2289	.2173	.0598
Internal efficacy	.0786	.0781	.0192
Party differential[a]	.0141	.0397	−.0351
Strength of party identification	.0920	.1254	−.0125
Trust in government	−.1122	−.1017	−.0556
Political Attitudes: Election Specific			
Candidate differential	.2179	.1070	.1851
Candidate satisfaction[a]	.0095	.0056	.0024

Note: Values in the table represent the change in probability of participating due to varying the value of each variable while holding the others constant. The administrative barriers were not included in the second stage of the selection bias model.
[a]Represents a dichotomous independent variable.
[b]The value of the age effect comes from the joint effect of changing both age variables in the model used to tap possible nonlinear effects.

mains the most influential force in the registration decision; but the centrality of this factor for participation is even stronger among those who moved recently than in the electorate as a whole. This is consistent with past work demonstrating the influence of education in overcoming the hurdles of moving on participation (Squire, Wolfinger, and Glass 1987). Age and external efficacy also continue to exert a substantively important influence. The closing date of registration exerts an effect comparable to that of a number of other influential attributes in these decision models, such as region, length of residence in one's home, and church attendance. In addition, trust in government attains statistical significance among recent movers, that is, those *less trusting* were more likely to register once other forces are controlled.

Several differences are worth noting between the model of recent registrants and the full electorate in the turnout stage. Consistent with expectations, the single most influential force in bringing recent registrants to the polls is the perceived candidate differential. While this force also exerts an influence on the decision to register, it is central to the decision to vote. Although the effect has been inconsistent in some past studies, examining the two-stage process for those who register shortly before voting supports the intuitive notion that political competition is central to voting behavior (cf. Brody and Page 1973, Rosenstone and Hansen 1993, Weisberg and Grofman 1981). While candidate differential is a significant determinant for both the full electorate and recent movers, the comparatively stronger influence in the second stage among the latter supports the idea of reciprocal causation for long-term residents; that is, those registered farther in the past grow to see greater differences as they participate more.

While church attendance exerts a strong influence on recent mover turnout, several other factors also deserve further investigation in the second stage. Region, marital status, race, and education are substantively related to turnout among registrants, although each fails to attain statistical significance in the selection bias model (p values of .053, .064, .144, and .146, respectively).[15]

Again, the unitary model generally combines the forces in the two stages. This masks some important factors, such as gender and race, whose effects are in the opposite direction in each stage, but it may accentuate the relationship of others. Marital status among recent movers, for example, does not attain statistical significance in either stage of the selection bias model; but in the single model, by combining the effects that

[15] Again, comparison with the independent models in Appendix B is useful, particularly since the correlation between the errors in the two stages cannot be statistically distinguished from zero in this model. In the independent model of turnout among recent movers, region, marital status, and education attain statistical significance.

go in the same direction, its relationship with participation can be statistically distinguished from zero.

CONCLUSION

This study has shown that the distinct electoral groups clearly differ enough to warrant closer examination than they are generally given. By specifying analyses that accurately reflect the nature of structural relationships and that test for potential statistical biases, we can better understand individual-level behavior. The idea that the relationship between structure and behavior may be interactive is far from original or startling, but the implications for our models and understanding of voting behavior may be.

While a number of forces are known to be related to voter turnout, the present analyses provide an even finer portrait, such as the relationship between age and each stage of participation (refer to Figure 1). It is also clear that long-term predispositions are more closely tied to registration than to turnout. It was predicted that those with little general attachment to politics in the United States would be less likely to overcome the first hurdle, registration. While one would expect such factors as education, political efficacy (both external and internal), and strength of party identification to play a large role in registration, it is still surprising to see their effects so concentrated on the first stage. Furthermore, the differential effects between stages are not just due to short- and long-term predispositions. Length of residence in one's home has a substantial influence on registration, which is a function of both eligibility and the lesser role given to politics during important life changes, such as moving (Stoker and Jennings 1995). Other factors, such as marital status, further highlight how individual attributes interact with structural factors more broadly than is often conceived. While previous studies accurately found these forces to be related to electoral participation, they did not illuminate the important differences that exist due to the structural environment.

The approach employed here demonstrates that a number of participation variables considered unimportant or inconsistent in recent elections deserve closer examination. Some have argued that gender and racial differences in turnout largely disappear once other factors, such as socioeconomic status, are controlled. The analysis presented here shows that once the structural process of registration is taken into account, these factors are still significantly related to the participatory process in recent elections. When other factors are controlled, it appears that African Americans were more likely to register but less likely to go to the polls during the 1980s, possibly due to the candidate options available.[16] In addition, it was seen that moving affects men and women differently. While the registration barrier is more of a hindrance for women who have recently moved than men, females who overcome the barrier are more likely to vote than their male counterparts. This may be due to resources of finances, civic skills, and time, which have been shown to differ for men and women (Schlozman, Burns, and Verba 1994). In short, it is clear that the traditional unified model of participation has concealed the relationship of race and gender to voter turnout because the direction of the influence is different for each in the two stages.

Another insight concerns the relationship between trust in government and turnout. That relationship is generally thought to exist but often eludes researchers. An interesting finding of this study is that trust in government and satisfaction with both major party candidates may be consequential, but the direction of influence is opposite to what is generally expected. In the 1980s, it appears that discontent had a mobilizing effect on participation among recent movers, consistent with Eulau's (1956) view of a "politics of happiness."[17] This warrants further study in light of the fact that both turnout and trust in government have declined since 1960, which raises the question of whether the relationship between the two may have changed in recent times.

Election-specific forces also were found to have a substantial influence on electoral participation in the United States, underscoring the importance of politics in the fundamental act of political participation. The greater the perceived difference between the candidates in the election, the greater are the benefits—instrumental and/or expressive—of participation. The findings raise interesting questions of whether this relationship is a combination of true mobilization in both stages and possible reciprocal causation between behavior and attitudes for long-time registrants. Whatever the case, the short-term mobilization effect of perceived candidate differences was clearly the primary determinant in whether recent movers who were registered went to the polls.

The distinctions that emerge between the full electorate and recent movers emphasize the utility of the paired analyses. While the primary interest of scholars is generally the full electorate, when studying behavior it is worth considering the processes for people performing the acts currently rather than in the (possibly distant) past. It was through the analyses of recent

[16] The utility of distinguishing between influences at each stage of electoral participation can be seen in other ethnic groups in the United States as well. For instance, while Asian Americans tie with Latinos for the lowest rate of registration (53%), they are more likely than any other group to vote once registered. Thus, 76% of Asian Americans who were registered claimed to have voted in 1994, compared to 73% of whites, 64% of Latinos, and 63% of African Americans (Ong and Nakanishi 1996). Citizenship and language explain part of the registration differences, but other factors influence the second stage of participation (United States Commission on Civil Rights 1992).

[17] Eulau's (1956) discussion of the 1956 election involved a political landscape far different from ours. He viewed the way the game of politics was played then as a "happy people's politics." While this does not seem to reflect the present era, his comparative discussion is still insightful. He stated that "overpoliticization" was not necessarily an unmitigated good in other countries and that greater involvement may reflect that people are politically unhappy. This view is consistent with the finding in the 1980s that the less trustful and more politically dissatisfied were more likely to participate, while those more content were more likely to abstain (see also Hadley 1978, Weisberg and Grofman 1981).

movers, who had to register close to the election in order to participate, that a number of the interesting findings in this study were revealed, such as those dealing with gender, trust in government, and candidate differential.

While this study clarifies our understanding of individual-level behavior and structural requirements, it also raises a number of questions. In particular, what is the likely effect of easing registration requirements through the National Voter Registration Act of 1993? The NVRA (a.k.a. Motor Voter) was credited with registering millions of new voters for the 1996 election, and once fully implemented it will fundamentally alter registration in the United States.[18] This will have implications for the level of turnout and its behavioral causes. Several key determinants of participation—particularly long-term political attitudes and education—play a much stronger role in the act of registration than in going to the polls. To some extent, the influence of these forces will be displaced to the second stage. Disinterested chronic nonparticipants are not likely to flood the polls simply because registration barriers diminish. I suspect that the displacement will be only partial, however. As de Tocqueville's "moment of national crisis" approaches, without the barrier of registration, a number of traditional abstainers are likely to go to the polls. Regardless of the NVRA's ultimate effects, this study has demonstrated that its implications may be more different than anticipated. The determinants of behavior at each stage are fairly distinct, and only by fully studying the operation of institutional forces can we understand how individuals will be influenced by such changes as the NVRA.

APPENDIX A: VARIABLES AND CODING

Demographics

South is a dichotomous variable coded one for individuals living in southern states and zero for all others. *Race* (black) is also a dichotomous variable coded one for African Americans and zero for all others. *Income* is in thousands of constant 1988 dollars. *Education* and *Length of Residence in One's Home* were coded in years. While *Age* was also coded in years, it was centered by subtracting the mean value to reduce collinearity between *Age* and *Age-squared*.

Items Tapping Social Connectedness

Church Attendance is a scale of the frequency of church attendance ranging from zero to one: 0 = never/no religious preference; .25 = a few times a year; .5 = once or twice a month; .75 = almost every week; 1 = every week. *Group Membership* is a dichotomous variable coded one if the respondent is a member of any organizations that represent the groups to which s/he feels closest. *Marital Status* is coded one for respondents who are married and living with their spouse, zero otherwise. *Home Ownership* is a dichotomous variable coded one if the respondent is a homeowner; zero otherwise. *Length of Residence in One's Town* is coded in years.

Political Attitudes: General

External Efficacy is a scale ranging from zero (low) to one (high) combining responses to (level of agreement with) three items: "People like me don't have any say about what the government does"; "I don't think public officials care much what people like me think"; and the response to "Over the years, how much attention do you feel the government pays to what people think when it decides what to do?" Each item was scaled from zero to one (high efficacy), and the average of the items was used for the scale. *Internal Efficacy* is a scale from zero (low) to one (high) reflecting the level of agreement with "Sometimes politics and government seem so complicated that a person like me can't really understand what's going on." *Trust in Government* is a scale ranging from zero (low) to one (high) combining responses to four items: "How much of the time do you think you can trust the government in Washington to do what is right?"; "Do you think that people in government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it?"; "Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?"; and "Do you think that quite a few of the people running the government are crooked, not very many are, or do you think that hardly any of them are crooked?" The scale was created in the same manner as that for external efficacy by averaging the rescaled individual items (zero to one). *Party Differential* is a dichotomous variable coded one if the respondent feels that one party is more likely to do a better job dealing with the problem that the respondent perceives as the most important one facing the country. *Strength of Party Identification* is coded zero (independents and apoliticals), one (independents leaning toward a party), two (weak partisans), or three (strong partisans).

Political Attitudes: Election Specific

Candidate Differential is coded as the absolute value of the difference in values given to the two major party candidates on the 100-point thermometer scale. *Candidate Satisfaction* is a dichotomous variable coded one if both major party candidates are evaluated more than 50 on the thermometer scale, otherwise zero.

Administrative Barriers

The administrative variables were compiled from the *Book of States*. *Closing Date* is coded as the number of days before the general election that states close their books for registration; states with no registration requirements or election-day registration are set at zero. *Purging* is coded as the number of years of nonparticipation before a state removes an individual's name from the registration list. States that do not purge their books are set to a value of ten years, which represents the most liberal value for this period.

[18] The League of Women Voters and Human SERVE credit Motor Voter with a net increase of 9 million registrants in its first 18 months, from January 1995 through June 1996 (Duskin 1997b). In the first two years, an estimated 28 million people used the law's provisions to register or update their records (Duskin 1997a). A number of reasons have been given for the decline in turnout in 1996, despite Motor Voter, such as lower involvement and interest, the campaign itself, decline in community, and the absence of the surge Perot inspired in 1992 (Woodwell 1997). Examining the behavioral change in the wake of the rule reforms will have implications for our general understanding of electoral behavior as well as the level of turnout.

APPENDIX B: SELECTION BIAS MODEL ESTIMATION

The selection bias models were estimated using the routine provided by SST. "The first equation (the selection equation) specifies which values of the dependent variable in the second equation are to be treated as uncensored. The second equation (the outcome equation) is estimated after correcting for selectivity bias" (Dubin and Rivers 1990, 127). The uncorrected independent probit models for the second stage are provided in Table B-1 for comparison. The Dubin and Rivers (1989/1990) routine provides a simplified optimization of the selection bias log-likelihood function:

$$L(\beta_1, \beta_2, \rho) = \Sigma y_{2i} (y_{1i} \log P_i (\beta_1, \beta_2, \rho) + (1 - y_{1i}) \log (Q_i(\beta_{2i}) - P_i (\beta_1, \beta_2, \rho)) + (1 - y_{2i}) \log (1 - Q_i(\beta_2)).$$

In this case, y_{2i} is the selection variable (registration), and y_{1i} is the outcome variable (turnout at the polls). $Q_i(\beta_2)$ is the probability that an observation is not censored (that an individual is registered), and $P_i(\beta_1\ \beta_2, \rho)$ is the probability of an "uncensored success" (an individual is registered and goes to the polls).

While the parameter estimates are robust, this estimation technique is sensitive to model specification and the starting values used to estimate the parameters (i.e., if the starting values are far off, the likelihood function does not converge). The starting values for the estimation procedure were produced by estimating the two stages independently using the relevant samples—the entire electorate for registration, and only registrants for turnout. Readers may be concerned with the robustness of the selection bias models, since the only variables that distinguish between the two stages are a pair of highly aggregated administrative variables. Yet, selection bias models with more constraints (based on preliminary analysis and theoretic expectations) between the two stages were run, and the substantive conclusions about the nature of registration and turnout were quite consistent (these are available from the author on request). For completeness, anonymous reviewers recommended the presentation of the full models with only the administrative constraints. In theory this does not seem problematic, as the closing date of registration will clearly only influence registration, and purging also should have a stronger influence on the first stage. Placing the burden of identification on these aggregate variables does not appear to be a problem for the parameter estimation, as the models converged smoothly with stable parameter estimates (for both the more constrained models and those with only the administrative exclusions), and the substantive results are consistent across both approaches. Given the stability of these parameter estimates, the fuller models are presented here.

While the parameter estimates were quite robust, and one

TABLE B-1. Independent Probit Models and Estimated Effects for Turnout among Registrants, 1980–88

	Full Electorate		Recent Movers	
Variable	Parameter	Effect	Parameter	Effect
Intercept	−.3515 (.2381)	—	−.8883* (.4527)	—
Demographics				
South	−.2864** (.0744)	−.0727	−.3367* (.1353)	−.1103
Age	.0084** (.0027)	.0986[a]	.0213** (.0076)	.2160[a]
Age-squared	−.0002* (.0001)	—	−.0004 (.0003)	—
Race (black)	−.2834** (.1068)	−.0718	−.2938 (.1947)	−.0951
Gender (female)	−.0031 (.0692)	−.0007	.2646* (.1258)	.0689
Education	.0517** (.0149)	.1095	.0868** (.0293)	.2244
Income	.0009 (.0019)	.0140	−.0019 (.0034)	−.0381
Time in home	.0085* (.0040)	.0561	−.0118 (.0804)	−.0069
Social Connectedness				
Church attendance	.5466** (.0954)	.1184	.5781** (.1802)	.1612
Group membership	.0868 (.0749)	.0181	.2024 (.1464)	.0542
Marital status	.2541** (.0753)	.0482	.2648* (.1336)	.0689
Time in town	−.0034 (.0024)	−.0434	−.0047 (.0047)	−.0515
Home ownership	.0648 (.0856)	.0137	.1273 (.1410)	.0352
Political Attitudes: General				
External efficacy	.2878* (.1232)	.0635	.3009 (.2311)	.0880
Internal efficacy	.1416 (.0776)	.0301	.0960 (.1330)	.0276
Party differential	.0365 (.0689)	.0078	−.1167 (.1260)	−.0356
Strength of party identification	.0840* (.0365)	.0570	−.0029 (.0701)	−.0025
Trust in government	.0119 (.1512)	.0019	−.3342 (.2744)	−.0737
Political Attitudes: Election Specific				
Candidate differential	.0039** (.0015)	.0723	.0090** (.0028)	.2177
Candidate satisfaction	−.1465 (.0864)	−.0347	.0095 (.1644)	.0028
n	2644		712	
LLF initial	−1832.7		−493.5	
LLF final	−920.1		−286.1	

Note: The dependent variable in these models is validated vote. These are probit models of voter turnout for the censored sample of respondents who were validated as registered. The first column for each analysis has the probit coefficient estimates, the second their standard errors (in parentheses), and the third the estimated effects, which represent the change in probability of participating due to varying the value of each variable while holding the others constant.

[a]Represents the joint effect of age from shifting values for both age and age-squared at the same time.

*$p < .05$, **$p < .01$. Standard errors in parentheses.

can be confident in the substantive conclusions drawn, the estimates for the standard errors were less stable in the selection bias models. I employed bootstrapping as an alternative to empirical estimation of standard errors in the selection bias models. Bootstrapping is based on the analogy that the observed data assume the role of an underlying population for repeated sampling (Stine 1989/1990). While bootstrapping is more computationally intensive, it has the advantage of allowing researchers to estimate sampling distributions directly without stringent assumptions often known to be implausible (Bartels and Brady 1993). Given the large number of variables in these fully specified models, few constraints between the two stages, and the relatively smaller size of the groups in the models for recent movers, about 20% of these bootstrapped samples did not converge smoothly, although the results are consistent with models with more constraints that did not have this problem. The standard errors for the selection bias models for the full electorate and recent movers are based on results from about 100 bootstrapped samples.

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Strategy and Background in Congressional Campaigns

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Common conceptions of the electoral connection often make two assumptions about the behavior of candidates and voters. The first is that candidates focus their campaigns on their records. The second is that voters evaluate candidates on the basis of their campaign messages. This article explores how candidates' backgrounds influence these two components of representation. The main premise is simple: Campaign messages are more effective if they emphasize issues on which candidates have built a record that appears favorable to voters. Consequently, candidates tend to focus on this type of issue when choosing campaign themes. Candidates are less successful in winning favorable evaluations if they stray from their records and make unsubstantiated claims.

Elections are central to American politics. Yet, despite extensive research into campaigns and elections, three central questions remain unresolved. First, do election campaigns follow any generalizable rules? Campaign practitioners often portray their strategy as more art than science. As one consultant put it, "in politics there are no rules" (Murphy 1995). Scholars have been more willing to generalize about campaign strategy and message selection. They posit that candidates manipulate campaign agendas by emphasizing issue positions which voters evaluate favorably (Ansolabehere and Iyengar 1994, Downs 1958, Page 1978, Riker 1986) and that ambiguous messages help a candidate avoid voter dislike of specific policy positions (Shepsle 1972). Scholars, however, rarely test these competing theories with actual themes from campaigns.

A second question asks whether campaigns make a difference in the choices of individual voters. Although millions of dollars are spent in each election, political scientists have not yet discovered a systematic link between candidates' messages and voters' evaluations. Campaign effects are difficult to separate from voters' preexisting inclination to favor candidates from a particular party (Bartels 1992). More precise ways of measuring campaign messages and candidate evaluations offer hope for establishing campaign effects among individual voters (Alvarez 1996, Franklin 1991, Petrocik 1996, Westlye 1991, Zaller 1992).

If campaigns influence voters' choices, then a third question arises: Does this influence help voters make more informed choices? Critics charge that political advertisements distort candidates' positions, thereby deceiving voters. In response, political consultants point to the extensive information about candidates and their issue positions that campaigns provide. Significantly, the more favorable interpretation of campaign effects contains two important assumptions: that candidates' messages accurately reflect their issue positions, and that voters consider these messages when choosing between candidates.

This article examines these questions in detail. I present evidence that campaign strategy does follow clearly identifiable rules and that campaigns affect individual voters' perceptions of candidates. Furthermore, this influence appears favorable: Candidates' messages reflect their record of issue positions, and voters seem to incorporate these messages into their evaluations of the candidates.

The arguments and evidence evolve from a simple premise: When choosing campaign themes, candidates tend to emphasize issues on which they have built a record that appears favorable to voters. This kind of message more effectively wins favorable evaluations from voters than messages lacking such a record. While this argument may appear self-evident, scholars have not fully explored the usefulness of candidate records in explaining political campaigns. The first section of the article explains how message selection and voters' evaluations may be influenced by two components of candidates' records: (1) the *extent* of a candidate's background on an issue and (2) whether that background suggests a *common interest* with voters. The next section develops measures of these two components, using the 1988 Senate Election Study (Miller et al. 1993). The third section uses these measures to predict the positive and negative campaign messages of the 1988 Senate candidates. The final analytical section compares these messages to voters' evaluations of the candidates.

CANDIDATE RECORDS IN CAMPAIGNS

The interaction of voters and candidates resembles the principal-agent relationship developed by economists (Ferejohn 1990). As principals, voters choose a candidate and delegate to this agent the responsibility for representing their interests in government. The principal-agent relationship is plagued by hidden information and opportunism. When deciding whether to hire an agent, the principal cannot observe the agent's skills and preferences until after the hiring decision has been made. In addition, the agent has an incentive to overstate her qualifications and misstate her prefer-

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For research assistance, I thank Patrick Brandt, Scott McClurg, and particularly David Holian. For useful suggestions, I thank John Aldrich, Bill Bianco, John Brehm, David Canon, Ted Carmines, Kathryn Firmin-Sellers, Paul Gronke, Bob Huckfeldt, Tom Kazee, Authur Lupia, Burt Monroe, Cliff Morgan, Emerson Niou, Leroy Rieselbach, John Williams, Rick Wilson, Jerry Wright, and three anonymous reviewers. All results were calculated using Stata 5.0.

ences in order to increase the likelihood of winning the position (Kiewiet and McCubbins 1991).

The principal can overcome these obstacles by focusing on properties of the agent that reliably indicate the agent's qualifications or preferences (Spence 1974). Some of these properties may be outside the agent's control, such as age or gender. The agent may expend time and resources to acquire other properties, such as education or experience. These properties may indicate expertise in a specific area. These properties may also signal an agent's preferences, in terms of commitment to a particular policy position. By deriving expectations of expertise or policy commitment from these properties, a principal can better predict whether a potential agent will produce desired results if hired.

Voters and candidates face similar obstacles and solutions in campaigns. As principals, voters may find it difficult to determine the skills and preferences of competing candidates. These potential agents have an incentive to exaggerate their attractiveness to voters. Aware of this incentive, voters may discount candidate statements as "political" and therefore inaccurate (Hinich and Munger 1992). When attempting to overcome these obstacles, voters and candidates may turn to the solution outlined above: They may focus on properties of the agent that reliably indicate qualifications or preferences (Iyengar et al. 1997). In campaigns, candidates often turn to their record to bolster their message. After devoting time and resources to developing a record on a particular issue, a candidate can use that record to convince voters that she is qualified and shares their interests.

Many scholars believe that incumbent legislators use their records to build a representational link with constituents. To support this belief, many models compare constituent opinion to incumbents' records, either on a specific issue or across a diverse set of roll call votes summarizing the legislator's "ideology." Correspondence between the two measures is interpreted as evidence of representation; that is, the principals (the constituents) have successfully selected an agent (the legislator). Such a linkage theoretically produces favorable evaluations for the legislator. This analytical framework is inherently limited, however (Bianco 1994, Jackson and Kingdon 1992). If the candidate's general ideology (as indicated by roll call votes or some other measure) is attractive to voters, then the candidate may emphasize it in campaign messages. But if the ideology is unattractive, then the candidate may instead highlight her record on more specific issues that generate more favorable evaluations. In the latter case, a comparison of the candidate and the voters may find little or no link between the two. A linkage may exist, however, on the issues that the candidate emphasized to voters.

The problem is magnified by the varying extent of candidates' records on different issues (Fenno 1996). One legislator may have developed a prominent ideological record as leader of the Conservative Opportunity Society. Another may have cast equally conservative roll call votes but may have focused her time and energy on building a record of increasing defense spending. When communicating with constituents, each legislator may emphasize the issue on which she has developed an extensive record. When every candidate's record on a single issue is compared to voters' preferences and evaluations, the evidence of a linkage may be weak. Instead, voters' evaluations of each legislator may hinge on different issues.

These examples point to two components of a candidate's record on a particular issue: common interest and extent of background. The former captures whether the candidate's record suggests an issue position close to that of the voter. But common interest presumes that the candidate actually has developed a record on the issue. If not, then the candidate's claims on the issue may not attract a voter's support. A claim supported by background information may be more effective.

This increased effectiveness may occur for at least two reasons. First, the candidate's record may indicate that she has developed extensive expertise on a particular issue. If the record also suggests that the candidate and a voter share a common interest on the issue, then the voter may trust the candidate and be swayed by the candidate on that issue (Lupia and McCubbins 1995). Imagine two congressional districts heavily dependent upon farming. One is represented by a freshman legislator, while the other has repeatedly reelected a senior member of the House Agriculture Committee. Both legislators have cast votes in favor of farming interests. But the second incumbent's lengthy record of committee activity suggests that she has developed extensive expertise on agricultural issues. As a result, this incumbent's campaign messages involving farming and her record on the issue may more effectively win favorable evaluations from voters. Constituents are more likely to support an expert who shares their position than a candidate who simply states the same position.

Second, background-supported messages may be more effective because a candidate's record may indicate a history of commitment to a particular issue and position. If that record of commitment also suggests a common interest with voters, the candidate may effectively influence voters' evaluations by emphasizing the issue and record. In the 1988 New Hampshire presidential primary, George Bush successfully rebuffed a challenge from Robert Dole. A central part of Bush's strategy was to emphasize his record of opposing tax increases. Both candidates claimed similar positions on the issue. But, Bush claimed that his record indicated greater commitment to the position, and that he therefore would be less likely to raise taxes if elected president (Aldrich and Alvarez 1994).

In sum, the common interest and extent of a candidate's record on an issue may influence the effectiveness of her claims on that issue. This argument suggests several predictions about candidates and voters. If both components are present in a candidate's record on an issue, then the candidate is likely to offer a positive message (about herself) on the issue. If a candidate's opponent possesses a record on the issue that indicates a lack of common interest with voters, then the candi-

date is likely to attack the opponent on the issue. On the level of individual voters, a candidate's positive messages are more likely to influence the voters' evaluations if those messages are supported by background information indicating common interest with the voter. A candidate's attacks on an opponent also will be more effective if they emphasize issues on which the opponent's record suggests a lack of common interest with voters.

These predictions identify particular campaign messages that are likely to be selected. Yet, other factors may mediate the importance of background, leading candidates to deviate from these predictions. One such factor is the competitiveness of a candidate (Skaperdas and Grofman 1995). On the one hand, candidates who are favored in an election may be more likely to offer positive messages about themselves and less likely to attack an opponent. They work to maximize their own support and avoid giving a weaker opponent any publicity. On the other hand, candidates considered likely losers resort to extensive attacks on their opponent in an attempt to weaken her electoral base. These considerations may lead candidates to ignore the logic of message selection based on their backgrounds.

Candidates' choices of message may also hinge on issue ownership (Ansolabehere and Iyengar 1994, Petrocik 1996). According to this explanation, the two major parties "own" certain issues. They have a lengthy record of accomplishment in these areas, and their core constituencies are strongly concerned about them. As a result, candidates' messages on their party's issues will more successfully influence voters' evaluations. This advantage leads candidates to campaign on issues owned by their party.

These two alternative theories also generate unique predictions about candidates' selection of campaign messages. Before comparing these predictions to those based on the candidates' records, I must first develop measures of those records. The next section develops measures of extent of background and common interest for Senate candidates in 1988.

MEASURING CANDIDATES' RECORDS

Candidates build records across an array of issues. It is difficult to define the set of possible issues underlying those records. Defense spending, for example, may overlap with foreign policy. Either category may cover attempts to fund the Strategic Defense Initiative. To overcome this difficulty, I used two sources to define the set of possible issues for this analysis. First, the National Election Study regularly compiles a list of issues mentioned by respondents as frequently discussed in the recent election campaign. Second, the contextual data set in the 1988 Senate Election Study (Miller et al. 1993) lists the issues emphasized by the Senate candidates that year. These two sources provided a comprehensive list, which I combined into 27 possible issues that may be discussed in a congressional campaign.[1]

My research assistant David Holian and I coded each candidate's record on each issue. For evidence on candidate's backgrounds, we turned to several sources: *The Almanac of American Politics* (the editions before and after the 1988 election); *Politics in America* (before and after the election); and the summaries of each campaign in *Congressional Quarterly Weekly Report*, published in February and October of each election year. Using information from these sources, we first coded the extent to which each candidate had developed a public record on a particular issue. Some candidates have built extensive records through committee work, the introduction of legislation, or a prior occupation. Examples include Daniel Patrick Moynihan's (D-NY) lengthy legislative record on welfare and poverty and Joseph Lieberman's (D-CT) efforts to fight crime while attorney general of Connecticut. These candidates were assigned a 2 for their record on the relevant issue.

Other candidates have taken prominent action on a particular issue but only by casting a roll call vote. Pete Wilson (R-CA), for example, was wheeled to the floor of the Senate hours after an appendectomy to help win a close budget vote (Barone and Ujifusa 1987, 76). If the election summaries mentioned that a candidate had cast this type of prominent vote, then she was assigned a 1 on the relevant issue.[2] If the summaries mentioned no public activity on an issue, we assigned a 0 to the candidate on that issue.[3]

We also coded the level of common interest for each candidate and issue. This measure captured whether the candidate's prior activity on the issue indicated a position that would be supported by the electorate in the candidate's state. First, we coded whether the prior activity indicated a liberal position, a conservative position, or no position at all on that issue. Second, I determined whether a state's electorate tended to be liberal or conservative, using the measure of state

[1] The issues are: abortion, child care, consumer interests/government regulation, crime/drugs, defense spending, education, environment, farming, federal spending/pork, foreign policy, general moral/social issues, government corruption, government management of the economy, health care, homosexual rights, immigration, individual economic concerns, macroeconomic concerns, negative campaigning, party/ideology, personal scandal, personal strengths, presidential candidates, race, trade, welfare/poverty, and women's rights. The determination of these 27 issues is arbitrary, and others have chosen substantially different classifications (Clausen 1973, Miller 1990, Page and Shapiro 1992). Certain personal characteristics, such as physical attractiveness, also may influence message effectiveness and voters' evaluations. These characteristics are omitted because of the difficulty of measuring them. I ran the models using various combinations of the 27 issues. The results were always equivalent to, and usually stronger than, the results reported below.

[2] In their descriptions of each legislator, *The Almanac of American Politics* and *Politics in America* include the legislator's position on "Key Votes" during the congressional term. We excluded these roll call votes from the coding, since they are provided for every legislator, regardless of each vote's importance for each legislator.

[3] In the message selection model below, it appears important to assign different values to roll call votes and from more resource-intensive activities, such as introducing legislation. The model was reestimated with the two combined and with roll call votes categorized as nonactivities. Both reestimations produced weaker results than those reported here, which use the 0-1-2 coding scheme for the extent of candidates' records.

ideology in the Senate Election Study contextual data set.[4] Third, I compared the state electorate's ideology to a candidate's position on an issue. If the two were both liberal or both conservative, then I assigned a value of 2 to the common interest measure for that candidate and issue. If the two were opposite, then I assigned a −2. Lack of a record meant that it was not possible to determine whether the candidate shared the electorate's interests on the issue. Here I assigned a value of 0. In a limited number of cases, a portion of the candidate's activities suggested a liberal position, while other activities suggested a conservative position. I also assigned a value of 0 to these cases.[5]

The measure of common interest is admittedly problematic. It is difficult to categorize a state's electorate as simply liberal or conservative, given the variation among voters in any state. In addition, all 27 issues do not fit easily on a liberal-conservative continuum. A particular position may be viewed as liberal in some states and conservative in others. I attempted to obtain better indicators of each state electorate's opinions on the 27 issues. Research assistants contacted polling organizations in the 33 states that held Senate elections in 1988. After 144 telephone calls to 54 individuals and organizations, they accumulated the summary frequencies for 176 statewide polls taken in 1987 or 1988. (Data for individual respondents were available for only a fraction of the polls.) Despite the comprehensiveness of the search, the polls still addressed only a fraction of the possible issues across the states. In light of the limited statewide survey data, I used the summaries of state ideology described above. These summaries are admittedly noisy proxies for state electorates' opinions. But, their imprecise nature makes it more difficult to find a significant relationship between the background measures and message selection.[6]

The coding process produced measures of extent of background and common interest on the 27 issues.[7]

TABLE 1. Mean Background Measures, by Candidate Type

Type of Candidate	Extent of Background	Common Interest	Number of Cases
Incumbent	.81	.22	729
Incumbent challenger	.22	−.01	729
Candidate for open seat	.44	.06	324

Note: The measure of extent of background ranges from 0 (candidate lacks record on the issue) to 2 (candidate possesses extensive record on the issue). The common interest measure has a minimum of −2 (candidate's record on the issue suggests a lack of common interest with the state's electorate) and a maximum of 2 (candidate's record indicates common interest with the state electorate).

Sixty-six major party candidates ran in the 1988 Senate races. As a result, each measure of candidate record has 1,782 observations, one for each candidate on each issue. Table 1 presents the averages of the two measures for different types of candidates. In the first column the averages for extent of background are calculated from the candidates' ratings across the 27 possible issues. None of the candidates had developed a record on all the issues; the most prolific incumbent was Ted Kennedy (D-MA), a legislative workhorse with a record on 16 issues. Given these patterns in the data, it is not surprising that the extent of background averages in Table 1 are closer to the minimum of 0 than the maximum of 2. The averages also vary in understandable ways across different types of candidates. Incumbents have built more extensive backgrounds than their opponents (.81 versus .22). The average candidates in races for open seats fall between these two extremes. Such variation reflects the tendency of races for open seats to attract higher quality challengers than incumbent races (Canon 1993, Jacobson 1997).

Similar differences appear in the averages for the common interest measure, reported in the second column of Table 1. Here, a candidate might receive a maximum of 2 and a minimum of −2, denoting, respectively, the existence and absence of common interest with voters on a particular issue. The averages for this measure are even closer to 0, reflecting the candidates' negative values on some issues. Incumbents' records indicate the greatest common interest with their state, while challengers' backgrounds suggest lower levels of common interest (.22 versus −.01). The average for candidates in races for open seats fall between these two values. The differences provide further evidence of the higher quality challengers in these contests relative to incumbent races.

The low quality of challengers to incumbents often

[4] States were coded as liberal if their ideological rating was greater than the average ideological rating for all 50 states. Conservative states had an ideological rating below the overall average. Jerry Wright compiled the measure of state ideology, using CBS/*New York Times* polls from 1976 to 1985 (Miller et al. 1993).

[5] Several of the 27 issues are valence issues, in that a single position is most favorable, regardless of state ideology. For example, all voters prefer candidates who avoid a personal scandal, such as absenteeism. For valence issues, we compared a candidate's prior activity on an issue (if any activity existed) to the favored position. We assigned a 2 if the two were consistent, a −2 if inconsistent, and a 0 if the candidate lacked a record of activity on the issue. Note that the coding for valence issues does not require comparison to state ideology. But for both valence and nonvalence issues, positive (negative) values indicate the presence (absence) of common interest between the candidate's record and the state electorate.

[6] I also examined the message and background variables only for valence issues, for which the most favorable position (and therefore common interest) is easy to determine (see note 5). When applied to these issues, the message and background variables were related in ways similar to the patterns reported below.

[7] Before coding the 1988 Senate candidates, the research assistant and I practiced coding the records of 60 House candidates in 1988. We both then coded the 1988 Senate candidates. For 91.81% of the candidates and issues, we chose the same code for extent of candidate background. Our agreement on candidate positions was 81.88%. The coding discrepancies fell into two categories: oversight of an aspect of the record (e.g., one of us would miss that a candidate had led legislative efforts to overturn the Endangered Species Act) and disagreement about how to code a particular activity (e.g., one of us would code the introduction of Strategic Defense Initiative legislation under defense spending, and the other would place it under foreign policy). We corrected the oversights regarding candidate activities. We resolved disagreements by placing the problematic activity under a particular issue and coding the activity the same if it subsequently appeared in other candidates' records. The models here use the corrected version of the coding.

has been traced to incumbent advantages, such as franking privileges and greater fundraising ability. Table 1 points to an additional advantage that is often ignored. Sitting legislators have more opportunities to develop records on issues that arise in congressional campaigns, which may help them offer more effective messages on those issues. If more extensive records confer such an advantage, then candidates will choose campaign messages with their records in mind. The next section uses the background measures to test this assertion.

THE SELECTION OF CAMPAIGN MESSAGES

When candidates prepare for an upcoming campaign, they select a set of messages to emphasize during the coming months. While short-term tactical considerations may lead to deviations from this strategic plan, a goal of all candidates and their handlers is to "stay on message" (Franklin 1996). I argue that the strategic plan is based on a candidate's record and that of her opponent. The candidate searches for issues on which her record supports claims that will win favorable evaluations from voters. Consequently, if the candidate has a record on an issue that reflects the preferences of the state's electorate, then she is likely to offer a positive message on that issue. Similarly, the candidate also searches for issues on which her opponent's record supports claims that will encourage unfavorable evaluations of the opponent among voters. If the opponent possesses a record that indicates a lack of common interest, then the candidate is likely to attack the opponent on that issue.

I examined these hypotheses using the Senate Election Study list of issues on which the Senate candidates offered positive and negative messages. The dependent variable is the type of message offered by a candidate on an issue: positive, negative, both, or none at all. With 66 candidates and 27 possible issues, the data set has 1,782 observations. The model includes six independent variables capturing the candidates' backgrounds. The first two are summarized in Table 1: the extent of a candidate's record on a particular issue and the common interest suggested by that record. The third variable is the interaction of the extent of background and common interest measures. Simply expressing a favorable position on an issue may be insufficient to make an effective claim on the issue. The most effective messages may require a background that supports the claim of common interest. Finally, three variables capture the background characteristics of the candidate's opponent and are likely to affect the probability of attacking that opponent.

In addition to the variables capturing candidates' backgrounds, I included independent variables for two alternative theories of message selection. Specifically, to operationalize the competitiveness theory (Skaperdas and Grofman 1995), I used the seven-point rating of each campaign in *Congressional Quarterly Weekly Report*, published in early October 1988. It rated whether the Democrat or Republican was more likely to win. I recoded the measure to capture each candidate's competitiveness. The new measure ranges from 0 (little chance of winning) to 6 (very likely to win).[8] It is important to consider the range of the competitiveness variable for different types of candidates. In 1988 all but two incumbents received (recoded) competitiveness ratings of 4 or higher. Likewise, all but two incumbent challengers had ratings of 2 or lower. Since the *Congressional Quarterly Weekly Report* measure has a different range for different types of candidates, any estimates of this variable's effect may capture differences between the types of candidates instead of the effects of competitiveness. To control for this mediating effect, I created two dummy variables, one for incumbents and one for challengers to incumbents. The excluded category, therefore, captures the candidates for open seats, of which there were six in 1988. I then interacted each of the dummy variables with the competitiveness ratings. For incumbents, the interaction term largely ranges from 4 to 6. For most of the challengers, it ranges from 0 to 2. In this manner, the interaction terms place variation in competitiveness on more comparable scales across types of candidates.

I operationalized the issue ownership theory (Ansolabehere and Iyengar 1994, Petrocik 1996) with a series of dummy variables. The first classifies a candidate as Republican (1) or Democratic (−1). Two additional dummy variables designate whether an issue is Democratic and whether it is Republican. The categorization of specific issues follows the coding of Petrocik (1996).[9] These three variables jointly capture whether a particular issue is owned by a candidate's party.

A Test of Message Selection Theories

All three theories of message selection may explain a candidate's decision to offer a positive message on an issue, a negative one, both, or neither. Consequently, I estimate the following model:

$$Prob(Y_{ij} = k) \,/\, Prob(Y_{ij} = m) = \alpha_k + \beta_{1k}\,(Background) + \beta_{2k}\,(Competitiveness) + \beta_{3k}\,(Issue\ ownership) + \varepsilon_{ijk},$$

where i represents a candidate, j represents an issue, k and m represent message choices, and ε_{ijk} is an independently distributed stochastic term. Since the four possible message choices are unordered and noncontinuous categories, I estimate the model using unor-

[8] The Senate Election Study provides other ratings of competitiveness, some of which were compiled more than five months before the election. I reestimated the model below with these alternative measures of competitiveness. The estimated patterns of message selection did not change.

[9] The following issues were assigned to the Democratic Party: child care, education, farming, health care, racial issues, welfare/poverty, and women's rights. Defense spending, government management, and moral/social issues were assigned to the Republican Party. A number of issues, such as abortion, were not easily classified using Petrocik's framework. Several others, such as macroeconomic concerns, were performance issues. Neither of these latter types of issues were assigned to the parties.

TABLE 2. Unordered Logit Model of Candidates' Message Selection

Independent Variable	Pr(positive message)/ Pr(no message)	Pr(negative message)/ Pr(no message)	Pr(positive and negative message)/ Pr(no message)
Constant	−1.92***	−2.15***	−3.46***
	(.32)	(.34)	(.52)
Candidate's background			
Extent	.47***	.18	.45**
	(.10)	(.12)	(.16)
Common interest	.005	−1.09	−.93
	(.64)	(.87)	(1.25)
Extent × common interest	.06	.37	.50
	(.33)	(.44)	(.64)
Opponent's background			
Extent	.09	.47***	.09
	(.10)	(.10)	(.17)
Common interest	.78	1.40*	−2.11*
	(.65)	(.59)	(.90)
Extent × common interest	−.32	−.88**	1.07*
	(.33)	(.30)	(.48)
Competitiveness	−.06	−.04	−.16
	(.09)	(.10)	(.14)
Incumbent	1.05	1.04	2.20**
	(.56)	(.61)	(.82)
Challenger to incumbent	−.62	−.75*	−1.14*
	(.35)	(.37)	(.55)
Competitiveness × incumbent	−.32*	−.42**	−.69**
	(.13)	(.15)	(.21)
Competitiveness × challenger to incumbent	.07	.44**	.38
	(.14)	(.13)	(.20)
Candidate party	−.09	.21	.33
	(.10)	(.12)	(.28)
Issue owned by Democrats	.78**	.06	3.23***
	(.23)	(.33)	(.49)
Issue owned by Republicans	.54	.72*	1.33**
	(.33)	(.33)	(.49)

Note: Entries are maximum likelihood estimates. Standard errors are in parentheses. $N = 1{,}782$. $\chi^2 = 318.35$***. Correctly predicted = 77%. * $p < .05$; ** $p < .01$; *** $p < .001$.

dered logit. The resulting estimation produces three equations, which are reported in Table 2. In each equation, each coefficient represents the effect of a particular variable on the probability of selecting a particular message type, relative to the probability of the baseline type.[10] Overall, the model explains a substantial portion of the variation in message selection. It correctly predicts approximately 77% of the 1,782 choices made by the candidates. The predictive power suggests that message selection does follow generalizable rules, even if campaign consultants do not always consciously articulate them.

Each group of variables makes a significant contribution to the model.[11] While it is difficult to interpret individual coefficients directly, the results in Table 2 do suggest a number of conclusions supporting the main arguments of this article. The first equation compares the probability of a positive message to that of no message. The only significant background variable is the extent of the candidate's background. The likelihood of a positive message appears to hinge on the background of the candidate (and not that of her opponent). The second equation compares the likelihood of a negative message to that of no messages.

[10] In the multinomial logit model, the baseline category is choosing no message. The multinomial model requires that the odds ratio for any two alternatives be independent of the probabilities of choosing the other alternatives (independent of irrelevant alternatives). This condition may be violated if expansion or contraction of the choice set affects the odds ratio of any two alternatives. In the model presented here, the choice set is unlikely to change. When deciding which type of message to offer, a candidate can always choose from positive messages, negative messages, both, or neither. The data appear to support this assumption. The estimates reported below are relatively stable when the alternatives in the choice set are sequentially omitted (Maddala 1983). I reestimated the unordered logit model four times, omitting one of the choices each time. I then compared the new coefficients to those of the original model. The coefficients never varied by more than one standard error.

[11] In χ^2 tests for total contribution of each theory's variables, it is necessary to calculate the difference between −2 times the log likelihood of the constrained model (estimated without the relevant variables), and −2 times the log likelihood of the unconstrained model. The χ^2 for each group of variables is as follows: competitiveness (106.84, with 15 degrees of freedom), issue ownership (113.92, with 6 d.f.), and background (119.32, with 9 d.f.). These results should be interpreted cautiously, since the test is more likely to produce significant joint effects as the sample size increases (Aldrich and Alvarez 1994, Maddala 1983).

TABLE 3. Estimated Probability of Choosing Messages, by Candidate and Issue Characteristics

Candidate or Issue Characteristic	Candidate's Message Choice: No Message	Positive Message	Negative Message	Positive and Negative Messages
Competitiveness				
Incumbent				
Competitive	.72	.16	.09	.03
Safe	.91	.06	.03	.00
Incumbent challenger				
Competitive	.70	.09	.18	.03
Losing	.81	.10	.07	.02
Candidate for open seat				
Competitive	.72	.14	.11	.03
Losing	.68	.16	.12	.04
Party ownership of issues				
Democratic candidate				
Issue not owned by Democratic Party	.76	.12	.10	.02
Issue owned by Democratic Party	.53	.19	.08	.20
Republican candidate				
Issue not owned by Republican Party	.73	.10	.15	.02
Issue owned by Republican Party	.57	.13	.24	.06

Note: The probabilities were calculated using the coefficients in Table 2. Each probability represents the estimated likelihood that a candidate will choose the particular type of message (positive, negative, and so on), given the level of competitiveness or party ownership of the issue. For the probabilities related to competitiveness, all variables other than those involving competitiveness were held at their means. For the competitive incumbent probabilities, I assigned a 1 to the incumbent dummy variable, a 0 to the incumbent challenger variable, and a 3 to the competitiveness variable. The remaining probabilities relating to competitiveness were calculated in a similar manner. For the probabilities related to issue ownership, all variables other than the party ownership variables were held at their means. For probabilities related to a Democratic candidate, I assigned −1 to the candidate party variable. I assigned a 0 to the Democratic ownership variable for the probabilities when the issue was not owned by the Democrats. I assigned a 1 when the issue was owned by the Democrats. In both cases, I assigned a 0 to the Republican ownership variable. I followed a similar procedure for probabilities related to Republican issue ownership.

Here, only the background variables for the candidate's opponent produce coefficients that are individually significant. The background of the candidate's opponent (and not that of the candidate) influences the likelihood of a negative attack. In the third equation, the backgrounds of the candidate and her opponent influence the probability of offering both a positive and negative message, compared to the likelihood of offering no message. These patterns are consistent with the theoretical predictions.

The predictions can be evaluated in greater detail by calculating the estimated effects of the three groups of variables on the candidates' choice of messages. As can be seen from the estimated probabilities in Table 3, competitiveness appears to influence the candidates' selection of messages, but not in the manner expected. According to the theory, positive messages are expected to grow more likely as incumbents move from a competitive race (CQ rating = 3) to a safe one (rating = 6). Instead, the estimated probability of a positive message decreases from .16 to .06. A symmetric result occurs for incumbent challengers. The theory predicts that they grow more likely to offer a negative message as they move from a competitive race (rating = 3) to a losing one (rating = 0). Here, the likelihood of a negative message actually falls from .18 to .07. In uncompetitive races, incumbents and challengers appear to offer few messages of any type. In more competitive races, both candidates make more campaign claims. These patterns do not extend to races for open seats, perhaps because these contests are more uniformly competitive.

Among the other theories, party ownership of issues appears to exert a strong influence on message selection. When issues are not owned by the Democratic Party, candidates from that party have a probability of .76 of offering no message, or a probability of .24 of offering any type of message (positive, negative, or both). The probability of any type of message jumps to .47 on issues owned by the Democrats. When issues are not owned by the Republicans, candidates from the GOP have a probability of .27 of offering any message. This value rises to .43 on issues owned by the Republican Party. Candidates appear to tilt their selection of messages toward issues owned by their party.

In addition to predicting which issues will be emphasized, it is helpful to know whether candidates will offer positive or negative messages on those issues. The theory based on candidates' backgrounds proves useful with both types of predictions. Table 4 presents the estimated probabilities of offering various messages for different types of candidate backgrounds. The probability of a candidate choosing "no message" on a particular issue always is at least .54. This pattern indicates that most candidates offer messages on few of the 27 issues. Indeed, the mean number of positive messages per candidate is 3.79, while that for negative messages is 3.24. Despite these low averages, the variation in message selection is linked to the candidates' backgrounds.

Consider positive messages, in which a candidate offers a message about herself. If a candidate possesses no background on an issue, the probability of offering a positive message on that issue is .10. This probability

TABLE 4. Estimated Probability of Choosing Messages, by Background Characteristics

Extent of Candidate's Background	Candidate's Common Interest	Candidate's Message Choice: No Message	Positive Message	Negative Message	Positive and Negative Messages
Lacks background	No clear position	.74	.10	.13	.03
Possesses background	No clear position	.60	.21	.15	.04
Possesses background	Common interest	.60	.26	.08	.06
Possesses background	Lack of common interest	.55	.15	.26	.04
Extent of Candidate's Opponent's Background	**Candidate's Opponent's Common Interest**	**Candidate's Message Choice: No Message**	**Positive Message**	**Negative Message**	**Positive and Negative Messages**
Lacks background	No clear position	.76	.12	.09	.03
Possesses background	No clear position	.64	.12	.20	.04
Possesses background	Common interest	.69	.17	.11	.03
Possesses background	Lack of common interest	.54	.08	.36	.02

Note: The probabilities were calculated using the coefficients in Table 2. When calculating the probabilities in the upper half of the table, all variables other than the candidate's background variables were held at their means. For the four probabilities in which a candidate lacks background and position, I assigned a value of 0 to the relevant extent of background and common interest variables. For the four probabilities in which the candidate possesses background but lacks a clear position, I assigned 2 and 0 to the extent of background and common interest variables, respectively. For the probabilities in which the candidate's background indicates common interest, I assigned a 2 to both the extent of background and common interest variables. For the probabilities in which the candidate's background indicates a lack of common interest, I assigned a 2 and −2 to the extent of background and common interest variables, respectively. The probabilities in the lower half of the table were calculated in a similar manner, except that I used the candidate's opponent's background variables, rather than those of the candidate.

rises substantially if the candidate possesses a record on the issue. If that record does not indicate a clear position, the probability of a positive message rises to .21. A record indicating common interest raises the likelihood of a positive message to .26. As might be expected, candidates are less likely to offer a positive message if their record indicates a lack of common interest (.15).

Negative messages targeting a candidate's opponent appear even more closely related to background (of the opponent, in this case). If the candidate's opponent lacks a record on an issue, the probability is .09 that the candidate will attack the opponent on that issue. The probability rises if the opponent has developed a record on the issue. An opponent's record indicating no clear position boosts the likelihood of attacking that opponent to .20. A record suggesting a lack of common interest raises the probability of a negative message to .36. Candidates are more likely to attack if the opponent's record indicates a lack of common interest.

Candidates' records thus appear to influence their choice of campaign messages. The tests of all three theories provide insights into candidates' choices. But the explanation based on background and common interest appears more powerful in explaining the variation in candidates' positive and negative messages. The influence of background is stronger for negative than positive messages, even though candidates offer, on average, more positive than negative messages. Attack advertising has grown more pervasive in recent elections (Jacobson 1997), and candidates increasingly debate the validity and accuracy of their attacks on each other. As a result, voters and political observers may view negative messages more dubiously, particularly compared to positive ones. This critical attitude may make candidates hesitate to attack their opponents unless they can validate the charges with information from an opponent's record. At the same time, candidates may choose to offer positive messages about themselves, unsupported by background information. These messages are less likely to attract questions about accuracy or validity.

MESSAGES, BACKGROUND, AND INDIVIDUAL VOTERS

The analysis thus far has focused on the activities of candidates, illustrating how they often emphasize issues on which they (or their opponent) have developed a record. Such a finding provides initial support for the favorable interpretation of campaigns offered in the introduction of this article. But the argument and evidence may still be dismissed as irrelevant unless it can be demonstrated that the background-related messages actually affect electoral outcomes. This section compares the candidate-level measures of campaign messages to individual voters' evaluations of the candidates, as captured in the 1988 Senate Election Study. It is important to determine how the candidates' overall messages influence evaluations. Accordingly, I first construct measures of the extent to which these overall messages are supported by background activities. I then compare these measures to voters' evaluations of the 1988 Senate candidates.

For each candidate, I calculated the mean of each background measure (extent of background and common interest) for the issues on which the candidate had offered a positive message. The result was the overall extent of background and common interest for the candidate's positive messages. I calculated similar measures for issues on which the candidate offered negative messages. For the latter measures, I used the background measures of the candidate's opponent on the issues where the candidate attacked the opponent. Significantly, the common interest measures are no longer calculated using the ideology of the state elec-

TABLE 5. Average Background Characteristics in Different Types of Messages

Type of Message	Extent of Background	Common Interest	Number of Cases
Messages about incumbent			
Incumbent positive messages	.93	.13	1,174
Challenger negative messages	1.05	.19	1,174
Messages about challenger			
Challenger positive messages	.45	.20	863
Incumbent negative messages	.23	.03	863

Note: This table contains the means of the extent of background and common interest measures for different types of messages. To obtain the first entry (.93), I calculated each incumbent's overall extent of background on the issues in her positive messages. Each of the 1,174 respondents was then assigned the value for the overall extent of background for their incumbent's positive messages. The average of these values across the respondents is .93. As in Table 1, these values range from 0 to 2. But while Table 1 included the incumbent's values across all 27 issues, here I only used the values for issues on which the incumbent offers a positive message. The remaining entries in the table were calculated in a similar manner.

torate. Instead, the individual-level nature of survey data allows calculation of common interest between a candidate's messages and an individual respondent.[12]

The overall background measures for the candidates' messages are summarized in Table 5. In the first column, the measure of extent of background again ranges from 0 to 2. At the latter value, a candidate has developed an extensive record on the issues raised in the particular messages. The measure equals 0 if the candidate possesses no record on the issues in the particular messages, or offers no messages of the relevant type (positive or negative). The differences shown in the first column are instructive. The extent of background appears much higher in messages about incumbents (.93, 1.05) than in messages about challengers (.43, .23). This difference undoubtably reflects the lengthier records of incumbents compared to those of many challengers.[13] That is, incumbents often have more background information to support their positive messages. But the lengthier records also give challengers more ammunition for attacking incumbents.

The second column of Table 5 summarizes the common interest in the messages. This measure again ranges from −2 to 2. Higher values indicate that for the issues in the particular messages, the candidate possesses a record which indicates a position close to that of an individual respondent. With lower values, the candidate's record on issues in the messages suggests a position far from that of an individual respondent. Among messages about challengers, the positive messages from the challengers themselves indicated greater common interest (.20) than do the incumbents' negative messages attacking those challengers (.03). The challengers want to encourage voters to perceive them as sharing common interests, whereas the incumbents want to portray their opponents as lacking common interest with voters. The two groups of candidates therefore emphasized different issues in their messages in an attempt to create contrasting pictures of the challengers' common interests. A different pattern occurs with messages about incumbents. Here, the challengers' attacks indicate only slightly greater common interest (.19) than do the incumbents' messages about themselves (.13). This unexpected result may stem from incumbents personally sharing the interests of their constituents and carefully building records to reflect that common interest. As a result, challengers may find it difficult to uncover evidence in the incumbents' records indicating otherwise.

These overall background measures theoretically indicate the overall effectiveness of candidates' messages. Recall that in a principal-agent relationship, properties of a potential agent can signal whether hiring that agent will further the employer's interests. In the context of a campaign, two important properties of a candidate are the extent of her record and whether that record suggests a common interest with voters. Campaign messages that highlight these properties are likely to win a voter's support more effectively. The results in Table 4 suggest that candidates often choose this type of message, considering it an effective way to win votes. In the analysis that follows, I test the accuracy of this belief by examining how the background measures from Table 5 influence voters' evaluations of candidates.

Individual Voters' Evaluations of Candidates

The Senate Election Study asks respondents to place both themselves and Senate candidates on a seven-point ideological scale. I used these placements to create one dependent variable for incumbents and another for challengers. For each candidate, the variable is the absolute value of the distance between a

[12] The Senate Election Study asks respondents to place themselves on a seven-point ideological scale. I used these placements to create a measure of common interest between respondent and candidate, for the candidate's positive messages. I first rescaled the self-placement to range from −2 to 2, with higher values denoting more liberal respondents. This range is identical to that of the possible positions of each candidate on each issue. I then calculated for each issue the absolute value of the difference between the candidate's position and the rescaled respondent self-placement. The resulting value ranges from 0 to 4, with low (high) values denoting high (low) common interest. To make this gauge of common interest more intuitively understandable and more comparable to the analysis in the previous section, I subtracted it from 2. This new value ranges from −2 to 2, with low (high) values indicating low (high) common interest. Finally, I averaged these new values for all issues on which the candidate offered a positive message. This average captures the overall level of common interest in the candidate's positive messages. I calculated a similar common interest measure for each candidate's negative messages. Significantly, these respondent-specific measures of common interest omit valence issues, which by definition have a single extreme point as the most favorable position. On nonvalence issues, each respondent's most favorable position is any point along a continuous ideological scale. So, in the individual-level calculations, the valence and nonvalence issues are based on different types of scales that are hard to combine into one measure.

[13] The analysis below omits races for open seats.

respondent's self-placement and her placement of the particular candidate. The more favorably respondents evaluate a candidate, the more likely they are to place that candidate close to themselves. That is, a smaller value of the dependent variable indicates a more favorable evaluation.[14]

To capture the effectiveness of a candidate's positive messages, the model includes the measures (from Table 5) of the overall extent of background and common interest on the issues in those messages. An interaction term captures the potential interaction between the two measures. I also include three similar variables for the issues used in negative messages attacking the candidate. I expect the message variables to influence voters' evaluations in the following ways. The greater the extent of background and common interest underlying a candidate's positive messages, the more favorable will be the evaluations from voters. In other words, evaluations will be higher if a candidate's background supports her claims of sharing voters' interests on an issue. For negative messages, the greater the extent of the opponent's background, the more effective those messages will be. Candidates attack on issues where their opponents lack common interest with voters. If the common interest suggested by the negative message is lower, voters are less likely to evaluate the opponents favorably. Simply put, attacks on opponents are more effective if they highlight a lack of common interest in the opponents' records.

I control for a number of factors that may mediate the relationship among messages, backgrounds, and evaluations. An important individual-level consideration is the congruency between the respondent's and the candidate's party. Partisanship can serve as a useful cognitive shortcut for voters struggling to evaluate a candidate (Popkin 1991). A voter may be more inclined to place her own party's candidates closer to herself, while placing candidates from the opposite party farther away. The partisan congruence measure equals 1 if the respondent shares the candidate's partisanship, 0 if the respondent is an independent, and −1 if the respondent is of the opposite party. Another control involving individual respondents is the number of days between election day and the survey interview for each respondent. As this period increases, respondents may encounter increasing amounts of information about the election winner (often the incumbent). The loser (often the challenger) may receive decreasing amounts of publicity during this period. The proincumbent bias in information may lead to a similar bias in respondents' evaluations of candidates (Wright 1993).

My analysis also controls for two campaign-level factors. The first is the balance of information flowing to voters. In elections with a strongly favored incumbent, voters may receive little negative information about her, and little information at all about the challenger. Voters in this type of campaign may place the incumbent closer to themselves and the challenger farther away. In competitive campaigns, in contrast, voters may receive more positive information about the challenger and more negative information about both candidates (Westlye 1991). The more balanced information flow may lead voters to place incumbents farther from themselves and challengers closer. I operationalize this control with a folded version of the October rating of each campaign's competitiveness, produced by *Congressional Quarterly Weekly Report*. The new version ranges from 1 (safe) to 4 (toss-up).

A second campaign-level factor, challenger quality, may affect voters' evaluations in similar ways. More qualified challengers often run more competitive campaigns, providing more positive information about themselves and more negative information about the incumbent. As a result, voters in these campaigns may place incumbents farther from themselves and challengers closer. The challenger quality variable ranges from 1 (experience-seeking challenger) to 4 (experienced challenger) (Canon 1993). It is important to note that the measure of quality may capture some of the same challenger characteristics used to produce the background variables. Including the challenger quality variable in the models makes it more difficult to produce evidence that the background or challenger quality variables exert a significant effect on voters' evaluations.[15]

Table 6 contains the OLS estimates for the two evaluation models. Of the control variables, only the partisan congruence measure produces significant coefficients. As one moves from a respondent not from the incumbent's party (−1) to a respondent who shares the incumbent's party (1), the estimated distance between respondent and incumbent falls by .60 (2 × −.30). The equivalent figure for challenger placements is .74. As expected, a candidate receives better evaluations from voters in her own party than from those in

[14] Two alternative measures of evaluations are vote choice and open-ended likes and dislikes. When I replaced the distance measure with these alternatives, the message variables exerted similar but weaker effects on the dependent variables. The weaker results are not surprising. First, vote choice can be influenced by all four sets of message variables (positive and negative messages of incumbents and challengers). Too complex and overlapping for a single vote equation, these effects can be separated by focusing on the individual components of each respondent's vote choice: her evaluation of each candidate. Thus, I present separate models for incumbent and challenger evaluations. Second, the distance measure holds an important advantage over the net sum of likes and dislikes. A respondent may carry an overall evaluation of a candidate, without remembering specific likes and dislikes. The respondent updates this summary evaluation whenever she encounters new details about the candidate. After updating the evaluation, the respondent may drop the details from memory, retaining only the overall evaluation. Under this scenario, the campaign still influences the voter's evaluation, but she does not remember details about the candidate. The likes-dislikes measure labels this voter as having no evaluation, whereas the distance measure captures the change in evaluation resulting from the campaign.

[15] This model resembles that of Franklin (1991). His model predicts respondents' actual ideological placements of politicians, while this model predicts the perceived distance between the respondents' placement of themselves and the politicians. I reestimated Franklin's model incorporating the background variables used here. Only the extent of background variables produced a significant and consistent effect, which was limited to respondents' certainty in making the placements. As the extent of background underlying candidates' messages increases, the variance in respondents' placements diminishes. That is, respondents are more certain in placing a candidate whose messages emphasize issues on which she has built a record.

TABLE 6. Model of Respondent's Perceived Distance from Candidates

Independent Variable	Incumbent	Challenger
Constant	1.49***	1.71***
	(.15)	(.16)
Partisan congruence	−.30***	−.37***
	(.04)	(.05)
Days between election day and respondent's interview	.001	.004
	(.003)	(.003)
Competitiveness of race	−.10	−.08
	(.06)	(.09)
Quality of challenger	.04	−.02
	(.03)	(.04)
Positive messages of candidate		
Extent of background	.04	−.06
	(.06)	(.12)
Common interest	−.13	−.26*
	(.12)	(.12)
Extent × common interest	−.03	−.07
	(.08)	(.15)
Negative messages of candidate's opponent		
Extent of background	−.01	.17
	(.06)	(.10)
Common interest	.48**	−.21
	(.15)	(.35)
Extent × common interest	−.34***	−.04
	(.09)	(.24)
Number of cases	1174	863
Adjusted R^2	.07	.12
SEE	1.29	1.33
F-tests for joint effect of variables		
Overall model	9.43***	12.81***
Three variables for positive messages	3.01*	7.39***
Three variables for negative messages	5.18**	2.90*

*$p < .05$; **$p < .01$; ***$p < .001$. Standard errors are in parentheses.

the opposite party. This pattern is weaker for incumbent evaluations. Legislators may use constituent services and other office resources to win the support of partisans of the other party but challengers lack these means.

Of the variables related to candidate messages, only three of the twelve coefficients are significant. The joint effects of the variables, however, are more strongly significant. As the last two rows of Table 6 indicate, the *F*-statistic for each trio of message variables is jointly significant at least at the .05 level (Kennedy 1996).[16]

[16] The inclusion of interaction terms appears to reduce the significance of the individual message variables that capture positive messages about the incumbent and both types of messages about the challenger. When I reestimated the models without interaction terms, the six variables capturing those three types of messages produced three significant coefficients. But the two variables related to negative messages about the incumbent produced no significant coefficients. The interaction term appears crucial for capturing the influence of these two variables on evaluations of incumbents (as evidenced by their strongly significant coefficients in Table 6). Therefore, I report the model including the interaction terms, despite their negative effect on the individual significance of the other message variables.

The interaction terms make it difficult to interpret the individual coefficients substantively. I therefore used these coefficients to estimate the distances between respondents and candidates for different types of backgrounds and messages. Table 7 presents these predicted values. In the incumbent's positive messages about herself, emphasizing issues with no background or position produces an estimated distance of 1.58. But if the incumbent focuses on issues on which she has developed a record but no clear position, the perceived distance rises slightly, to 1.66. Such an increase runs counter to theoretical expectations. Incumbents may have developed extensive records on many issues in their positive messages, which depresses the variation and subsequent effect of the extent of background variable. The influence of common interest conforms more closely to expectations. If an incumbent's background underlying her positive messages suggests a common interest with a respondent, then the perceived distance drops to 1.30. In other words, evaluations of the incumbent improve if her positive messages highlight evidence of common interest with voters.

Slightly different patterns occur for a challenger's attacks on an incumbent. Comparing messages that lack with those that possess background information about the incumbent, we see that the estimated distance from the incumbent is largely unchanged (1.58 versus 1.56). This lack of change in extent of background does not support theoretical expectations. Comparing the presence or absence of common interest produces more predictable evaluations. If a background-supported attack on the incumbent suggests a lack of common interest with a respondent, the voter's perceived distance increases to 1.98. That is, this type of negative message leads to less favorable evaluations.

Challengers' backgrounds appear to influence the effectiveness of messages even more dramatically than do the incumbents' records. If a challenger's positive messages focus on issues on which she lacks background and position, the estimated distance is 1.75. Consistent with expectations, the distance drops to 1.63 if she emphasizes issues on which she has built a record but no clear position. If she emphasizes issues on which her record indicates common interest with a voter, the perceived distance drops even more, to 0.85. Thus, careful selection of issues for campaign messages can win more favorable reactions from voters.

Incumbents' attacks on challengers also appear to vary in effectiveness. If an incumbent criticizes a challenger on issues where the challenger lacks background and position, the perceived distance between respondent and challenger is estimated to be 1.64. But if the incumbent attacks on issues where the challenger has built a record with no clear position, the estimated distance rises to 1.97. The distance increases to 2.56 if the challenger's record on the issues under attack indicates a lack of common interest. The correct type of negative messages can hurt significantly a challenger's evaluations.

There is a flipside to the selection of issues and messages. Emphasizing the wrong issues can hurt a candidate's own evaluations or improve those of the

TABLE 7. Estimates of Voters' Perceived Distance from Candidates, by Background Characteristics in Different Types of Messages

Incumbent's Background Characteristics in Types of Message about Incumbent		Voter's Perceived Distance from Incumbent	
Extent of Background	Common Interest	Positive Messages about Incumbent	Negative Messages about Incumbent
Lacks background	No clear position	1.58	1.58
Possesses background	No clear position	1.66	1.56
Possesses background	Common interest	1.30	1.15
Possesses background	Lack of common interest	2.02	1.98
Challenger's Background Characteristics in Types of Message about Challenger		**Voter's Perceived Distance from Challenger**	
Extent of Background	Common Interest	Positive Messages about Challenger	Negative Messages about Challenger
Lacks background	No clear position	1.75	1.64
Possesses background	No clear position	1.63	1.97
Possesses background	Common interest	0.85	1.37
Possesses background	Lack of common interest	2.42	2.56

Note: The distances were calculated using the coefficients in Table 6. For the distances related to positive messages about incumbent, I held at their means all variables except the background measures linked to these positive messages. For the estimated distance when those messages emphasized issues with no background or position, I assigned a 0 to both the extent of background and common interest variables for the incumbent's positive messages. I then calculated the perceived distance when the incumbent's positive messages are supported by background but no clear position. Here, I assigned a 2 to the relevant extent of background variable and a 0 to the common interest variable. Next, I calculated the perceived distance when the incumbent's positive messages are supported by background indicating common interest with the voter. I assigned a 2 to both the extent of background and the common interest variables. Finally, I calculated the perceived distance when the incumbent's positive messages are supported by background indicating a lack of common interest with the voter. I assigned a 2 to the extent of background variable and a −2 to the common interest variable. The other distances in this table were calculated in a similar manner.

candidate's opponent. For example, if an incumbent emphasizes issues on which her record suggests a lack of common interest with a respondent, the model estimates the distance at 2.02. This evaluation is worse than the case of no record at all (1.58). Similarly, if a challenger attacks on issues about which the incumbent's record indicates common interest with a respondent, the estimated distance is 1.15. This value is smaller than in the case of attacks on issues where the incumbent lacks a record (1.58). Drawing attention to an opponent's favorable record hurts a candidate's chances more than issuing attacks unsupported by background information.

Overall, these results contain a number of noteworthy findings. When candidates emphasize issues on which their records indicate a common interest with respondents, those voters are likely to perceive the candidates more favorably. Negative messages are more effective if they highlight issues on which the opponent's record suggests a lack of common interest with voters. These patterns provide strong evidence of the influence of candidate background on the effectiveness of campaign messages.

An additional finding merits attention. Despite the alleged superiority of attack ads in influencing voters' evaluations (Jacobson 1997), my analysis of positive and negative messages does not produce a clear pattern of contrasting effects. Neither type of message is more effective for both candidates. Instead, the main variation occurs between messages about incumbents and challengers: Differences in the records underlying messages affect evaluations of challengers more strongly than incumbents. The varying effects may stem from information that voters possess at the start of the campaign. During years of Senate service preceding the campaign, incumbents pass extensive information about themselves to voters. Those precampaign reputations may be more impervious to campaign claims, positive or negative, than challengers' reputations. Voters are less likely to know much about challengers at the campaign's outset, so their reputations may be more susceptible to campaign influences. These contrasting patterns only partially support the argument that challengers' campaign efforts are more effective, dollar for dollar, than those of incumbents (Jacobson 1997). Positive messages boost the evaluations of challengers more than they boost the evaluation of incumbents. But the negative messages of incumbents appear more effective than those of challengers, possibly because challengers' reputations are more malleable.

CONCLUSION

These results directly address the three questions raised at the beginning. Election campaigns do appear to follow generalizable rules: Candidates often campaign on issues for which they have built favorable records. Background-supported messages appear to influence individual voters. Candidates win more favorable evaluations if they focus on their records and avoid making unsubstantiated claims. These findings provide compelling evidence against the view that campaigns distort candidates' positions and deceive voters.

The greater effectiveness of background-supported messages thus appears to limit the extent to which candidates stray from their records when campaigning. This limit varies from one candidate to another, how-

ever. For those who have developed a record on only one or two issues, the most effective strategy is to focus on those few issues. More experienced candidates often have built records covering a wide range of issues. They can selectively choose the issues on which their records support the most attractive claims. Manipulation is also possible on an individual issue for which a candidate has developed a record. Some elements in that record may support a liberal position, while others may support a conservative one. The candidate may emphasize the electorally attractive position and supporting evidence, while her opponent may focus on the unfavorable position and evidence. Voters then find it difficult to decide which claim accurately characterizes the candidate. But if the large majority of information in the record supports a particular position, then it is more difficult for the candidate or her opponent to make an effective claim that the candidate does not hold that position.

Despite these continued opportunities for manipulation, candidate records still help campaigns play a crucial role in representation. Background information does not ensure that candidates will campaign on every aspect of their records. But the greater effectiveness of background-related messages appears to discourage candidates from making unsubstantiated claims when communicating with voters. The constraint of past activities pushes candidates to campaign on their records, and voters appear to incorporate these claims into their evaluations of candidates. Such behavior is crucial for representation to occur.

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The Dynamics of Foreign Policy Agenda Setting

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Theoretical and empirical work on public policy agenda setting has ignored foreign policy. We develop a theory of foreign policy agenda setting and test the implications using time-series vector autoregression and Box-Tiao (1975) impact assessment methods. We theorize an economy of attention to foreign policy issues driven by issue inertia, events external to U.S. domestic institutions, as well as systemic attention to particular issues. We also theorize that the economy of attention is affected by a law of scarcity and the rise and fall of events in competing issue areas. Using measures of presidential and media attention to the Soviet Union, Arab-Israeli conflict, and Bosnian conflict, we show that presidential and media attentions respond to issue inertia and exogenous events in both primary and competing issue areas. Media attention also affects presidential attention, but the president does not affect issue attention by the media.

Various theoretical and empirical analyses have focused on understanding how domestic policy issues reach the systemic or institutional agenda in the United States (e.g., Cobb and Elder 1972; Downs 1972; Jones 1977; Walker 1977; Anderson 1978; Kingdon 1984; Peters and Hogwood 1985; Light 1991; Baumgartner and Jones 1993; Jones 1994; Flemming, Wood, and Bohte 1995). There have also been case studies that explore how single issues develop, also all domestic (e.g., Bosso 1987, Carmines and Stimson 1989, Glick 1992). As of yet, however, there has been no systematic analysis exploring the dynamics of U.S. foreign policy agenda setting.

This neglect of foreign policy by students of American politics is not a matter of chance but is due to practical reasons. Foreign policy does not readily fit the theoretical mold most scholars associate with domestic issues. Theories and analyses of domestic policy agenda setting usually focus on concepts like problem perception, issue definition, mobilization of interests, subsystem formation, venue shopping, and institutional attention.[1] The first two and last of these concepts are certainly relevant to foreign policy agenda setting. It is less certain that interest groups, subsystems, and venue shopping are of much importance. The conventional wisdom is that for many foreign policy issues subsystems do not develop, since these issues do not typically involve the material or solidary benefits required to invoke strong interest group participation (e.g., Ripley and Franklin 1991).[2] Congress and its committees are generally considered to have less influence in foreign policy than in domestic policy. Moreover, the president is the primary venue through which new foreign policy issues enter the U.S. system.[3]

There can be little doubt that the presidency has evolved through time as the primary actor responsible for U.S. foreign policy. The Constitution limited presidential authority in this area with respect to war, making treaties, and appointing official representatives to foreign nations. Short of war, however, the president has traditionally received considerable latitude. The president is the only actor in the U.S. system who can legitimately speak for the nation as a whole or be spoken to by other nations (usually through ambassadors). The increasing involvement of the United States in the international community, the public's desire for strong personal leadership, and the political ramifications of foreign policy mean that the presidency attends more heavily to foreign policy issues than does any other U.S. institution.

Theories and analyses of domestic policy agenda setting also tend to describe the process as proceeding in a slow secular fashion, occasionally spurred by focusing events or the policy entrepreneurship of important actors or groups (Anderson 1978, Cobb and Elder 1972, Kingdon 1984). Yet, foreign policy issues do not often arise in such a gradual fashion. Rather, they tend to burst onto the scene as a result of crises or

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The authors appreciate comments on earlier versions of this manuscript by George C. Edwards III, Kenneth J. Meier, Steven A. Shull, Nehemia Geva, and four anonymous reviewers. This research was supported financially by the Center for Presidential Studies, Texas A&M University.

[1] Venue shopping means that because of the U.S. system of fragmented representation, potential agenda setters have multiple institutions through which to pursue policy goals.

[2] We do not mean to imply that interest groups and congressional committees never become involved in foreign policy or have no influence, but their involvement is typically less than for domestic issues. The degree of involvement obviously depends on the type of issue. Trade policy, for example, would provide the cement for subsystem relations to develop. Congress may also become involved in human rights or strategic arms control. Ripley and Franklin (1991; see also Ripley and Lindsay 1993, chapter 2; Lindsay 1994, chapter 7) provide a framework for considering different types of foreign and defense issues.

[3] The view stated here has long been the dominant one among presidential scholars. For example, historian Arthur Schlesinger states: "In foreign policy the inclination is to let the Presidency have the responsibility—and the power" (1989, 420; see also Halperin, quoted in Doherty 1993, 323). Likewise, Barbara Sinclair (1993; see also Hinckley 1994) argues that while congressional leaders have been more active in foreign and defense policy since the Vietnam War, their influence is limited to the extent they are acting as independent policy entrepreneurs, rather than as agents of an active and involved membership. In contrast, see Lindsay (1994) for an extended discussion asserting congressional influence on U.S. foreign policy making.

other dramatic occurrences, and once they are on the scene attention is driven by a continuous stream of policy events. While events are occasionally important in driving attention to U.S. domestic issues (e.g., a Martin Luther King speech and civil rights, a thalidomide controversy and drug safety, an attempted presidential assassination and gun control), they are of much greater importance in driving attention to issues in foreign policy.

THE ECONOMY OF FOREIGN POLICY ATTENTION

We explore the determinants of attention to major foreign policy issues in the U.S. system. As noted above, past work on domestic policy agenda setting provides a weak theoretical basis for exploring the dynamics of foreign policy attention. Pluralism and interest groups are generally considered of lesser importance, the process is seen as event driven, and foreign policy making is considered the primary domain of the president. A theory of foreign policy agenda setting and attention, therefore, should necessarily focus on the president. Yet, the president's ability to attend to particular foreign policy issues is often constrained by other responsibilities. The president is governed by an economy of attention in which competition exists for scarce resources.

Of course, the proliferation of demands on the modern presidency has produced new institutions within the White House that are intended to alleviate limitations due to scarce presidential resources. Yet, modern presidents and their staffs remain only boundedly rational (Simon 1947). Cognitive limitations mean that they neither can accurately forecast the rise of new foreign policy issues nor possibly attend to the plethora of potential issues that ultimately arise. As a result, presidents purposely restrict the set of issues at any one time to a few. They tend to process these few issues in a serial rather than parallel fashion, attending to one before moving on to the next (Jones 1994). In order to prioritize issues, presidents rely on cues from their environment regarding the relative severity of the problem, as well as political benefits and costs. As political creatures, presidents are ever aware of the risks associated with ignoring or attending to new policy problems.

To be sure, this economy of attention is altered when presidents make conscious efforts to give higher priority to some issues rather than others. The president brings to office an agenda and, if possible, will attempt to implement it (Light 1991). For example, President Nixon clearly gave a high priority to relations with China, which necessarily restricted the amount of time he could spend on other issues. This personal dimension the president brings to the office is inherent to the motivations and goals of the individual. Yet, even though most presidents have foreign policy goals, they often have agendas thrust on them by the office and external events.

Indeed, multiple forces move attention to foreign policy issues away from presidential discretion. For one thing, presidents often inherit problems that dominate their attention through time. Chief executives from Truman through Bush inherited the foreign policy problems associated with the Cold War and devoted considerable time to these issues. Similarly, President Nixon inherited Vietnam from the Johnson administration and necessarily devoted more attention to this issue than to any other foreign policy domain. On issues of this sort, presidents must fulfill continuing obligations to the office and nation, regardless of personal agendas.

More generally, once issues gain prominence in the economy of presidential attention, attention becomes highly inertial. As a technical matter, the most important foreign policy issues are very persistent and require time and effort to resolve. To be successful, presidents must maintain focus on the difficult issues until they wane or at least are displaced in importance by other concerns. The security of the free world, public approval, and the president's historical legacy all depend on dealing effectively with important problems, so there is strong incentive for the president to maintain focus to the potential exclusion of other issues.

This highly inertial character of some foreign policy issues suggests a rationale for the economy of attention to exhibit stability. Presidents tend to concentrate on the most difficult foreign policy problems. The proportion of presidential time devoted to these problems remains fairly stable so long as they require strong consideration and no more pressing problem arises. The duration of attention to foreign policy problems is proportional to the tractability of the problem. As long as the problem remains unresolved, presidential attention remains fairly consistent.

Nevertheless, short-term disturbances to this stability may occur due to policy events or changes in perception of the relative importance of the issue. Because the president must ration attention according to which issues are most critical, shifts also may occur as new problems arise. These shifts may be either short or long term. In either case, the chief executive often operates in a responsive mode, reacting to a continuous stream of exogenous events. There can be little doubt, for example, that President Truman preferred not to be dealing with the Soviet blockade of West Berlin in 1948, but this event skewed foreign policy attention even more toward U.S.–Soviet relations through early 1949. Likewise, President Carter preferred not to be attending to the Iran hostage crisis during the 1980 election year, but he had little choice, since it also dominated media and public attention that were critical to the election. Not all foreign policy events are this dramatic, but still there is a continuous progression of world events to which presidents must always attend.

The decision as to what constitutes an issue deserving of executive attention may initially arise through developing events, but political factors affect the perceived importance and persistence of foreign policy issues. The president cares about public perceptions. The president has always depended on public ap-

proval as a source of legitimacy and strength in dealing with other actors, both foreign and domestic (Neustadt 1960). While approval of the way a president is doing the foreign policy job does not fully determine approval, it does at times comprise an important component, especially when foreign policy is salient (Edwards, Mitchell, and Welch 1995). To maintain good standing, the president must attend in some measure to what the public perceives as important. Thus, as the public pays greater attention to an issue, the president must in turn attend more to that issue.

Of course, public perceptions of the relative importance of an issue are largely determined by the news media. The media interpret issues, giving them more or less significance through the type or amount of coverage provided. The media, particularly television news, also focus attention on events through this same interpretive function. Americans generally do not pay attention to foreign policy issues until events covered by the news media cause them to do so, and then their attention is slight and deteriorates quickly (Wittkopf 1990). Iyengar and Kinder (1987) show that what the television news covers influences what citizens think is important and how they evaluate their leaders. Likewise, Bosso (1989) argues that the media played an important role in mobilizing the citizenry toward famine relief in Ethiopia during the 1980s. Television news chose to focus on the famine, making an endemic problem in Africa a new foreign policy problem in the United States. Thus, media attention to foreign policy issues and events may drive systemic attention, as well as attention by the president.

The direction of this relationship may not run purely from the media or system to the president, however, Presidents are not purely responsive actors bending with the breezes of popular attention. Rather, they also seek to lead on matters of policy. The president often wants to achieve a personal agenda, both domestic and foreign, but especially in the area of foreign policy the president is expected to be the locus of policy influence. Manipulation of the press is critical to the White House operation (Cohen 1963), and all presidents since Franklin Roosevelt have maintained a staff dedicated to informing the media on matters of interest to the president. In efforts to lead, the president attempts to mold how the media treats emerging issues and events. For example, Brace and Hinckley (1992) found that presidential trips, summits, speeches, and other initiatives create rallies in public approval while attracting large amounts of media attention. More generally, each year the president is given the stage to set the policy agenda for the American people and Congress through the State of the Union address. Cohen (1995) showed that public opinion follows the president's lead on which problems are most pressing, as outlined in the State of the Union address. Thus, the media are a principal means whereby presidents attempt to shape the foreign policy agenda and lead systemic attention toward those issues the president deems as important.

EVALUATING THE ECONOMY OF ATTENTION TO FOREIGN POLICY

To what extent is presidential attention to foreign policy issues a result of issue inertia or a continual progression of stochastic events? To what extent is systemic attention to foreign policy issues a result of issue inertia or stochastic events? Does the president respond to changing systemic attention to foreign policy issues? Does the system respond to changing presidential attention to foreign policy issues? These are important questions for understanding the president's role as foreign policy leader, as well as for extending political science theories of agenda setting to the foreign policy domain.

We address these questions by developing measures of systemic and presidential attention for three foreign policy issues and related events. In particular, we look at issues involving the Soviet Union, the Arab-Israeli conflict, and the more recent Bosnian conflict. We chose these three because they have been the most highly visible and enduring foreign policy issues for the president over the last 15 years. Foreign policy issues of lower visibility and endurance (such as the South China sea, chemical and biological weapons) are also part of the president's agenda and may be of considerable importance. As a result, we suggest that future research examine a diverse range of issues with differing visibility and interest to the president.

We used weekly time-series data to capture the fine time dynamic associated with responsiveness by both the media and president to world events and also to enable greater confidence in the findings due to the larger sample size. We measured systemic attention for issues involving the Soviet Union and Arab-Israeli conflict as the weekly broadcast time devoted by television news from the 27th week of 1984 through the 23d week of 1994. The Bosnian conflict, which has been less protracted and is confined to the 1990s, is measured as the weekly broadcast time from the 25th week of 1991 through the 11th week of 1995.[4] Rather than counting the number of stories, as has been done in some past research (e.g., Baumgartner and Jones 1993; Flemming, Wood, and Bohte 1995), we counted the number of minutes devoted to each issue on the three nightly television news programs. Specifically, we searched the *Vanderbilt Television News Abstracts* using keywords (listed in the Appendix) to capture stories concerning the Soviet Union and Arab-Israeli and Bosnian conflicts.[5] We examined each hit of the key-

[4] For post–Soviet Union observations after late 1991, we examined relations between the United States and the states of the former Soviet Union. The time frame studied was driven entirely by data availability. The beginning and ending dates were constrained by the PANDA events data (to be discussed below), which are continuously being updated and extended toward the present by the project directors. At the time we constructed the Soviet and Arab-Israeli measures, the PANDA data were available only through mid-1994; at the time we constructed the Bosnia measure, the PANDA data were available only up to early 1995.

[5] The *Vanderbilt Abstracts* are available on the web at the following address: http://www.TVnews.Vanderbilt.edu. The information logged here comes in the same format as the hard copy versions

words for validity concerning whether it dealt with some facet of the foreign policy issue or whether some isolated event affected the measure. Although the nightly news programs are not the only television news sources that may influence or be influenced by the president, they do provide a consistent sample of coverage and are also continuous for the period under study. Each network spends about 22 minutes each day delivering the news in half-hour programs (Tyndall 1995). This leads to a typical weekly news coverage of roughly 150 minutes, or 450 minutes total for the three networks.

Measuring presidential attention to these three issues is more difficult. The president's public face may not be a true reflection of what is actually being attended to behind the scenes. Unable to know beyond question what the president is doing, we must assume that what he does and says publicly from week to week reflects what is on his agenda.[6] Cohen (1995) made this assumption and measured presidential attention to domestic policy issues by looking at State of the Union addresses. These speeches are only made annually, however, and may give an incomplete image of attention as it changes due to events and shifting circumstances during the year. It is also unclear that the relative issue emphases in the State of the Union address provide an accurate picture of presidential priorities on foreign policy. Presidents make their priorities known through a number of outlets, including the State of the Union address, legislative proposals, news releases, position taking, briefings, speeches, press conferences, and letters.

To capture presidential attention registered through all these diverse outlets, we used *Public Papers of the President*, an annual compilation of activities during each year. As with the TV news search, we used keywords (listed in the Appendix) in the subject index in each volume and searched the text for relevant activities. Specifically, we counted the number of paragraphs during each week of each year of the *Public Papers* devoted to some facet of Soviet relations, the Arab-Israeli conflict, and the Bosnian conflict. We also read each entry to assure that it was pertinent to the keyword entry and the concept under investigation.

We measured international events using the PANDA events data set provided by the Program on Nonviolent Sanctions and Cultural Survival from Harvard University (Bond and Bond 1995). PANDA uses an artificial intelligence program (KEDS) to code discrete events from Reuters news leads.[7] "An event is an interaction, associated with a specific point in time, that can be described in a natural language sentence that has as its subject and object an element of a set of actors and as its verb an element of a set of actions, the contents of which are transitive verbs" (Bond and Bond 1995, 5). In other words, PANDA events are reports of discrete actions by a source country toward a target country or discrete actions completely within a single country, such as a civil war or rights violation. Using keywords (listed in the Appendix) relating to the three issues, relevant events were separated from the entire data set. Of course, we could have coded a dummy variable for the presence or absence of international events based on the PANDA data set. Using a weekly count of events, however, enabled the measure to reflect the relative seriousness of developing situations, as well as their presence. The events were then counted by week to create a consistent measure of exogenous events to match the other two series. Both the TV news networks and the president use Reuters to monitor world events, so it is almost prima facie evident that there should be some effect from this measure on media and presidential attention to these issues. The question is whether, independent of world events, effects exist from the national media to the president or from the president to the media.[8]

With measures in hand for systemic and presidential attention to foreign policy issues, as well as exogenous events, we used vector autoregression (VAR) methods (Freeman, Williams, and Lin 1989; Simms 1980) to evaluate the causal direction of attention and also to provide evidence of the temporal dynamics associated with the policymaking system. VAR is the most appropriate method for circumstances in which theory provides a weak rationale for imposing restrictions on the parameters of a structural equation system. As our theoretical discussion above indicates, we have no a priori reason for imposing parameter restrictions in either direction; yet, without them a structural equation system is not identified. Thus, structural equations are inappropriate. This does not mean, however, that the approach we use is devoid of theory. Indeed, all aspects of the reported VAR model have some theoretical rationale. We merely ask the data to tell us

available in most university libraries and has the advantage of allowing electronic searches. We used *NETSCAPE* as a search engine, which virtually eliminated the problem of double counts that may plague the use of electronic searching in other contexts. The cache file in *NETSCAPE* changes the colors (by default from blue to pink) of all links previously accessed, until it is reset. Each story was rounded to the nearest half minute.

[6] We can argue the relative merits and demerits of this assumption from either perspective. On the one hand, the president wants to appear "on top of things" in order to garner public approval. As a result, his public activities and statements should actually reflect what is being attended to behind the scenes. On the other hand, there may be occasions when secrecy is important, in which case public activities and statements may be a facade that masks what the administration is actually doing.

[7] KEDS stands for the Kansas Events Data System. This program was developed by Philip Schrodt of the University of Kansas at Lawrence. Schrodt and Gerner (1994) describe the program in detail and also report validity tests of the machine-coded events.

[8] In a strict sense, not all PANDA defined events are strictly exogenous to the presidency. The president may take initiatives that form "historical threads" which become part of event progressions. In addition, some PANDA events may not be causally prior to presidential or media attention. The president and media may anticipate such events as elections, scheduled meetings, and the like. In theory it would be possible to separate out such potentially confounding influences if one had access to the text used to create the PANDA data. Given that we do not have such access, we simply acknowledge these potential confounding effects.

(given that theory does not) which, if any, parameter restrictions are appropriate.

Another important advantage of VAR modeling over the structural equation approach is that it provides a strong control for history by including multiple lags of each variable in all equations. This means that problems of specification error are of less concern, since the VAR disturbances are random with respect to time.[9] One way of viewing VAR modeling is as a multivariate extension of the Granger (1969) approach to causal inference. Each dependent variable is regressed on lagged values of itself, as well as lagged values of the other dependent variables in the system.[10] Causal relations are evaluated by conducting joint hypothesis tests for blocks of lags associated with each variable. What is interesting for us theoretically is that the control for history provides a representation of the degree to which issue inertia affects attention by both the media and president. A significant block of coefficients on the dependent variable in each equation implies that issue inertia is strong.

VAR methods typically exhibit high colinearity due to the multiple lags included for each variable in the system. For this reason analysts do not typically try to interpret coefficient estimates but instead do simulations to track out the system dynamics. We report Granger tests and simulations using the moving average response approach. That involves introducing a shock to a variable in the system and tracking out movements in the other variables using the VAR estimates for computing a forecast. Shocking a variable means successively multiplying the matrix of vector autoregressive estimates by a simulation vector containing changes in the variable of interest.[11]

Based on theory, we also entered international events into the VAR system as a single exogenous variable with no lags. Such an approach is sometimes termed ARX modeling (Judge et al. 1988, 776). We used no additional lags on the events variable, because events are discrete occurrences that should affect the media and president immediately, but once the events have occurred, the stimulus is no longer present. The system should be free to work itself out as the president and media interact to interpret the relative significance of the events. Thus, the events variable provides a theoretical test for the manner and extent to which events instantaneously shock attention by the media and president to foreign policy issues. The events variable is also a statistical control for the hypothesis of spuriousness. That is, we can exclude the possibility that events definitively shape systemic or institutional attention, rather than the processes discussed above. The question we address after controlling for events is whether persistent attention by either the media or president has long-term effects on attention by the other actor.

THE DETERMINANTS OF U.S. FOREIGN POLICY ATTENTION

The determinants of attention to Soviet, Arab-Israeli, and Bosnian issues are so similar that a single discussion can pertain to all three analyses.[12] This similarity in findings suggests a certain theoretical consistency for the three issue areas. In particular, the analysis shows that events were extremely important in driving contemporaneous attention movements by both the president and media. Attention inertia was strong for both actors, and a definite pattern of influence emerged from media attention to presidential attention, but not in the opposite direction.

In all three vector autoregressions reported in Table 1, contemporaneous events are strongly significant at less than the 0.01 level. Of course, this consistently strong relationship shows that the economy of attention is often determined by occurrences external to both the media and presidency. Controlling for exogenous events, how did issue inertia, media attention, and presidential attention play out over time? Table 1 also reports the Granger tests for attention to the Soviet Union, Arab-Israeli conflict, and Bosnian conflict, respectively.

In each case, past media attention to an issue Granger causes current media attention to the same issue, and past presidential attention also Granger causes current presidential attention.[13] Thus, as our

[9] We do not mean to imply here that the omission of relevant independent variables is not a problem in VAR. It can be. Disturbances can be random with respect to time but not random with respect to the dependent variables. Relevant to this point is whether the VAR should contain a congressional variable. Of course, theory suggests that continuous congressional influence on foreign policy attention should be less than that for the president. Edwards and Wood (1997) tested for the presence of congressional influence for two of the same issues and found none. See also Bartels (1996) for similar null findings.

[10] Based on Simms' (1980) methods for determining the appropriate lag length, we included in the Soviet and Arab-Israeli VARs four weekly lags for the media and presidential variables. We included five lags of these variables for the Bosnian conflict VAR. The Simms procedure entails sequentially adding lags to the VAR system and testing the statistical significance of each additional lag using a modified F test. We tested lags from 1 through 8 for the VAR system and arrived at the lag lengths reported below. We also did sensitivity testing to determine the effects of including additional lags up to eight weeks. Longer lag lengths diminished marginally the probabilities associated with the reported findings, but in all cases relations were stable and consistent. Residuals from final analyses were nonautocorrelated and normally distributed.

[11] To facilitate interpretation of the moving average responses, and because the variables had no natural metric, we standardized all variables prior to the analysis. All initial shocks are one standard deviation in magnitude. Because the innovations are correlated between variables, we plotted Choleski orthogonalized responses to simulated shocks of one standard deviation.

[12] The results were also reasonably consistent through time. In testing for coefficient stability, we did Chow tests as well as separate VARs for different presidencies. There were no substantively important differences across presidencies, but there were some differences in the magnitude of responses. Generally, President Reagan was somewhat less responsive on all three issues to the media than were presidents Bush and Clinton. All responses were in the same direction, however, and fully consistent with results reported below.

[13] A variable X Granger causes a variable Y when, controlling for the history of Y, past lags of X covary with current values of Y. The standard approach to Granger testing involves selecting an appropriate common lag length to simulate an infinite order autoregressive disturbance process and then doing significance testing for the effects of lagged X on Y.

TABLE 1. Granger Tests for Attention to Three Foreign Policy Issues, Controlling for Related Events

Dependent Variable	Coefficient Block	Soviet Union	Arab-Israeli Conflict	Bosnian Conflict
Media Attention	Media	6.64	14.92	13.51
		(0.00)	(0.00)	(0.00)
	President	1.49	0.89	1.61
		(0.20)	(0.47)	(0.16)
	Events	371.71	129.96	70.73
		(0.00)	(0.00)	(0.00)
Presidential Attention	Media	2.63	2.81	1.95
		(0.03)	(0.02)	(0.09)
	President	5.21	2.40	5.31
		(0.00)	(0.05)	(0.00)
	Events	134.56	36.24	26.63
		(0.00)	(0.00)	(0.00)

Note: The numbers in the tables are *F* statistics; *p* values are in parentheses. The Soviet and Arab-Israeli VARs contain four lags. The Bosnian conflict VAR contains five lags. Events at lag 0 are included as an exogenous variable to control for the contemporaneous effect of events on both the media and president. There were 514 weekly observations in the Soviet and Arab-Israeli series, running from the 27th week of 1984 to the 23d week of 1994.

theory predicts, much of what the media and presidents attend to through time is determined by issue inertia and what they have attended to in the past. As new issues emerge, political actors are not free to choose attention levels. Rather, they must attend to issues of continuing importance until they are resolved or displaced in the economy of attention.

The Granger tests are also consistent in showing that media attention to the three issues is exogenous to presidential attention. The president does not Granger cause media attention to the Soviet Union, Arab-Israeli conflict, or Bosnian conflict. Theoretically, this implies that the president is not very successful in directing media attention to foreign policy issues; rather, issue inertia and the continual progression of world events seem to drive media attention. Yet, the media do Granger cause presidential attention for all three issues. This is, of course, consistent with a model of presidential attention that views the president as responsive to those issues on the systemic agenda. It also suggests a view of the president as responsive to cues from the policy environment in deciding which issues should receive priority in the economy of presidential attention. As the media and mass public become more concerned with an issue, the president also becomes more concerned.

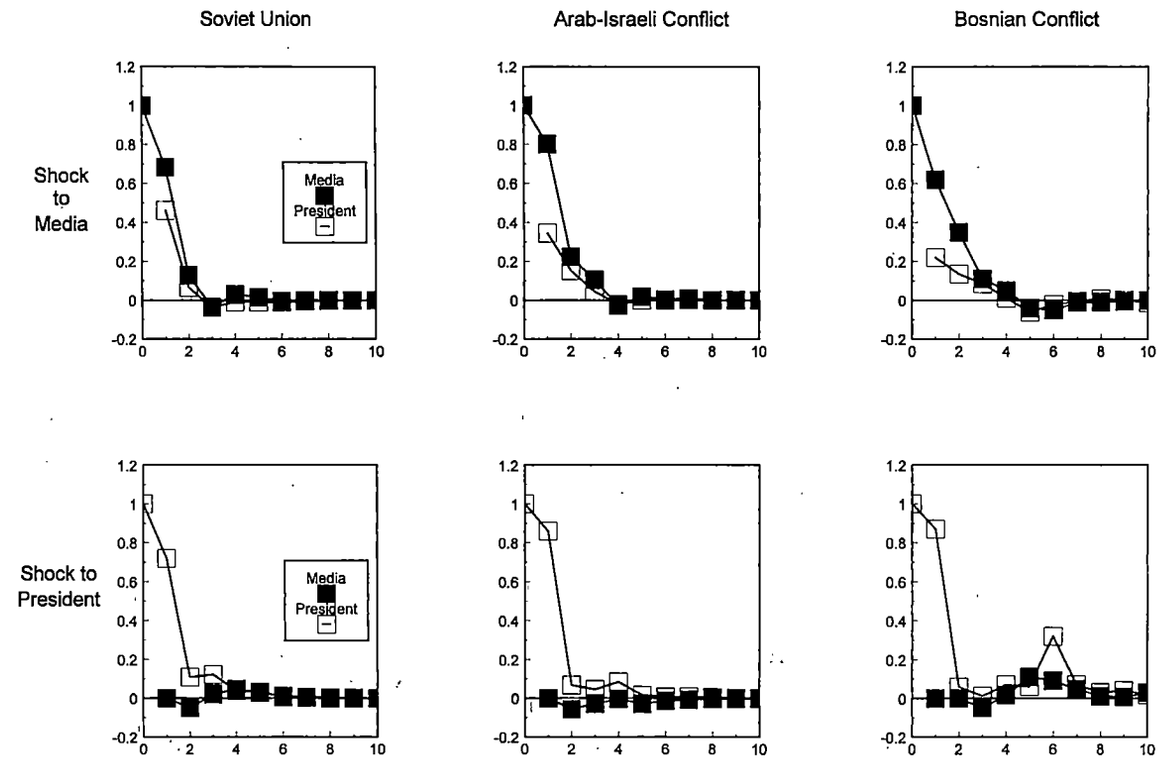
The Granger tests provide evidence concerning the joint effects of lagged media attention while controlling for attention inertia and exogenous events. They provide no indication of the polarity of relations, however, or the dynamics associated with evolving attention processes. We can explore the dynamics of presidential responsiveness to the media by tracking out simulations from the VAR through time. Figure 1 plots the moving average responses of the media and president, respectively, when each variable is subjected to a simulated shock for all three issues. The first row of Figure 1 shows that a simulated shock to media attention produces an increase in presidential attention. In initial response to this shock, presidential attention to the issues shifts upward by about 0.20 (for the Bosnian conflict) to around 0.40 (for the Soviet Union and Arab-Israeli conflict) standard deviations in the week after. The increase in presidential attention due to the media lasts for about two (for the Soviet Union) or three (for the Arab-Israeli and Bosnian conflicts) weeks, eventually returning to the preintervention equilibrium. In the other direction, the second row of Figure 1 shows that a shock of one standard deviation in presidential attention produces no systematic response by the media in either direction for any of the three issues. Thus, the president responds to the media, but the opposite relation does not occur.

COMPETITION IN THE ECONOMY OF ATTENTION

If presidential attention is a scarce resource, and if there is competition for presidential attention between multiple issues, then attention to an issue should change not only as a function of events, inertia, and media coverage of that same issue but also as a function of events associated with other issues. More specifically, as competing issues become more prominent in the economy of attention, attention to the primary issue should decline. A similar logic should also pertain to television news coverage and the media. Given the limited amount of time the TV evening news can devote to any one issue, it would not be surprising that as one issue becomes more visible another is squeezed out. For the president it is less clear that the squeeze should be as serious as for the TV evening news. The chief executive is under less pressure to balance coverage between domestic policy issues and foreign policy, so there may be attention to more than one foreign policy at a time by simply diminishing attention to domestic policy issues. The president also has much more than the standard half-hour of coverage for attending to foreign policy issues and may speak to many topics in one briefing, news conference, speech, or statement.

We can test the notion that competition exists

FIGURE 1. Simulated Moving Average Responses for Foreign Policy Attention Vector Autoregressions



among issues in the economy of attention using two different approaches. First, we can enter the events variable for competing issues exogenously into the VAR system of interest. If competition affects attention, then events from competing foreign policy issues should Granger cause and reduce attention to the primary issues. Second, we can identify some critical event associated with a competing foreign policy issue and do intervention analysis to determine whether the event reduced systemic and presidential attention to the primary issue. Using either approach, care is required in designing the test to assure that issues are orthogonal and actually competing for attention. For example, the stream of events associated with the Bosnian conflict may actually increase, rather than decrease, attention to the Soviet Union because of concerns that the former Soviet Union, due to long cultural and political ties to the Serbs, would react to U.S. involvement. Similarly, a critical event like the Persian Gulf War could increase attention to the Arab-Israeli conflict because of inseparability of the war from long-term conflict between the Arabs and Israelis. With these design constraints in mind, we employed both approaches to test for competition in the economy of attention.

We entered events for the Arab-Israeli conflict exogenously into the VAR for the Soviet Union and events for the Soviet Union and the Bosnian conflict exogenously into separate VARs for the Arab-Israeli conflict. Table 2 reports Granger tests from these analyses. In the analysis for the Soviet Union, reported in column 1, there is clear evidence that competing events from the Arab-Israeli conflict affected attention by the media and president to the Soviet Union. The evidence is less clear from the Granger tests for the Arab-Israeli conflict. Competing events associated with the Soviet Union did Granger cause presidential attention to the Arab-Israeli conflict, but there is little evidence that competing events from the Bosnian conflict caused either media or presidential attention to the Arab-Israeli conflict.

The Granger tests are suggestive but do not reveal the direction of relations, which should be negative if the theory is correct. The simulation analyses reported in Figure 2 show the dynamics associated with shocks of one standard deviation to events from competing systems. Consistent with the Granger tests, column 1 of Figure 2 shows that both media and presidential attention to the Soviet Union diminished for some time after a positive shift in Arab-Israeli events. Column 2 demonstrates that media attention to the Arab-Israeli conflict diminished sharply when there was a positive shift in Soviet events.[14] It also shows an initial decline in presidential attention to the Arab-Israeli conflict in response to Soviet events but suggests that three or four weeks later there was a resurgence of presidential attention. Of course, this

[14] Note that this large negative response was invisible to the associated Granger test. Such a disparity may occur due to contemporaneous feedback through the covariance matrix of errors.

TABLE 2. Granger Tests for Attention to Two Foreign Policy Issues, Competing Events Included

Dependent Variable	Coefficient Block	Soviet Union Competing Events, Arab-Israeli Conflict	Arab-Israeli Conflict Competing Events, Soviet Union	Arab-Israeli Conflict Competing Events, Bosnian Conflict
Media Attention	Media	6.91	14.59	3.14
		(0.00)	(0.00)	(0.02)
	President	1.67	0.96	1.62
		(0.16)	(0.43)	(0.17)
	Competing Issue	2.14	0.20	1.51
		(0.07)	(0.94)	(0.20)
Presidential Attention	Media	2.47	2.61	1.38
		(0.04)	(0.03)	(0.24)
	President	5.17	2.18	0.72
		(0.00)	(0.07)	(0.58)
	Competing Issue	1.99	2.95	0.37
		(0.09)	(0.02)	(0.83)

Note: The numbers in the table are *F* statistics; *p* values are in parentheses. All VARs contain four lags. As in Table 1, events at lag 0 are included as an exogenous variable to control for the contemporaneous effect of events on both the media and president. There were 514 weekly observations in the Soviet and first Arab-Israeli series, running from the 27th week of 1984 to the 23d week of 1994. There were 155 weekly observations in the second Arab-Israeli series, running from the 25th week of 1991 to the 23d week of 1994.

resurgence is consistent with the *long-term* importance of Arab-Israeli issues over the period of interest. Column 3 shows a consistently small but negative response by both the media and president to events associated with the Bosnian conflict. Thus, in all three issue areas the simulations demonstrate negative movements, with responses that remain negative with few exceptions.

We also did intervention analyses to test whether single events associated with competing issues affected media and presidential attention to primary foreign policy issues. In order for such critical events to have an effect they must be large in magnitude; they must also be relatively orthogonal to the foreign policy issue of interest. For the Soviet Union issue we

FIGURE 2. Simulated Moving Average Responses to Shocks in Exogenous Issues

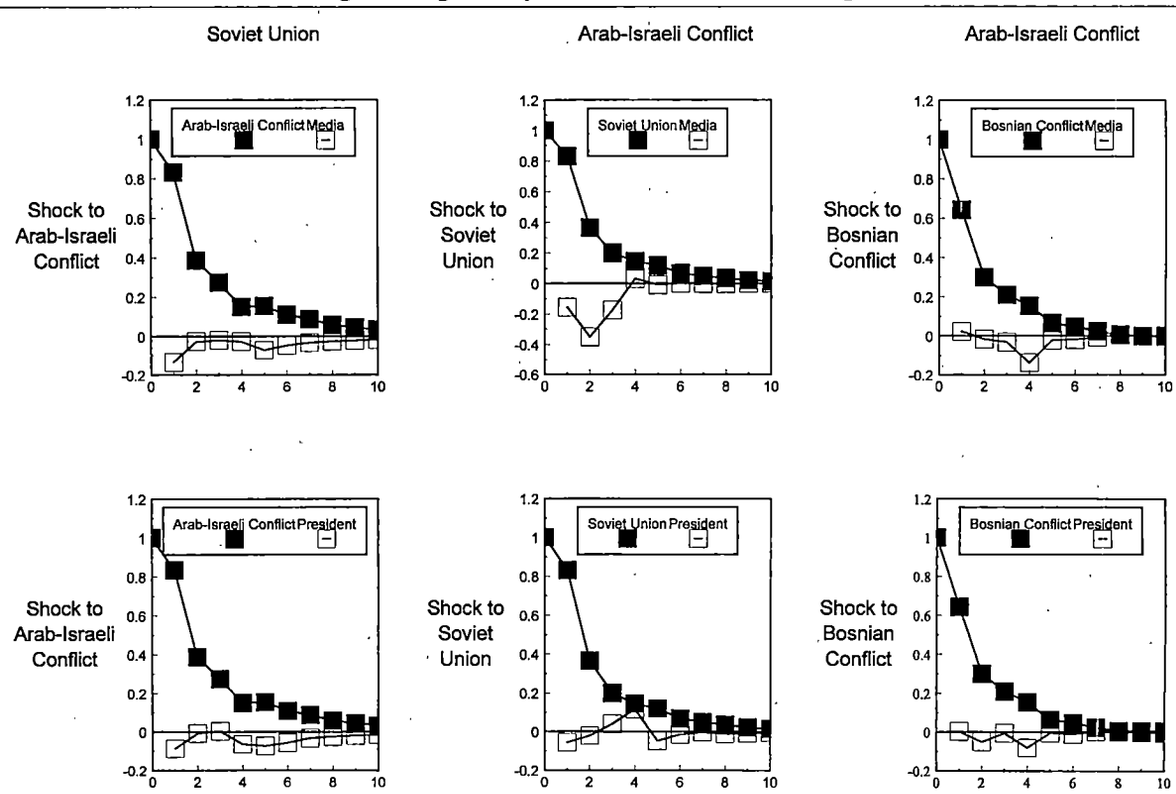


TABLE 3. Effect of Critical Events in Competing Issue Areas on Foreign Policy Attention

Variable/Parameter		Soviet Union: Media Attention	Soviet Union: Presidential Attention	Arab-Israeli Conflict: Media Attention	Arab-Israeli Conflict: Presidential Attention
Preintervention mean	μ	27.78***	29.76***	26.78***	9.02***
		(5.34)	(2.61)	(7.61)	(6.03)
Critical event	ω_0	−28.45***	−28.44	−15.75*	−8.26*
		(−2.58)	(−1.33)	(−1.68)	(−1.96)
	δ_1	0.97***	0.96***	1.00***	1.00**
		(40.79)	(15.05)	(38.37)	(39.50)
		(Gulf War)		("Death" of the Soviet Union)	
Autoregressive	ϕ_1	0.27*	0.58***	0.21*	0.26***
		(1.84)	(4.26)	(2.09)	(2.58)
Box-Ljung Q with 23 d.f.		4.88	3.56	26.47	20.20

Note: There are 100 weekly observations in each series. The Soviet time series runs from the 1st week of 1990 through the 26th week of 1991. The Arab-Israeli time series runs from the 38th week of 1990 through the 33d week of 1992. The interventions were pulses. The values of the dynamic parameters (δ) for the Arab-Israeli conflict were rounded upward to 1.00 and did not exceed the bounds of stability. The "death" of the Soviet Union variable was lagged two weeks for the presidential attention equation. The numbers in parentheses are t-statistics.
*$p < .05$, **$p < .01$, ***$p < .001$ (one tailed).

selected the beginning of the Persian Gulf War as having such characteristics.[15] The Iraqi invasion of Kuwait occurred on August 2, 1990, and we hypothesized this date as the time attention should have shifted away from the Soviet Union. For the Arab-Israeli conflict we selected the announcement of the "death" of the Soviet Union and formation of a commonwealth of former Soviet republics. This announcement occurred on December 21, 1991, and dominated the news for the next several months. Accordingly, this competing event should have produced a subsequent decline in attention to the Arab-Israeli conflict.

We used Box-Tiao (1975) methods to evaluate how these critical events affected attention to the primary issues. The results for the Persian Gulf War, reported in the first two columns of Table 3, show an immediate and long-term decline in attention to the Soviet Union by both the media and president. Media and presidential attention to the Soviet Union (ω_0), dropped off to near zero in the week of the invasion and recovered very slowly through time. The results for the "death" of the Soviet Union, reported in the third and fourth columns of Table 3 (ω_0), show a similar negative decline in attention to the Arab-Israeli conflict by both the media and president following the announcement. In all four analyses the near unity rate of recovery parameters (δ_1) show that the effects would have been near permanent if other events had not occurred later to restore the primary issue to prominence in the economy of attention.[16] Thus, the results uniformly show that critical events associated with competing issues diminish foreign policy attention to primary issues by both the media and president.

CONCLUSION

The political science literature on agenda setting has been directed at explaining how U.S. domestic policy issues reach and remain on the systemic and institutional agendas. Yet, foreign policy is an important component of U.S. policymaking and has been largely ignored. This is undoubtedly because foreign policy is fundamentally different from domestic policy and requires a different rationale for explaining the rise and fall of issue attention. In this study we have advanced a theory of attention to foreign policy issues and then tested expectations that arise from that theory using empirical data.

In contrast to domestic policy, foreign policy is primarily centered on the presidency due to constitutional obligations, the need for strong centralized leadership, and expectations by the public and other institutions. Yet, the president is not in full control of what receives attention in the foreign policy domain. There is an economy of presidential attention that is governed by the realities of scarce resources and rational efforts by the president to garner favorable public approval and historical treatment. The chief executive can occasionally establish issue priorities in the economy of foreign policy attention (as Nixon did with China) but may also have an agenda thrust on him by problem persistence, a continual progression of events, and a perception that he must attend more heavily to those issues the public deems most important. The economy of attention is stable so long as

[15] One could argue that this event was not orthogonal and that an increase in attention to the Soviet Union should have occurred at this time. Although the Soviet Union had been an ally of the Iraqi regime prior to the war, the changes occurring in the Soviet Union at about this same time diminished the likelihood of a Soviet reaction. Moreover, the Soviets actually cooperated with the United Nations in removing Sadaam Hussein's forces from Kuwait. Thus, we are comfortable in asserting orthogonality.

[16] The level at any particular time after an intervention may be computed recursively using $y_t = \omega_0 + \delta_1 y_{t-1}$. Thus, prior to the Iraqi invasion of Kuwait, the amount of presidential attention to the Soviet Union resulted in about 29.76 paragraphs per week. The week of the invasion the number of paragraphs dropped to $y_t = -28.44 + 29.76 = 1.32$ paragraphs per week. Five weeks after the onset of the Persian Gulf War the number of paragraphs had risen to 6.57 per week; ten weeks after it was up to 10.85 per week; 26 weeks after it was 19.92 per week. Of course, other events and interventions can affect this mathematical sequence, moving attention either up or down through time.

issues persist and problems continue to be defined as important. Disturbances to this stability may occur, however, due to exogenous events or changing public perceptions of the relative importance of foreign policy problems.

Our empirical analysis is based on the three most visible and enduring U.S. foreign policy issues over the past 15 years. The limited number of issues that we examine suggests a need for future research which differentiates on the basis of issue salience, importance, or issue type. The empirical findings may also be limited because the measure for the president depends on the president's public rather than private or hidden agenda. Tentatively, however, our study confirms expectations that flow from the preceding theory.

Both presidential and systemic attention are highly inertial. The president attends heavily to those issues that have also been heavily attended to in the past, as does the media, which is an indicator of systemic attention. We presume that this observed inertia exists because foreign policy problems are not easy to resolve and because problems that are defined as important at one point are likely to remain so defined. Such problems also demand continuing presidential attention because the system defines certain problems to be important. Thus, presidents who want to be viewed in a favorable light attend to those problems toward which the system directs them.

Foreign policy attention is inertial, but this inertia is often disturbed by a continual progression of events associated with the issue itself. As foreign policy crises emerge, attention becomes more intense to the issue in proportion to the severity of foreign policy events. Attention also shifts due to changing media interpretations of events and the perceived relative importance of an issue. Intense media coverage of an issue can extend presidential attention to that issue for a longer period than is normal. Because the economy of attention involves scarcity, issue inertia is also often disturbed by competition from other foreign policy issues. Systemic and institutional attention shifts away from one issue toward another due to world events that alter perceptions of what issues are most important at the time. As issues in one area are perceived as more important, constraints imposed by limited time and resources necessarily mean that issues in other areas receive less attention.

In these regards, foreign policy differs sharply from domestic policy, since the progression of events is far more important. There is no analogous stream of domestic policy events similar to those that determine the context for attention to the Soviet Union, Arab-Israeli conflict, and Bosnian conflict. Whereas critical domestic policy events can occur and focus presidential attention for a time on particular issues, the foreign policy agenda operates in the context of a *continually* unfolding international drama. The drama depends on the media for production and interpretation. The president is drawn into the drama as an integral part, but he has no script and often must respond in impromptu fashion to media interpretations and the continuous progression of events affecting U.S. interests.

APPENDIX

The following keywords were used to search the *Vanderbilt Television News Archive*. The list includes the words and the corresponding years in which they were used. Not all the stories that came up with these keywords were counted. We read the abstracts and coded the variables so that only those stories related to the Soviet Union, Arab-Israeli conflict, or the Bosnian conflict were part of the measure.

Year	Vanderbilt Key Words
Soviet Union	
1984	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons
1985	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons
1986	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Chernobyl, Gorbachev
1987	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Gorbachev
1988	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Gorbachev
1989	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Gorbachev
1990	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Republic, Gorbachev
1991	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Gorbachev, Yeltsin, Republic, Commonwealth
1992–94	USSR, Soviet, Russia, Moscow, US-USSR rels., arms control, summit, nuclear weapons, Yeltsin, Gorbachev, plus all 16 former Soviet states, besides Russia: Latvia, Lithuania, Estonia, Azerbaijan, Mongolia, Kazakhstan, Tajikistan, Moldova, Belarus, Armenia, Ukraine, Uzbekistan, Turkmenistan, Kyrgzstan, Georgia, Kurdistan
Arab-Israeli	
1984–94	Israel, Jerusalem, Tel Aviv, Syria, Lebanon, Jordan, Egypt, PLO, Arafat, Palestine, Palestinian, Arab-Israeli, terrorism, intifada, W. Bank, Gaza, Golan, occupied
Bosnia	
1991–95	Yugoslavia, Bosnia, Bosnia-Herzegovina, Serbia, Croatia, Slovenia, Sarajevo, civil war, embargo, ethnic cleansing, Moslem, Croat, Serb, Balkans

The keywords for the index of the *Public Papers of the President* are provided in the following list. All the entries mentioned in the index were read to ensure validity for inclusion as attention to the Soviet Union, Arab-Israeli conflict, and Bosnian conflict.

Year	Public Papers Key Words
Soviet Union	
1984–90	United Soviet Socialist Republics (all concurrent listings), nuclear weapons (all concurrent listings), arms control, Afghanistan, any mention of Soviets in any other listing in the Index (i.e., Middle East—Soviet Role)
1991–94	Same as above, except no Afghanistan, and included: Commonwealth of Independent States (all concurrent listings), Estonia, Latvia, Lithuania
Arab-Israeli	
1984–94	Israel, Jordan, Syria, Egypt, Lebanon, Middle East (having to do with conflict or terrorism), terrorism (dealing with the Middle East), and all "see also"
Bosnia	
1991–95	Bosnia (all subheadings), Macedonia, Croatia, Serbia, Yugoslavia, Montenegro

The following keywords were used for searching the PANDA events data set within a database environment (Microsoft Access). Abbreviations were used per the codes. These correspond to targets, sources, and places.

Year	PANDA Key Words
Soviet Union	
1984–94	Armenia, Azerbaijan, Belarus, Estonia, Georgia, Kazakhstan, Kurdistan, Kyrgystan, Latvia, Lithuania, Moldova, Mongolia, Russia, Tajikistan, Turkmenistan, Ukraine, USSR, Uzbekistan
Arab-Israeli	
1984–94	Israel, Jordan, Palestine, Syria, Egypt, Lebanon
Bosnia	
1991–95	Bosnia, Yugoslavia, Croatia, Serbia, Montenegro, Macedonia

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Comparing European Publics

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Beliefs in Government, Volume Four: The Impact of Values. Edited by Jan W. van Deth and Elinor Scarbrough. New York: Oxford University Press, 1995. 608p. $65.00

Beliefs in Government, Volume Five: Beliefs in Government. By Max Kaase and Kenneth Newton. New York: Oxford University Press, 1995. 200p. $45.00.

This monumental five-volume series, written by leading European political scientists, presents a comprehensive analysis of the role of mass publics in political life. The result of a unique program in cross-national collaborative research, it constitutes an important event in the development of transnational social science. Produced under the general editorship of Max Kaase, Kenneth Newton, and Elinor Scarbrough, these volumes are the product of the Beliefs in Government Research Program sponsored by the European Science Foundation, which funded the five-year program. The final product is the work of fifty-five authors from sixteen countries.

The general aim of the project was "a study of changing mass attitudes towards government in Western Europe in the post-war period" (Kaase and Newton, v. 5, 1). A key methodological decision was to base the entire project upon a reconsideration and reanalysis of available survey data. Kaase and Newton explain that the availability of the biannual Eurobarometer data was the major factor leading to the decision to conduct this project, and those data provide the main source for the analyses. The origins of the Eurobarometer surveys have been described in Reif and Inglehart (1991); they now constitute one of the most widely used data sets archived by the Inter-university Consortium for Political and Social Research in Ann Arbor. Other data sets are used in these volumes, the most important being the 1981–83 and 1990–91 World Values Surveys and surveys conducted by the International Social Science Program (ISSP) in 1985 and 1990. But the Eurobarometer data provide the most extensive time series, together with a wide range of cross-national comparisons. Nevertheless, these data are limited: They normally sample only members (or future members) of the European Union, and the survey content often reflects the specific policy needs of EU decision makers and not necessarily the broader range of scholarly interests.

Kaase and Newton point out that "it would have been enormously attractive and desirable to organize the analysis around a tightly knit and broadly conceived theoretical framework" (v. 5, 8). For example, they could have attempted to marry David Easton's (1965) theoretical distinction among the authorities, regime, and community with Gabriel A. Almond and Sidney Verba's (1963) empirical analysis distinguishing among the system, inputs, outputs, and the self. But, as they correctly note, although it was a theoretical breakthrough, *The Civic Culture* led neither to a sustained effort to develop the theoretical underpinnings of the concept of political culture nor to a systematic account of how political cultures change over time. Although Easton's theoretical distinction among objects of support is highly suggestive, and although it is used in the empirical analysis, public opinion researchers have found it difficult to distinguish operationally among these objects. Likewise, as Kaase and Newton point out, it would have been useful to study elite attitudes toward government, but there are too few studies of elites, let alone comparative studies, to permit such comparisons.

As a result of these theoretical and data limitations, the Beliefs in Government project necessarily follows an eclectic approach and is largely guided by past research on West European politics. The authors take a broad view of government and include not only the groups or parties that hold political power. Rather, the term *government* "has a much broader meaning and covers all the political institutions of the executive, the legislative, and the judiciary and their associated activities, decisions, and outputs." The study itself deals with "the attitudes of individual citizens which directly and indirectly relate to their role in the political process, their views of individual and corporate political actors, and their assessment of the institutions of government and its democratic rules of the game" (Kaase and Newton, v. 5, 8).

The project makes an extremely valuable contribution to the research literature on comparative politics. The vast majority of our own work has focused on political attitudes among mass publics, and our natural inclination is to be sympathetic to this enterprise. We will begin by providing an overview of the five-volume study and then provide our own conclusions about its implications for future research in comparative politics and for the future of democratic politics in Western societies.

Note: The authors thank an anonymous reviewer for his helpful suggestions.

THE FIVE VOLUMES

The first four volumes contain many chapters that students of European politics and comparative public opinion will find valuable. The fifth volume, coauthored by Kaase and Newton, provides a useful overall perspective on the research. Space constraints make it impossible to discuss each chapter's contributions in any detail.

Citizens and the State focuses on the question: "Does contemporary politics pose a challenge to representative democracy?" Responding to a set of theories that postulate a legitimization crisis of late capitalism (Habermas 1973), a crisis of governability (King 1975), or a crisis of democracy (Crozier, Huntington, and Watanuki 1975), the authors analyze a massive body of cross-nationally comparable data in order to determine whether such factors as changing values and cognitive mobilization have undermined mass support for representative democracy. Summing up the findings from various analyses in this volume, Dieter Fuchs and Hans-Dieter Klingemann conclude that citizens have indeed changed during the postwar years, but representative democratic institutions have responded and adapted to these changes in ways that have maintained mass support. One of their most interesting findings is that, from 1970 to 1990, postmaterialists and Europeans with high levels of cognitive mobilization increased most in their relative levels of system support and trust in government: Relatively disaffected at the start of this period, they became increasingly integrated into the system. This was a major achievement. Fuchs and Klingemann attribute this development to widespread policy changes that were made in West European countries. Political leaders adopted key elements of the postmaterialist agenda. For example, there was a massive increase in emphasis on environmental protection policies, which rose from relative obscurity to play a prominent political role, overshadowing emphasis on traditional socialist economic policies by the 1990s.

Klingemann compares the issue positions taken by political party elites with those of their mass electorates. He finds that, across Western Europe as a whole, the parties of the Right have behaved according to Anthony Down's theory: They moved to the political center, where the votes are concentrated. But the parties of the Left did not. Depending upon one's ideological perspective, they may be described as more sincere or as more ideologically rigid than those of the Right, but in any case they tended to take issue positions that were more extreme than those of their own electorates and of voters in general.

In a contribution that is particularly interesting in light of widespread concern about declining electoral participation in the United States, Richard Topf concludes that West European voter turnout has *not* declined in recent decades. Actually, Topf's own analysis indicates that across Western Europe as a whole electoral participation *did* decline, although only by an overall mean of four percentage points—which he dismisses as insignificant. But this change may be more significant than Topf realizes. During this period, West European countries experienced a tremendous increase in the proportion of the electorate with secondary and higher levels of education, and the average age of the electorate was rising. Both factors tend to be associated with a higher rate of electoral participation and may have been expected to produce a substantial *increase* in voter turnout. Since turnout actually declined in most countries, demobilizing forces (perhaps a diminishing ability of established parties to control their electorates) must have been at work. In contrast with this finding, and in keeping with the thesis that long-term value changes and demographic changes should tend to increase political participation rates, Topf finds that nonelectoral participation has increased dramatically. Moreover, the repertory of acceptable political action has broadened since the 1960s to include a number of direct and uninstitutionalized forms of action, such as petitions, demonstrations, and political boycotts. Only electoral participation (a highly institutionalized and elite-directed form of action) has stagnated or declined.

Hermann Schmitt and Sören Holmberg examine the thesis that new forms of interest mediation are challenging the functions of political parties and that the links between parties and people have been weakening. They find an overall decline in partisan loyalties across Western Europe, but the depth and speed of this development vary considerably from one country and one period to another. For example, in the new democracies of southern Europe there has actually been a modest growth of partisanship. Sweeping theories about the decline of partisanship, they conclude, must be qualified by examining specific cases.

This volume presents a wealth of fascinating findings that often qualify broad theories of social change, reminding us that empirical reality is complex. Overall, the findings indicate that, despite major social, cultural, and economic changes, West European democracies have maintained high levels of political legitimacy, contrary to predictions of a legitimacy crisis, ungovernability, and overload. Democratic institutions and actors seem to be more flexible and more responsive to social change than these theories anticipated.

The second volume focuses on international governance and mainly upon attitudes toward the European Community. It relies heavily upon the Eurobarometer data, although it also provides interesting results from East Europe based upon an East European Eurobarometer conducted in 1990. Richard Sinnott provides a useful discussion of the European integration literature, including a critique of neofunctionalist theories. He emphasizes the need to study mass political attitudes, noting that political developments since 1989 have underscored the importance of understanding public opinion. Oskar Niedermayer and Bettina Westle provide an interesting typology of orientations, drawing largely upon a reformulation of Easton and Almond and Verba. The utility of this typology in actual analysis is not very clear, however. Several chapters point to differences in question wording that create problems for studying trends, and as Niedermayer makes clear, the actual questions used are often diffi-

cult to relate to the typology of orientations that is intended to provide the framework for analysis. The data strongly suggest widespread support for European integration but no growing sense of European identity among the mass public. The data suggest an increase in European identity between 1982 and 1986, followed by a decline until 1990, a brief recovery in 1991, and a general decline in 1992. Niedermayer's analysis suggests that trust is centered on the nation-state. Still, there has been a growing sense of trust among members of the EU. And there is public support both for enlargement of the EU and extended military cooperation in NATO.

Bernhard Wessel's analysis of the correlates of support for European integration is especially interesting. He specifically contrasts Inglehart's argument (originally advanced in 1967) that generational replacement among the West European public would gradually lead to support for European integration with the thesis that change results from intrasocietal diffusion. Based upon his analysis of the Eurobarometer surveys, Wessel concludes that age differences in support for European integration are not strong or consistent enough to support a generational replacement explanation. Instead, the results of his analysis suggest that diffusion processes are more important. Since Wessel's analysis of diffusion processes is based largely upon inferences about the declining relationship of socioeconomic factors to attitudes toward European integration, the data supporting his conclusions are indirect, but his chapter provides an interesting and valuable study of the dynamics of attitude change.

The chapter by Frank Aarebrot, Sten Berglund, and Thomas Weninger analyzing attitudes in Austria, Finland, Norway, Sweden, and Switzerland toward the European Community is also interesting, particularly when they examine the decision of Norwegian voters in 1994 not to join the EU. Based upon a European integration study conducted in these countries in 1992 (coordinated with Eurobarometer No. 37), it seems the Norwegian public is not very different from other West European publics, although Norwegians were less likely to believe that their country would benefit from membership. Another difference may help account for the Norwegian defeat of EU membership. In Denmark, Finland, Austria, and Sweden, respondents who felt informed about the EC were more likely to support European integration. In Norway, opponents of membership felt as informed as supporters of membership. The authors attribute this relationship to the presence in Norway of a strong and well-organized mass movement against membership. Norway has often been studied as a deviant case in a wide variety of contexts, from the maintenance of stable democratic institutions to the dynamics of coalition formation. Obviously, a broadly comparative volume does not leave much space for deviant case analysis.

The third volume of the series differentiates between the "range" of government activity and its "degree." As Ole Borre and Michael Goldsmith use these terms, *range* refers to "the whole gamut of government activities, such as spending on services, taxing people, regulating their behavior, providing for defense, improving the environment," whereas *degree* means "the intensity with which government pursues a particular activity" (v. 3, 4). Contributors to this volume rely heavily upon the Eurobarometer surveys but also employ surveys sponsored in 1985 and 1990 by the ISSP, as well as the two Political Action surveys conducted in 1973–76 and 1979–81, and they occasionally use national surveys, such as the British Election Studies. But there are substantial limitations, and, as Borre and Goldsmith write: "Perhaps more than the other concerns of the Beliefs in Government project, our topic reveals the lack of systematic comparative survey data" on this topic (v. 3, 14). Indeed, throughout this volume the authors express frustration about the lack of longitudinally comparable data. Nevertheless, the sensitivity of these scholars to these problems gives more credibility to the patterns that they do discover.

The contributors carefully document both governmental growth itself and the demographic changes that have contributed to this growth. The thesis that government overload has led to a decline in support for government involvement receives little support, except perhaps in The Netherlands in the early 1980s. Maria A. Confalonieri and Kenneth Newton provide an especially interesting chapter on attitudes toward taxation, although their data are limited to the two relevant ISSP surveys, which reduces their ability to study trends. Their overall conclusion, drawing upon the entire Beliefs in Government project, is that there is a good deal of evidence that public faith in politicians has declined in some West European countries, but there is still substantial support for the political community and the regime. They find "very little evidence to suggest a tax revolt which would undermine the principles of the modern welfare state" (v. 3, 146).

At a more macroanalytic level, Edeltraud Roller finds no relationship between national living standards and support for socioeconomic equality. Per Arnt Pettersen specifically examines support for the welfare state, relying heavily upon the British Election Studies. He finds generally stable levels of support. This holds true in Scandinavia, except for Finland, where limited data suggest declining support. The major conclusion is that there is no hard-core opposition to the welfare state. In fact, "there is no evidence that the welfare states, or specific welfare programmes, are generally losing support over time" (v. 3, 229). Olof Johansson's discussion of support for protecting the environment is interesting, even though, given data limitations, he was not able to examine questions about the priority of environmental protection as compared with other issues. Nevertheless, Johansson presents a typology combining values and views of economic expansion that may prove useful in future studies of environmentalism.

The bottom-line conclusion to emerge from *The Scope of Government* is that there is little support for major changes in the welfare state, no evidence of a tax revolt, and little evidence that Western governments have succumbed to a "crisis of legitimacy." Yet, this volume, more than the other four, underscores some of

the limitations of available survey data. Indeed, at one point Borre laments: "In view of the inadequacy of our data on many important points, one may well wonder whether beliefs about the scope of government is a worthwhile topic of study" (v. 3, 384). We think it is a worthwhile enterprise, even if the authors of this volume were often forced to rely upon fragmentary evidence.

Volume four focuses on three major value dimensions and their effect on mass publics: (1) religious-secular values; (2) materialist/postmaterialist values; and (3) the materialist Left-Right dimension. The contributions make certain key findings clear. First, there has been a pervasive decline in religious values. Wolfgang Jagodzinski and Karel Dobbelaere first demonstrate this trend and then imaginatively test a number of hypotheses about why this change has occurred. Second, there has been a shift from materialist to postmaterialist values, as Scarbrough documents convincingly. Third, Left-Right materialist values have persisted with considerably more continuity than is generally believed. Despite journalistic claims to the contrary, there has been no significant shift toward the Right, as Oddbjørn Knutsen makes clear. What has happened instead has been a redefinition of what Left and Right connote.

The authors find substantial intergenerational differences. Younger people tend to be more secular, more postmaterialist, and more likely to identify with the materialist Left than are their elders. They also tend to be more supportive of ecological and feminist movements and more libertarian than older respondents. These differences seem to be linked with long-term change. But here again, the authors draw a relatively differentiated picture of what has been happening, distancing themselves from claims of a monolithic shift in any one specific direction. In the concluding chapter, Jan W. van Deth and Scarbrough argue that it is more accurate to speak of "value changes" than of "value change," for a number of different changes are taking place. The overall picture is one of growing value heterogeneity that may make the job of governing increasingly difficult, although the authors conclude that democratic institutions are good at adapting to challenges.

One inherent virtue of democracies is that parties and politicians must adapt to changes in the electorate. If they do not, then they are not reelected.

The authors of this volume emphasize the analytic primacy of values in explanations of political change. For example, Knutsen finds that values have a stronger effect on European voting behavior than do social structural variables. Moreover, structural voting is a more important factor in the less advanced industrial societies than in the most advanced, a finding that has interesting developmental implications. Values also play a significant role in shaping concerns about environmental pollution and participation in environment-saving activities (though, as Masja Nas emphasizes, values are not the only factor involved). Values help shape political interest and involvement, as Oscar W. Gabriel and van Deth demonstrate; and they have a major influence on grass-roots political activity, as Peter Gundelach makes clear.

In the analysis of social change, there is no substitute for time-series data. This volume draws on an unusually extensive time series, using it in new ways in order to carry out critical reevaluations of previous work—including our own. The authors present some thoughtful and well-reasoned criticism of the theory of intergenerational value change first proposed by Inglehart in 1971, which argued that a shift from materialist to postmaterialist values was taking place in advanced industrial societies and was driven by intergenerational value change.

This claim was controversial at the time, and scores of articles and books have attacked the thesis. The most widespread alternative interpretation was that the observed age differences were not a permanent part of the outlook of the younger birth cohorts, but simply a life-cycle difference that would fade as the younger cohorts aged. The intergenerational replacement thesis implied that a long-term shift in prevailing values would occur as the young replaced the older cohorts; the life-cycle interpretation implied that no value change was taking place. The evidence in our view (Abramson and Inglehart 1995) strongly suggests that the shift from materialist to postmaterialist values in Western Europe largely results from generational replacement.

Although this volume contains contributions by some of Inglehart's most prominent earlier critics, one of its main conclusions is that the shift from materialist to postmaterialist values is indeed taking place. Thus, although proposing alternative interpretations of Inglehart's findings, Scarbrough's evaluation of the empirical evidence concludes: "Indisputably, across much of Western Europe, value orientations are shifting" (v. 4, 156). "The potency of postmaterialist theory lies in providing us with one of the few explicated accounts of value change in contemporary West European societies" (v. 4, 157). The debate about the value change thesis has changed. The issue is no longer whether an intergenerational value shift is taking place but *why*. The authors of this volume examine a variety of alternative explanations.

Various analyses in this massive work explore the effect of postmaterialist values on political behavior. Summing up the findings from the five volumes, Kaase and Newton conclude: "We find substantial support for the model which traces social changes to value changes, and value changes into changes in political attitudes and behaviors, especially through the process of intergenerational replacement.... It is clear that the decline of religious values and the rise of postmaterialist values have transformed the cultural composition of Western democracies in recent decades" (v. 5, 63).

CONCLUSION

Continuity and longitudinal comparability are crucial to the cumulative progress of social science research. In ongoing research projects there is always tension between scholars who strive for maintaining compara-

bility over time and those who stress the need for innovation. We tend to side with those who see the need for maintaining comparability (see Abramson 1990), but at the same time we recognize that some of the more innovative developments in survey research employ new techniques (Sniderman 1993, Sniderman et al. 1991). Tradeoffs between comparability and innovation will be increasingly difficult in an era of declining resources to support social science research. The goal of maintaining comparability over time is especially difficult to attain in cross-national research. Scholars must constantly be aware of changes in question wording and changing contextual effects. On balance, the Beliefs in Government series provides one of the most ambitious examples of secondary data analysis in social science research, and it highlights both the strengths and limitations of this approach. The strengths are impressive. Although future research will need to address new problems that Western publics will face in the post–Cold War era, it is essential to maintain longitudinal comparability. The authors of this work do so in exemplary fashion, applying a healthy dose of skeptical criticism to our own work and that of other students of social change.

The study also highlights the strengths of cross-national cooperation. The scholars who contributed to this volume represent a wide range of European countries, and their specific country-level expertise often leads to interesting insights. These scholars were heavily influenced by American political science research in their choice of topics and the analytical strategies pursued, but they bring a wide variety of perspectives to bear on their interpretations. Though very few data from the United States or Canada are presented, students of North American politics have much to learn from these volumes. Unfortunately, the dearth of material on the United States seems likely to limit the readership of these volumes in this country. Despite a growing dissatisfaction with the compartmentalization of comparative politics into area studies (Bates 1997, Lustick 1997; for a dissenting view, see Johnson 1997), students of American politics tend to remain overly parochial. This is unfortunate, for the research in the Beliefs in Government volumes provides important insights for public opinion researchers in the United States. Students of American politics need to account for the very high level of political cynicism in the United States as well as the remarkably low level of electoral participation. The fourth volume, *The Impact of Values*, seems particularly valuable for placing these questions into a broader comparative perspective.

The substantive findings that emerge from the Beliefs in Government project are encouraging for supporters of democracy. During the postwar years, Western governments have faced many problems, and the nature of these problems will doubtless change in the post–Cold War era. All the same, we concur with Kaase and Newton's optimistic conclusion: "The lesson of the Beliefs in Government research is that democracies can stand considerable internal and external pressure while at the same time adjusting to changing environments" (v. 5, 16). The end of the Cold War, they argue, has created a new era for Western Europe. But, they say, "our speculation is that after an initial period of irritation and disorientation, liberal democracies will measure themselves against fresh challenges—religious, cultural, environmental, for example—and survive by adapting" (v. 5, 16). We share this optimism, at least for the established democracies of Western Europe, North America, and Japan.

This optimism seems to present a sharp contrast with the earlier thesis that democracy was undergoing a crisis of governability. The evidence that Western political systems may face problems of governability has by no means disappeared. During the past forty years there has been a massive decline in trust in government among the U.S. public. This trend abated during the 1980s, but trust fell again during the 1990s. In 1994, according to National Election Study surveys, the American people's trust in government was at an all-time low, even though unemployment was low, inflation was moderate, economic growth was high, and crime rates were falling. Trust did rise between 1994 and 1996, but the increase was relatively small. The decline in political trust has given rise to a good deal of scholarly debate (see Abramson 1983; Craig 1993; Nye, Zelikow, and King 1997).

Declining trust in government does not seem to be limited to the United States. Russell J. Dalton (1996) and Ronald Inglehart (1997) find that public confidence in political elites and hierarchical institutions has been eroding, as part of a pervasive decline in deference to authority that can be seen throughout advanced industrial society. The large vote for Perot in 1992 and the erosion in support for established ruling parties, from Italy to Japan, seem to be symptoms of this process. Similarly, Dalton (1997), using the longest possible time series, finds a decline in party identification in eighteen of the twenty countries for which data are available for the past two to four decades. As Robert Michels pointed out decades ago, political parties are inherently oligarchical, and support for such institutions seems to be declining.

How can we square these findings with the trendless fluctuations in support for the political system reported by the authors of *Beliefs in Government*? The main variable in their trend analysis is based on the question: "Are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy is functioning in (respondent's country)?" This question was first asked in the 1973 EC survey and was used repeatedly in subsequent Eurobarometers and other surveys; an exceptionally large body of time-series data is available.

This is a hybrid question. It refers to "democracy," and insofar as the responses are evaluations of *democracy*, we would expect to find a rising trend, not a falling trend over time. As Inglehart (1990, 1997) has argued, economic development tends to bring rising support for democracy; we wholeheartedly concur with the authors of *Citizens and the State* that support for democracy has not eroded over time.

But the "satisfaction with democracy" question does not just tap support for democracy: It *also* reflects current economic conditions; its level falls during re-

cessions and rises with economic recovery. Moreover, this variable taps satisfaction with the authorities currently in power. When a government of the Left is in office, respondents who prefer the Left show higher levels of satisfaction; when a government of the Right is in power, Right-leaning respondents show higher levels of satisfaction. Because this variable reflects current conditions, we would expect it to show short-term fluctuations. And any tendency for people to show diminishing support for hierarchical authorities in their country tends to be offset because the other half of this hybrid variable does tap support for democracy—which tends to be *rising*.

Are mass publics losing confidence in democracy itself? The analyses presented here indicate that the answer is "no." On the contrary, the same publics that are becoming increasingly critical of hierarchical authority are becoming increasingly resistant to authoritarian government, more interested in politics, and more apt to play an active political role. Although hierarchical political parties are losing control over their electorates, and elite-directed forms of political participation such as voting are stagnant or declining, elite-challenging forms of political participation are becoming more widespread. And though they tend to distrust political authority and big government, the publics of advanced industrial societies value democracy more, not less, than the publics of less secure societies. The changes *do* make life more difficult for the governing elites. But they do not undermine democracy; within reasonable limits, they tend to make it more secure.

Can the decline in respect for authority go to such an extreme that it undermines the stability of democracy, rendering democratic societies ungovernable? It seems conceivable. Harry Eckstein (1966, 1996) has argued that, to function well, democracy requires a mixture of activism and respect for authority (cf. Almond and Verba 1963). A total rejection of authority could render democracy ungovernable, as the crisis-of-democracy critics have argued. This has not occurred, and whether it occurs in the future remains to be seen. As the authors of the Beliefs in Government volumes demonstrate, thus far democratic societies have shown an impressive capacity to adapt to new challenges.

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The Doleful Dance of Politics and Policy: Can Historical Institutionalism Make a Difference?

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State and Party in America's New Deal. By Kenneth Finegold and Theda Skocpol. Madison: University of Wisconsin Press, 1995. 342p. $54.00 cloth, $19.95 paper.

The Poverty of Welfare Reform. By Joel F. Handler. New Haven, CT: Yale University Press, 1995. 177p. $25 cloth, $15.95 paper.

Improving Poor People: The Welfare State, the "Underclass," and Urban Schools as History. By Michael B. Katz. Princeton, NJ: Princeton University Press, 1995. 179p. $22.50.

The Politics of Welfare Reform. Edited by Donald F. Norris and Lyke Thompson. Thousand Oaks, CA: Sage, 1995. 255p. $45.00.

Words of Welfare: The Poverty of Social Science and the Social Science of Poverty. By Sanford F. Schram. Foreword by Francis Fox Piven. Minneapolis: University of Minnesota Press, 1995. 242p. $55.00.

Social Policy in the United States: Future Possibilities in Historical Perspective. By Theda Skocpol. Princeton, NJ: Princeton University Press, 1995. 326p. $29.95.

Published in the waning days of Aid to Families with Dependent Children (AFDC), these books on social policy, poverty, and the welfare state provide reminders that policy studies and political science have been awkward dance partners for the past half-century. As a coarse rule, policy analysts, especially in schools of public policy and public administration, produce analytically guided case descriptions rather than the tools themselves. Their orientation to theory and history tends either to be starkly instrumental or purely normative. By contrast, political scientists who write about policy are disposed to muster their cases to illustrate or test deductive theory, furnish units for quantitative or qualitative multivariate manipulation, or validate propositions about structure, process, and behavior rather than participate in policy debates. If policy analysts aspire to influence in current affairs, political scientists primarily want to advance the discipline.[1] Inevitably, there is a lot of stepping on toes.

My purpose in this essay is to ask whether and how historical institutionalism may better choreograph interaction by students of American politics and policy to the advantage of both. There are good reasons to think it can. In recent years, a growing number of political scientists who self-identify as historical institutionalists have transformed scholarship on social policy by privileging the formative capacity of specific institutional legacies and arrangements (Dunlavy 1993; Hattam 1993; Immergut 1992, 1996; Koelble 1995; Pierson 1994; Steinmo 1993; Steinmo, Thelen, and Longstreth 1992; Weir 1992). While advancing their subfields of comparative and American politics, they also have sought to influence studies of public policy, aiming, as Finegold and Skocpol put it, to "grasp the true choices and the actual possibilities that face . . . makers of public policies for the future" (p. 242).

It makes sense, therefore, to think about the achievements and promise of historical institutionalism in the context of considerations of welfare and social policy, one of this school's favored set of subjects, juxtaposed to more conventional ways of working and to other critiques of policy studies. The books under review cluster in three categories. Norris and Thompson's collection of essays on welfare reform in six states and Handler's critical explication of the terms of the recent welfare debate directly address the last years of AFDC. Schram and Katz generate vigorous challenges to the customary ways scholars study welfare and poverty. In partnership with Finegold and on her own, Skocpol mounts the case for an alternative grounded in history and focused on institutions.

The first set, read with and against the intentions of its authors, and the second, scanned for antidotes to complacency, disclose unsatisfactory features of domestic policy studies; not least their flat use of history and uncoordinated orchestration with the concerns, methods, and analytical torque of political science. The third makes a resourceful, exigent case for historical institutionalism with which I am in keen sympathy and much agreement. Notwithstanding, I dispute the encircled qualities of Skocpol's and Finegold's research program, arguing its enclosure cobbles its persuasiveness as an advertisement for the approach they advocate.

AFDC AS TRACER

Though the scale of AFDC as part of the federal enterprise at the time of its demise was small (spending, on a matching basis, approximately $17 billion of federal funds in 1995), its repeal, capping a period of innovation at the state level inaugurated by the call for experimentation in the Family Assistance Act of 1988, condensed important trends crying out for analysis: the transformation of American federalism; a practical and

[1] A careful reader will discern that in the first parts of the text I refer to "policy analyses" and "policy studies" but not to "policy sciences," a term I reserve for the essay's conclusion. I make this distinction for two reasons. First, I want to underscore the advantageous characteristics of the insufficiently attended approach to public policy advocated by Harold Lasswell, who used the designation "policy sciences." Second, my sharp criticisms of the qualities of much work in the policy field are not targeted here on the more Lasswellian tendency, which has adopted the policy sciences label in its journal and in the name of its professional association. That said, I also want to distance myself from the arcane world of Lasswell scholarship, which debates exactly what he meant and intended in his often opaque writing, much of which, alas, remains out of print and difficult to find. Still useful for an overview of the policy literature is Nagel 1984.

symbolic assault on the once secure New Deal welfare state and its Great Society elaboration; changes to the contours of race relations and the social geography of poverty; shifts in the center of gravity of ideological debate about the respective roles of governments, markets, and communities; and a host of normative and instrumental issues at the heart of liberal theory concerning representation, citizenship, and diversity. Set beside these challenging subjects, Norris and Thompson's accumulation of case materials and Handler's screed against current policy trends provide disappointing reminders that the great bulk of extant work on public policy combines semitheoretical description with policy advocacy weakened by the absence of analytical bite.

The Politics of Welfare Reform, seeking "to understand the specific processes by which the current wave of welfare reform has grown" (p. 3) usefully can be read as emblematic of the strengths and weaknesses of the vast majority of only loosely theoretical policy studies. Chock full of germane information about how various states responded to the 1988 invitation to apply for waivers from federal rules, the book collects papers treating a cross-section of welfare policy history over the course of a single two- to three-year wave of reform in one western (California), three midwestern (Wisconsin, Michigan, and Ohio), and two eastern (New Jersey and Maryland) states. As cases situated inside a slack application of Eastonian systems analysis, these stories tell sad tales about insufficient organizational capacity at the state level, the absence of prospective analysis and poor planning by policy elites, and the short-term political horizons of politicians whose courtship of opinion failed to challenge wrong-headed features of conventional wisdom about race and poverty. Perhaps the most coherent finding is that devolutionist welfare reform has been concerned rather more with ideology and political jockeying than with addressing poverty.

What to do? Norris and Thompson direct their advice to policymakers, offering only a series of anodyne pleas: "Turn down the volume of ideological debate"; "employ elements of a more rational process"; "suspend ideology, moralism, and political agendas"; and propose "reforms which should address real problems" (pp. 236–7). Having rehearsed, with their collaborators, how just these hopes have been contravened, they offer no structural or agency-centered analyses of causes or mechanisms. They strike a legitimizing tone which opposes science to normative purpose. Overall, there is little theory, only a weak specification for the selection of cases, very limited comparison, and imprecise conclusions. The "principal lessons" of the concluding chapter are models of equivocation: "The politics of welfare reform in the states were remarkably similar and remarkably different.... Different actors and institutions with different relevant strengths played roles in the politics of welfare reform" (p. 229).

This kind of slack though well-intentioned brief for rational policy intervention is one of the main targets of Schram's robust critique of the limited scope and top-down empiricism characteristic of much policy analysis. His *Words of Welfare*, a collection of vigorous essays written over the course of a decade, scores many points. He discerningly excoriates the credulous qualities of neutered research once inserted into fields of stratification and power. He demands that students of policy come to grips with their pivotal role in the policy process. He insists they invert or at least extend their conventional research to focus attention on the effect of policy as seen from the bottom reaches of society.

Schram goes farther. He claims that policy research based on premises of rationality and systematic social science necessarily contributes to the perpetuation of suffering by reproducing the "economistic-therapeutic-managerial discourse" (p. 4) on which current policies depend. As an alternative, he combines modernist studies of ideology with more thoroughgoing anti-science postmodernist theories of signification to promote a different discourse as the basis for a self-critical social science of poverty capable of assisting, if not guiding, social change.

This project is characterized by three main flaws. First, it commits the sin of overaggregation. "Rationality" becomes such a broad covering term that it encompasses virtually every type of policy analysis. Any differences that exist, say, between Charles Murray, who has advocated the complete elimination of federal income supports, and William Julius Wilson, who advances a social democratic program geared to problems of ghetto disemployment (Murry 1984, Wilson 1996), disappear in Schram's discussion, which places both scholars inside an undifferentiated diatribe against "neutered analysis for a managerial state" (p. 9). Second, despite postmodernism's usual disdain for polarities, the book advances a simple distinction between the role of "critic" and "scientist," as if a normatively engaged critical social science other than the discursive one proposed by Schram necessarily is an oxymoron. Third, Schram fails to examine reflexively the limits of his own preferred way of working or to explain how a focus on signification, buttressed, as in the substantive essays in the text, by an episodic engagement with data, effectively can advance either the goals of systematic understanding or social change.

By contrast, Handler's refreshingly direct intervention in debates about AFDC possesses the merit of clearly harnessing argumentation to purpose. Unlike Norris and Thompson, Handler understands that ideology, moralism, and politics cannot be wished away in the name of an abstract and agentless rationality. Unlike Schram, he does not weigh down his intervention with the baggage of a semisystematic postmodernism. Recent welfare reform proposals, he argues, blame and bruise the poor. However pitiful AFDC has been, its demise is even more deleterious. The arguments favoring abolition do not hold up; worse, they are partial and pernicious.

As an urgent policy brief shifting debate from the behavioral flaws of the poor to the labor-market causes of their poverty, this is more than a creditable performance. Its virtues, however, go hand in hand with defects it shares with other work of this kind, irrespec-

tive of ideological proclivity; for Handler puts on display a genre regularly found in policy writing of all persuasions, the policy prospectus that marshals only the evidence it likes. Handler is right to underscore how much public and elite misinformation there is to correct. Welfare mothers are not all African-American or very young; most do not linger as recipients; public provision has been meager; insecure job markets are an important part of the story. Yet, the divide he elaborates entirely separating poverty from personal mores is not convincing. He downplays the fraction of long-term recipients of AFDC or the disproportion of African-Americans who have stayed on the rolls more than seven years. Full of such elisions, the text unnecessarily relinquishes some of its credibility.

Welcome nonetheless as an antidote to conventional wisdom, Handler's advocacy is unsatisfactory as policy analysis. History, ranging from England's Statute of Laborers of 1349 to President Clinton's welfare proposals, is gleaned to attest that, inside the triad of capitalism, wage labor, and poverty, welfare policy has focused for more than five centuries on individual rather than systemic flaws. The past is reduced by this generality. Handler's history contains no variation or institutional particularity. It lacks a repertoire of choice. Analytically, it seems to contradict the purpose of the book itself. Why rail against the inevitable? The book's causal story—a simple functionalism in which symbolic needs are deployed as cause—likewise is unyielding, because it fails to specify mechanisms, analyze politics, or indicate points of likely intervention.

Thus, for all their differences, the flat rationalism of Norris and Thompson, the heretical postmodernism of Schram, and the semisystematic policy promotion of Handler share common features: a hortatory tone; a reductionist, instrumental application of history; and a lack of attention to the methods and contributions of social science. If critical policy analysis were to depend on the moves these scholars make, then it would be a nonstarter, since they demand decisions between symbolism and substance, politics and policy, and criticism and science which, if made, would leave inquiry misshapen and practical purpose unsecured.

HISTORY AND INSTITUTIONS AS REMEDY

Motivated by a policy literature on poverty gone "stale" (p. 18), Michael Katz, a historian who has written a large body of first-rate work on cities, schooling, poverty, and race, much of it influenced by the social sciences, tries out a type of writing rarely validated by the academy: a combination of intellectual self-reflection tethered to autobiography, historical argument, and the analysis of policy. The result is an elegant and engaging, yet elusive, book. Providing a rich text for those of us interested in the sociology of knowledge, Katz probes how his own experiences and motivations have galvanized and shaped his fertile scholarship and directs attention to shifts in standards, language, and expectation that occur when scholars try to influence policy.

Deliberately abjuring prescription, he resonantly explores links between education and social policy, the conceptual faces of the term *underclass*, the ways central city inequality intensifies more widespread trends, the role of ideas in policy development, the contested distinction at the heart of liberal theory between public and private spheres, and the means by which historical time and forms of narrative are deployed in policy debates. This plenteous brew, like Schram's, is incisively critical of most policy studies of poverty. Katz, however, is far more judicious and fair minded; as in his appreciation of Wilson's work on the social and economic isolation of the ghetto, he is rather more interested in engaging the best policy analyses we possess than in dismissing them as necessarily implicated in the status quo.

Not surprisingly, Katz takes history very seriously. He helps us see how regardful attention to the past and the elongation of the time span of policy research can puncture myths and enrich contemporary analysis. History appears in *Improving Poor People* to make evident similarities and variations in distinct policy spheres over time and to observe that some policy prescriptions thought to be new in fact are not. Paradoxically, it is here, with Katz playing to his strength as a policy historian, that the book becomes most intellectually reserved. Its powerful challenge to dominant ideas focusing on improving the poor rather than on their circumstances (without losing sight of the poor as complicit in shaping their own lives) and to the deficiencies of historyless policy studies disappointingly stops short of generating a historical imagination constitutive of policy analysis.

Katz abjures the chance to make history causal by declining to explore how sequencing and path dependency might be taken seriously as operative ideas. The text's narratives are chock full of chances to do just this, but they are missed. He also draws back from comparing situations in which continuity has proved the rule (as in elite and mass representations of the poor as unworthy), as distinct from those marked by significant change (the discursive racialization of welfare is a rather recent phenomenon). Likewise, he shrinks from deploying the conceptual tools elaborated by political scientists to deal with the role of ideas in politics and policy, a theme central to his book (Goldstein 1993, Jacobsen 1997, Sikkink 1991, Weir 1992). Furthermore, the text, especially its brilliant analysis of decentralized school reform in Chicago, forgoes the opportunity to heed systematically how institutions shape human cognition and dispositions to form solidary groups and cooperate by conferring identities, polarizing, excluding, and assigning properties to categories (Douglas 1986, Wildavsky 1987). In short, Katz's excellent policy histories are incompletely historical and insufficiently institutional.

These are infractions certainly not committed by Kenneth Finegold or Theda Skocpol. By showing and telling, they strongly advance the case for a historical-institutionalist approach to social policy and the welfare state and, more broadly, to American political

development. *Social Policy in the United States*[2] compiles Skocpol's essays written from the late 1980s through the early 1990s at the moment she was bringing her landmark *Protecting Soldiers and Mothers* (1992) to publication. Though hardly alone in advancing historical-institutionalism, Skocpol has convoked this school's most impressive program of policy research for nearly two decades; her substantive achievements and programmatic counsel thus can be read as archetypal.

Although Skocpol asserts that this course of research can account for present-day social policy and help set agendas for future initiatives, the core of her essays lies less in the present than in characterizing past outcomes, including key traits of the New Deal, social policy during World War II, and postwar employment policy. Uneasy with the work of "moralists" and "technocrats" who lack historical and political sensibilities, she quests to discover "reliable insights into the political constraints and possibilities for making and remaking American public policies at any given historical juncture, including the present," by focusing on the interplay among institutions, conflicts, and alliances over time to investigate the "limits and possibilities in the *politics* of U.S. social policy making by taking a long historical view" (pp. 5–6). A secondary purpose secured via a comparison of policy episodes is the production of a shaded account of American distinctiveness, persuasively forswearing all-or-nothing notions of exceptionalism based on value continuities, that can account for variations under such comparable conditions as mobilization for total war. A key feature of her analysis is its focus on the significant influence of history and on how prior policy outcomes and their cognate institutional arrangements constrain the present and the future (p. 7).

These resonant strengths can be found in many of *Social Policy's* essays, including an excellent paper written with Gretchen Ritter, "Gender and the Origins of Modern Social Policies in Britain and the United States," and my favorite in the volume, an essay coauthored with John Ikenberry on social provision during World War II. Ikenberry and Skocpol want to know why outcomes concerned with planning and employment policy differed in these two cases; that is, why war and mobilization strengthened the social democratic impulse in the British case but limited it in the American. They proceed within an explanatory frame they summarize this way:

> Wars are not fought by "the state" in the abstract, but by particular states with different institutional structures, prior policies, and contending political coalitions. Preexisting institutional structures and political coalitions influence the choices national leaders make about how to plan for the postwar period and how to mobilize the nation for war. The exigencies of war and the mode of mobilization reinforce some political forces and undermine others, in part by changing the agenda of problems discussed by the nation's politicians and in part by changing the goals and capacities of politically active groups (p. 190).

Likewise, Finegold and Skocpol's long-awaited comparative historical study of the short-lived and in many respects unsuccessful National Recovery Administration (NRA) and the durable, instrumentally capable Agricultural Adjustment Administration (AAA) hunts within this orientation and displays its value-adding power. Elaborating on articles they first published in 1982, *State and Party in America's New Deal* uses a mainly structural-institutionalist research design focusing on bureaucracy and state capacity linked to a coalitional analysis of the Democratic Party to parse the New Deal. (Finegold 1982, Skocpol and Finegold 1982). Finegold and Skocpol show that Roosevelt's policies varied with respect to institutional design, governmental capability, and terms of linkage with civil society. They argue that his policy proposals, though advanced under common conditions, did not share conjoint fates. These varied not because of their intrinsic merit but as results of the lineage and arrangement of the circumstances in which they were propounded.

Set alongside the usual sorts of social policy studies or even the best work on policy by historians, the gains they make are pretty clear. History and institutions, treated configurationally rather than abstractly, are rendered integral and causal. The analytical space between thick historical chronicles and timeless models is filled to good and telling effect. Certainly, Finegold and Skocpol advance our understanding of the heterogeneity of the early New Deal and the character of America's welfare state far better than most students of public policy have been able to grapple with welfare reform.

LIMITS AND LESSONS

No one who reads Finegold and Skocpol's provocative and well-executed studies of New Deal industrial and agricultural policy or examines Skocpol's artfully honed disquisitions on social policy can fail to admire their craft, depth of research, or vivid originality. Why not stop with this warm appreciation and end with a call for more scholarship of this kind in political science and policy studies?

I have two main reservations. The first concerns the conflation in their work of an *approach* to the design of research with *tools* for a research design. Finegold and Skocpol counsel a reduction in levels of abstraction, preferring plural words (states, policies, institutions, coalitions) to singular concepts. They insist on the funneling role of past decisions and institutional products. They wager on times of upheaval. They venture the importance of parties and coalitions. I have no principled reason to oppose these judgments, but they raise as many questions as they answer. Finegold and Skocpol's advice would be more compelling if accompanied by an appreciation that the choices they sanction mask others about which they offer no counsel. Research at any level of abstraction, after all, requires what Jon Elster (1989) has denominated as nuts and

[2] This volume was published in Princeton Studies in American Politics: Historical, International, and Comparative Perspectives, a series I coedit with Theda Skocpol and Martin Shefter.

bolts for the social sciences, and these can take many forms. Concreteness, too, is not just a single good thing. After all, nominalist and notionally "abstract" issues like collective action problems entailed by nonvoting are just as "concrete" as realist structures like multiparty or two-party systems.

If, moreover, empirical particularity is to be favored over conceptual generality (I, too, would make this choice if forced), then how, if at all, should more extensive concepts (or concepts linked in models of various kinds) be used to interrogate specific historical constellations? Surely, this is not an either/or matter, for absent such a linkage, case research hazards becoming an ad hoc art, risking makeshift redescription. If Finegold and Skocpol would rather err, as would I, on the side of historical specificity and the integrity of cases, then how should these instances be constructed to make them potentially deployable as units for comparative study (Katznelson 1997a, 1997b)? The scrutiny of past policies plausibly (I think manifestly) is important; but how might history be searched? By way of which mechanisms—branching models, learning theory, or other possibilities? If there is special merit to a focus on times distinguished by high uncertainty like the starkest parts of the Great Depression or the period of total mobilization during World War II, then what, if any, theory of historical periodization should underpin this selection? If the substance of individual and group identities and preferences necessarily varies by condition and occurrence, then how should their formation be studied to undergird both the macro- and microanalyses they recommend? How does the declaration that the past acts as a straightjacket on structures and actors square with the suspension of constraints often associated with times of emergency? Why is a focus on public administration, state capacity, and political parties inherently superior to studies of class mobilization, Congress, or Supreme Court decisions, to take other familiar (but here criticized) centers of attention? By overlooking such questions, Finegold and Skocpol undercut their intention to demonstrate historical institutionalism's capabilities.

Moreover, if historical institutionalism is to be more than a category in a scholarly typology but, rather, a systematic method for work in policy studies and political science, there simply is no good reason it should set aside research gear it might deploy to further its own purposes, provided the use of these tools is disciplined by rules of use historical institutionalists confidently establish. Instead, and this is my second reservation, Finegold and Skocpol take an entirely different tack. Rather than treat the existence of a theoretical and analytical tableau in the social sciences as an opportunity to sharpen and deepen the research capacities of historical institutionalism, they declare instead it is the superior *alternative*. Making social science a contest between clusters of schools rather than a form of knowledge possessing an ensemble of opportunities to guide analysis, they recurrently ask the reader to choose their superincumbent candidate, which, they aver, can cope with complexity on its own, thank you very much, without any help from its putative friends.

Both *Social Policy in the United States* and *State and Party in America's New Deal* are organized in comparable ways to "test" the capacities of alternate ways of working. "State Formation and Social Policy in the United States," the essay Skocpol has chosen to open her volume of collected essays, explicitly contrasts her research program to "existing theories"—schools she labels logic of industrialism, national values, welfare capitalism, and political class struggle—each of which "offers insights but falls short of offering *fully* satisfactory explanations of... historical phases and policy patterns" (p. 15; emphasis added). In the book written with Finegold, the list of competitors to historical institutionalism shifts from these mainly substantive categories to the theoretical ones of pluralism, elitism, Marxism, and rational choice. Both books, especially the latter, measure historical institutionalism against these options.

Finegold and Skocpol perform this task in the following manner. Their comparative study of the NRA and AAA identifies a double-sided dependent variable concerned with the origins and content of these programs. Both, of course, represented unprecedented delegations of power to the executive branch to plan, regulate, and coordinate large chunks of the economy previously organized by the price system. They differed, however, in key features of sponsorship and in their patterns of development. The authors ask (p. 23): "Why did the national government enact programs favored by organized industry and rejected by organized agriculture?" "Why did the industrial program fail" while stimulating class conflict, whereas "the agricultural program [succeeded] for commercial agriculture" while reinforcing "class domination in agriculture?"[3] To account for these diverse results, Finegold and Skocpol place "states and political parties at the center of analysis. The main variables employed in our analysis of the state are autonomy and capacity. The main variables employed in our analysis of parties are alignments and strategies" (pp. 23–4). They then measure up this mode of inquiry against its competitors on the basis of "fit," thus adopting, without public reflection, standard positivist criteria for testing a theory. While conceding that both the "NRA and the AAA are consistent with the most general premises of pluralist, elitist, Marxist, and rational choice theories of politics," they set these "big, abstract, and complex" candidates aside as insufficient because each "is inconsistent with specific aspects of the origins, implementation, and consequences" of the two programs (p. 218); but in this respect as well, they are theoretically reserved, leaving to the reader to speculate how they might want to distinguish between the abstract formulations they do not like and the "nuts and bolts" tools which can be

[3] Note that the criteria of success are not in the least normative; rather, without argument, they seem to be exclusively those of instrumental rationality. I have raised questions about this approach to stateness elsewhere (Katznelson and Pietrykowski 1991, Katznelson 1993).

found inside such scholarship but which are not consistent from one type to another. What they do argue, in contrast to literature they do not much like, is that their work enjoys the advantage of a close accord between its objects of analysis and the causal narratives they develop, based on an appreciation for discrete institutional textures.

Given the interpenetration of their explanandum and explanans, however, this trial only could have produced the result it confirms, especially given the way Finegold and Skocpol characterize their opponents, often coming close to caricature. Consider elitism and pluralism. They announce that these depictions of social pressure and mechanisms of representation regard the social basis of politics as fixed. Always? Necessarily? Neither really is a credible claim. Consider Marxism, which they dismiss as functionalist and social determinist. Certainly, they appreciate that this tradition has been diverse, even historical and institutional, in ways far more nuanced than they represent and quite compatible in some of its variants with their own enterprise. Or consider rational choice, the main target of their ire, which they hold up for critical examination only when it wears its most aseptic and disembedded, and least institutionalist, garb. But, surely, they know just how curious some rational choice scholars have become about the origins and constraints of institutions and how committed many are to careful specification of cases, actors, and preferences in order to designate how and why people, understood as interdependent actors, make choices *inside* particular historical and institutional settings.

The victory Finegold and Skocpol produce is Pyrrhic. By flattening other orientations and epistemologies, they fail to do justice to the power of their own way of working or show its endowments to best advantage. Expostulating this way, they demand too high a price for entry to historical institutionalism's house (implicitly insisting other theories be left behind as irremediably flawed), and they unduly limit the scope of inquiry. Their "victory" contracts the number of causal variables we might examine, including the social structural ones both elite theory and Marxism embrace. It privileges some features of liberal representation associated with the party system as opposed to others (congressional processes favored by many rational choice analysts or interest-group activity singled out by the pluralists) without clarifying whether this is a theoretically motivated selection (if so, why?) or is limited to the two time-bound cases on offer. If the latter, then the avowal that historical institutionalism wins by a knockout is vastly inflated; but the former never is addressed. It also leaves the issue of microfoundations so important to rational choice entirely too open. To be sure, coalitions inside political parties regularly make an appearance in these texts, but in an untheorized way. Of course, there is more than one way to think about issues of agency; which do they like better, and why?

I prefer both more modesty and greater assertiveness. More modesty because the power of a given theory, we do well to remember, varies not just from case to case or from one object of analysis to another, but can vary even inside the moments of cases under investigation. Challenging substantive puzzles always are complex; no one set of "nuts and bolts" ever can claim exclusive purchase on them. Indeed, neither of the key words in historical institutionalism's lexicon—*history* or *institution*—implies a single (unitary or compound) theory or subject matter. "Institution" gains its power as a concept to probe the infinitely intricate history of humankind by virtue of its junctional location at the intersection of structure and agency and of past and present. If it is one of historical institutionalism's goals to embrace complexity, then why, a priori, should it deny itself access to creative confrontations and substantive syntheses or work without a full kitbag of tools, including the fine-grained, systematic, and dynamic understanding rational choice offers with respect to the endogenous generation of institutions?

But I also prefer greater assertiveness. Historical institutionalism is right to claim a privileged, though not exclusive, ability to bind structure and agency to contingency.[4] There is nothing wrong with wagering strongly on a particular approach, provided it remains open and self-reflective. Resolute and insistent, historical institutionalism should engage other theoretical and analytic approaches on *its* terms, assuredly making selections disciplined by its own purposes as it searches for criteria of appraisal which stand in the challenging location between the too-constraining jacket of strict positivist theory testing and the too-permissive art form of postmodernism (Laudan 1996). With respect to microfoundations, for example, it should advance situated understandings of rationality, an approach that does not so much compete with as incorporate the best features not only of rational choice (especially its focus on strategic settings and mutually dependent interaction) but also of bounded rationality (especially its realist sense of constraints on individuals not only because of their cognitive limits as decision makers but also as a consequence of restrictions imposed by structural situations), while insisting on the conditions of this encounter. Such a course would emphasize that rationality only can be inferred over time; that particular versions make sense only in specific normative and institutional settings which are culturally and structurally thick, not just strategically lean; and that it must link the internal and external environments of persons. A historically institutionalist approach to rationality would be agnostic about whether there really are choices in all "choice" situations and would locate behavior within the rules and practices that constitute regimes. It would have to make room for culture and incorporate identities ordinarily seen as nonrational. And it would refuse simple oppositions between rational and norm-guided action.

Following such a course, historical institutionalism might well teach students of policy and political science to dance more gracefully, beginning, perhaps, by retracing forgotten steps in the genealogy of policy

[4] I owe this formulation to my colleague Charles Cameron's presentation at a Columbia University workshop on American Political Development, which we share in directing.

studies. I particularly have in mind the epigrammatic advocacy Harold Lasswell deployed more than four decades ago when he helped convene a who's who of American social science, including Kenneth Arrow, Edward Shils, Paul Lazarsfeld, Robert Merton, and Clyde Kluckhohn, to review and codify gains by the policy sciences during and after World War II "in order to cope with the gigantic crises of our time" (Lerner and Lasswell 1951, 7).

Lasswell's agenda-setting essay, "The Policy Orientation," opened the collection of papers these scholars produced. Substantively, he promoted a problem-centered focus. Normatively, he endeavored to advance the prospects of liberal democracy. Analytically, he sought to join two levels of analysis: methods focusing on choice, especially decision theory drawn both from microeconomics and cognitive psychology, deployed *in tandem* with an appreciation of and deep concern for "the awareness of time" and context ("the key terms which are used in the policy sciences refer to meanings, and contexts of meaning are changeable") and for "the institutional pattern *from* which we are moving and the pattern *toward* which we are going," an interest he herded under the umbrella he called "developmental constructs." Policy sciences braiding these elements, Lasswell argued, could improve "the rationality of the policy process" and become "relevant to the policy problems of a given period." He defined these quandaries as issues on the scale of depression, war, and America's racial divide, problems more fundamental than mere "topical issues of the moment." (Lerner and Lasswell 1951, 3–15).

This attractive program has been accomplished only in part. A good many students of policy, including political scientists of diverse stripes, have been happy to use the variants of decision theory to work on specific problems, but they have tended to neglect Lasswell's anxious normativity or his particular version of historical institutionalism and the manner in which he sought to link these theoretical and macroscopic concerns to agency-centered microanalysis. The time is ripe, I believe, not only to secure Lasswell's prospectus for studies of policy but also to deploy the elements he advocated to advance a richer, more appealing political science. The new historical institutionalism must be central to these advances. Its contribution will dissipate, however, if it seeks a role that is too autonomous or adverse.

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BOOK REVIEWS

Political Theory

Left and Right: The Significance of a Political Distinction. By Noberto Bobbio. Chicago: University of Chicago Press. 1997. 148p. $34.95 cloth, $14.95 paper.

Thomas W. Gold, *New School for Social Research*

This is an important book for the field of political science and of particular interest to those whose work lies in the area of political ideology, European party politics, or contemporary Italian politics. Written by one of Italy's greatest living political philosophers, this compact, clearly written monograph makes a major contribution to one of the most crucial debates facing contemporary European politics.

In the first part of the book, Bobbio argues against the prevailing assumptions that the terms Left and Right have lost meaning in the late twentieth century. Some of the reasons that are often used to negate the distinction between Left and Right are the inadequacy of the polar division of politics in increasingly complex societies and the development of movements during the last few decades which are hard to place on either the Left or Right. But the impetus to smooth over the distinction, Bobbio argues, comes mainly from the Left itself. Groups and movements which claimed to be left-wing when the "wind of history appeared to be blowing in their direction" are now questioning the meaning of the division. He contends that by negating the difference, the Left is trying to recuperate some of its lost power: "In a universe in which the two opposing parts are interdependent, in the sense that existence of one presupposes the existence of the other, the only way to invalidate the adversary is to invalidate oneself" (p. 14).

Using a "conceptual" method with selective historical references drawn mainly from the Italian case. Bobbio argues that the continuing necessity for the distinction rests in the dyadic structure of politics. While the terms Left and Right, which go back as far as the French Revolution, are purely accidental, the dichotomous structure of politics is not. Political conflict, like other aspects of human relations, naturally falls into a dichotomous logic, creating "seemingly unnatural coalitions and alliances, both in international relations and between parties within a single state" (p. 32). On the basis of this "axiological either/or" structure, everything within the political universe belongs to one of two parts, leaving no room for a third alternative. Within this context, the subjective designation of one side as "good" automatically assumes the existence of the another side as "bad."

Bobbio further attempts to define the substance of the Left and Right. Left and Right are not "intrinsic qualities of a political universe" but terms located within a particular "political space." The two sides of the political spectrum "are not words which designate immutable meanings, but can signify different things in different times and situations" (p. 56). The "criterion," however, which actually governs the distinction in Bobbio's view is the "attitude of real people in society to the idea of equality" (p. 60); in other words, the difference between a "horizontal or egalitarian perception of society and a vertical or inegalitarian perception of society" (p. 58). He maintains that the Left and leftist movements in history have been primarily concerned with the fight for greater equality in society, while the Right has traditionally been tolerant of inequality and divisions within the social order. Bobbio further deconstructs the terms Left and Right to a conflict between "fundamental choices," that is, between those who feel (on the Left) that inequality in society is socially created and hence able to be eradicated, and those (on the Right) who feel that inequality is a manifestation of natural forces and therefore inevitable (p. 67). While the Right emphasizes that which makes people different and unequal, the Left searches for what unites people and does so by encouraging policies which reduce inequalities.

Although Bobbio convincingly points out the validity of the distinction between Left and Right, his equation of the two terms with equality and inequality, respectively, conflicts with a more complex reality. In the first place, he is quite generous to the Left by identifying it almost unconditionally with the notion of equality, particularly when you take into account the changes in the programs of European Left parties during the last couple of decades which have brought them closer not only to the Right but also to tolerating different forms of inequalities. This oversight may have something to do with the timing of Bobbio's book, which was published shortly before the formation of the first Left-led government in Italian postwar history and the eventual implementation of the most severe budget-cutting measures in Italian postwar history. Bobbio's conceptual view of politics has a tendency to overlook the more instrumental nature of political behavior. The Left's image and identity continue to change as it faces a shifting social base and will, unfortunately, tolerate inequality if it means more votes.

Furthermore, the Left, the main proponent of the welfare state, is itself responsible for the creation of new forms of inequalities today. In an era of mass migrations in Europe, for example, the dividing line between the haves and the have nots is located between those who are citizens and noncitizens, or between those who receive generous social benefits in education, state jobs, medical care, and so on, and those who do not. In other words, while the welfare state has helped create a more egalitarian society for its own citizens, it has simultaneously set the basis for new kinds of inequalities in relation to new immigrants.

In addition, Bobbio spends a great deal of time examining the egalitarian roots of the Left, but he does not provide the same attention to the Right and its different varieties (from the neoliberal Right to the more statist, Gaulist Right). The Right in this book remains an unexplored residual category. Bobbio further does not analyze the full effect of how current relevant issues like federalism have been adopted by both the Left (in an attempt to bring power closer to the people) and the Right (as a means to reduce the national state's capacity to create a more egalitarian society).

In sum, Bobbio's analysis of the Left has normative implications which detract from his attempt to provide a value-free approach to the distinction between the Left and Right. In spite of this flaw, Bobbio successfully returns the heuristic value of the distinction between Left and Right to the study of politics. As he notes, the terms are still used widely today in journalism, academia, and public discourse and continue to provide the basis of political tendencies and trajectories. His book aptly demonstrates that even though global events—such as the fall of the Berlin Wall, the failure of the socialist republics in Eastern Europe, and the general "liberal" trend in Europe—have brought about a crisis within the Left, the distinction between Left and Right still is of critical importance.

Dancing in Chains: Narrative and Memory in Political Theory. By Joshua F. Dienstag. Stanford, CA: Stanford University Press, 1997. 268p. $45.00 cloth, $15.95 paper.

Jonathan N. Badger, *St. John's College*

One of the many paradoxes in Nietzsche is his insistence on the primary status of historical horizons for human self-

understanding coupled with his prescription for the deliberate construction of those horizons. How can we reliably or confidently destroy horizons and make new ones if we simultaneously depend on them. It is rather like fish trying to build an aquarium. A solution may lie in the form of some transcendent standard and guide—a standard beyond both old and new horizons—but this is exactly what Nietzsche cannot allow. Such a standard and perspective would undermine the view that all life occurs inside horizons.

This simple statement of the Nietzschean problematic is given a rich and beautiful expression in Joshua Foa Dienstag's *Dancing in Chains: Narrative and Memory in Political Theory*. Unfortunately, a solution to this paradox seems to elude Dienstag, just as it *seems* to elude Nietzsche. It does not really elude Nietzsche—he sees the solution perfectly well. Rather, he rejoices in the openness of the paradox. He deliberately banishes the solution. It is not clear that Dienstag wishes to settle for the paradox or that he sees that he is stuck in it.

Dancing in Chains develops a thesis which holds that most if not all texts of political theory are profoundly narrative in nature, containing plots, characters, and roles. While many texts masquerade as treatises of abstract reason, in fact they are often carefully crafted dramas. His case studies are Nietzsche's *On the Genealogy of Morals*, Hegel's *Philosophy of History* and *Aesthetics*, and (perhaps the most interesting and innovative example of this methodology) Locke's *Second Treatise of Government*. These "political narratives," Dienstag explains, are structured stories that contain revised histories which account for a dramatic present and point the way to a particular future. Political philosophy is about time.

This is not an old style historicism. Indeed, Hegel and Heidegger are hit rather hard in these pages. Their narratives *reconcile* the past and make the present utterly dependent upon it; but, says Dienstag, there is no future in their narratives. Hegel's "temple of memory" reverently holds the past up as the substance of the present. The story is about the present, which is the fruition of history. The present is the end. The narrative is closed. It cannot point to a future. "Like Narcissus, we lose our future in admiration of our present self-image" (p. 203). For Heidegger (who builds upon an Hegelian historicism), human beings are "thrown" into their lives. They have no control over their time. Their past is fixed, and thus their future is likewise given.

Not so for Locke and Nietzsche, whose narratives *redeem* the past, says Dienstag. Locke offers a revised history which points to a future of liberal self-rule. Locke found himself in a particular history but was free (enough) to change it. The Biblical account of history which supported monarchical rule was challenged by Locke's new history of labor, commerce, property, and contracts. Dienstag shows that Locke thereby points the way to a new and different future. Locke's "characters," which include his readers, live in an alternate narrative from the one given by the tradition of their time and place. Thus they have access to an alternate future. The story he tells is open, unfinished; the characters walk out of the text's present and into its future.

Similarly, and in a more powerful and profound manner, Dienstag suggests, Nietzsche transforms history and thereby the future. Nietzsche is more explicit in his use of time: His aim in the *Genealogy* is none other than an account for the development of memory and the historical consciousness itself, the creation of the very nature of human life. Man is the beast which remembers. Creating memory (rewriting history) is a crucial element of shaping the future.

"Memory," "history," "time," and "narrative" come to mean much the same thing in Dienstag's vernacular. They become the elements, for him, of philosophy itself, replacing the conventional language of reason and ideas as the motion and substance of philosophy. He is careful to point out that he does not intend to replace philosophy as rational account with philosophy as narrative, only to offer it as an alternative to be considered. This seems reasonable, and his book is surely fruitful and wonderfully explicative of the texts under consideration. He demonstrates that his method is a useful tool in textual exegesis. Understanding the narrative nature of a text is indeed a necessary component of an authentic encounter with the text, and here Dienstag persuades. Thus, his book discloses something very important about the texts under examination. (The illumination of the narrative of Locke's work is particularly helpful.) But he seems to err in setting narrative alongside "reason" as the competing activity of philosophy. He does not seem to allow that narrative is a means which operates at the behest of reason, or of whatever is at the core of philosophy. Narrative cannot be the core.

Dienstag obeys this principle even as he denies it. His explication of Locke (his disclosure of narrative as the form), for example, must have its source beyond Locke's narrative but nevertheless within Locke's intent. In other words, Dienstag's understanding of Locke's narrative cannot be part of the narrative, but it may be part of—and consistent with—Locke's intent. Dienstag shows us Locke's aim (a new future) but is unwilling to distinguish this aim from Locke's narrative.

The desirability and formulation of the future to which a text points can be understandable only by means of rational account or, at the very least, by means of a mixture of reason and intuition. The narratives of great works are too complex to reduce to past-present-future formulations. Something beyond the narrative must guide us in our reading of the narrative. While our *following* the narrative is very important, it cannot rival our *thinking about* the narrative, which is inevitably *beyond* narrative.

Reason (or a part of reason), or some subrational impulse, is the source of the aim of the philosopher, and it is the thinking through of these aims that is the activity of political philosophy. Narrative at most is the means of presenting the aim. It is either a way of dramatizing the thoughts of the philosopher or a kind of subterfuge which seduces foot-soldiers into its scheme. We might also consider that the narrative is a way of making the aim accessible and desirable to the nonphilosopher.

What narrative is *not* is the source of the aim. If it were, then the philosopher would be a mere character in his narrative, not the author of it. To suggest that narrative is the activity of philosophy is to suggest that history produces the ideas which the philosopher believes to be her own. It is to contain the philosopher within the narrative which he believes he creates. Yet, if the philosopher does in fact create the narrative, then she does so for some reason, and this reason would necessarily be beyond the narrative to which it gives birth.

One can argue that history is in fact the author of ideas and that philosophers are not able to think independently of their time and place. But Dienstag goes out of his way to say this is not his view. He attempts to walk a difficult line: Ideas are not merely the product of history, but they may, finally, be expressible only in historical terms. On the one hand, if he is right, then philosophers (and students of philosophy) are caught in a very strange paradox—stranger than Dienstag admits. On the other hand, if there is serious thinking which is not bound by time and place, then we are obliged to look beyond the narrative, even as we attend to the narrative—the temporal motion—of a text of political theory. Dienstag does so, even if he resists admitting it.

Hypocrisy and Integrity: Machiavelli, Rousseau, and the Ethics of Politics. By Ruth W. Grant. Chicago: University of Chicago Press, 1997. 201p. $22.50.

Ronald J. Terchek, *University of Maryland*

Ruth Grant has written an exciting and provocative work reassessing the role of hypocrisy and integrity in politics. Relying primarily on Rousseau but also reaching to Machiavelli and some of Molière's plays, she weaves together the complex and often competing goals of politics. For her, politics is not about discovering and then implementing a perfect system of justice, making a coherent community, or finding and then applying a standard of goodness. The hard realities of her version of politics are power, plurality, and dependency, realities that do not disappear when people gain their freedom.

Grant is neither a crude Machiavelli nor Mandeville when she writes about hypocrisy but a morally sensitive, humane writer who sees harm in pursuing an absolutist moral position in politics. To make her case, she contrasts integrity with authenticity. Authenticity often leads to a perfectionism that is intolerant of failure and accepts cruelty as a regrettable but necessary cost for honesty. Moreover, Grant finds that there is nothing intrinsically laudable about authenticity when the authentic person pursues goals that are self-regarding and damaging to others.

Integrity assumes that individuals have moral standards and act on them, and one of their standards has to do with how their conduct affects others. Sometimes this means that persons of integrity conceal their true feelings when honest expressions cause harm or embarrassment to others; at such times they display civility and manners. As Grant argues, there are times when it is preferable to offer others a polite compliment rather than express our authentic displeasure with them.

One reason Grant argues for a minimal hypocrisy is that it introduces flexibility into politics. Her argument is much richer than this summary view allows, however. She finds that politics is concerned with how a plurality of people live together, and this means, to Grant, how people depend on one another. Dependency can take many forms, as with the slave and master, but republican dependency is expected to occur among political equals. Republican citizens minimally depend on one another to vitalize and, if necessary, defend the free institutions of their republic, which assures them of their liberty. Such dependency, however, particularly when it involves sacrifice, may strike many citizens as counterintuitive. Grant's founding legislators as provided by Machiavelli and particularly Rousseau offer a myth that takes individuals away from their personal self-concerns and moves them into a shared community which teaches them that they have a stake in the well-being of their republic and that it can be maintained only through collective efforts. In a sense, the founding legislator provides citizens with materials to lead a principled life and act with integrity, and they do this through the founding myth.

As Grant sees matters, the founding myth is not meant to be transitional, eventually leading all citizens to authenticity. If all that people have is their authenticity and no standards, then interests become the subject matter of politics, which becomes the story of how winners are sorted out from losers. From a republican perspective, the ascendancy of interests introduces the politics of corruption. Corruption takes many forms, and today one of its manifestations comes with the decay of political language. For Machiavelli and Rousseau at least, political language should be about liberty and not about economics or management. When the latter two dominate, interests become primary, and the autonomy of citizens becomes secondary.

Grant's work addresses the modern preoccupation with individualism, something she wants to tame but hardly eliminate. She knows that a politics composed only of interests is shallow, and she seeks to move politics to a better terrain. Why should she offer hypocrisy as an alternative? First, she is self-consciously concerned about power and how it can be used and abused. For most liberals and communitarians, politics must be authentic and serve justice, rights, or community; from this perspective, politics occupies a secondary, not central, role. Second, Grant works with the observation that founding myths are fragile, and even when they are most robust they do not cancel out the interests people have in their own well-being. Grant's Machiavelli and Rousseau know this, and she wants to remind us of how they use hypocrisy politically to dampen the place of interests in politics.

There are, of course, some dangers in assigning hypocrisy a place in politics. It invites those with power to shave the truth and mislead for what they take to be a noble cause, which the public might not accept if they had the unvarnished truth. One thinks of many, in and out of the military, who supported the war in Viet Nam and who relied on hypocrisy to win popular support for the project.

Grant offers hypocrisy and integrity as alternatives to contemporary liberalism, which she finds is defective in important respects. Grant, who is one of the most perceptive readers of liberalism, sometimes uses the term loosely in this work. Two kinds of liberalism surface here. She sees the liberalism of the Enlightenment seeking to unmask everything that it considers to be dishonest and the liberalism of self-interests making politics rest on self-regard and depend on bargaining. Grant knows there is another liberalism, the moral liberalism of John Locke, which she discusses in her sensitive account in *John Locke's Liberalism* (1987), but it does not appear here. Moreover, she is too quick to link all liberals to political interests, something she finds with Adam Smith. What is ignored here is that Smith justifies a negative state because he does not believe it is possible for the Invisible Hand to sort out interests to produce a political equivalent of a just price he sees emerging in economic markets. Smith wants to keep politics free of interests.

Grant's work is to be highly recommended, offering a fresh reading of Rousseau and Machiavelli as well as presenting a penetrating analysis of hypocrisy and integrity. What is also appealing about this book are its views of politics as both aspirational and pragmatic. To credit it only with the former is to invite a perfectionism which is inattentive to the cruelty it often unwittingly spawns. To consider only the latter is to make politics exclusively the story of power. In inviting us to look at the indeterminacies of politics, Grant offers a way out from the perfectionism of many communitarians and modern liberals and the pragmatism of many pluralists. Grant's politics is never fixed; it depends on idealism but requires prudence; it accepts that hands sometimes become dirty when integrity is at stake; and it is concerned with many goods, not just one. In carrying out her project, Grant writes about the ironic place of hypocrisy and integrity in politics, and she contributes to a literature that seeks to speak to a politics fit for a free people who want to keep their freedom.

Endgames: Questions in Late Modern Political Thought. By John Gray. Cambridge: Blackwell Publishers (Polity Press), 1997. 212p. $54.95 cloth, $22.95 paper.

Harlan Wilson, *Oberlin College*

Much of John Gray's published work engages political theory with real political life. *Endgames* is no exception. This extremely varied collection of essays addresses such topics and themes as critique of New Right politics; prophecy of the end of Enlightenment; exposition of green theory; commentary on contemporary theorists; and analysis of the direction of British party politics. The occasional papers in this volume, most of them previously published, consist of scholarly articles, book reviews, and even a series of commentaries on current affairs in the *Guardian*. If the compilation is uneven and not quite coherent as a whole, it is nonetheless of consistent interest. Most essays are polemical and aimed at a broad audience.

Gray shows a keen sense of endings and beginnings—perhaps too keen. Writing just before the sweeping victory of the Labour Party and Tony Blair, he pronounces a death sentence on the Tories. This may be thought surprising on the part of a man associated with the Right in Britain. But Gray is a conservative without nostalgia as well as a liberal without Enlightenment. For him, the problem is neoliberalism and the capture of the Conservative Party by Thatcherite neoliberal ideology. This he sees as equally disastrous for the Conservatives and for Britain itself. Politically, neoliberalism destroyed the culture of deference and solidarity which allowed the Conservatives to adapt and govern. Indeed, Gray thinks, the present Labour Party, like the New Liberals of a century ago, now better represents authentic conservative values than do the Tories. Intellectually, the triumph of neoliberalism with its rationalist economic doctrines is a bankrupt abstract ideology that does great damage to any society foolish enough to be seduced by it.

Gray has shown himself a good prognosticator of electoral fortunes. His analysis predicted very well, and for many of the right reasons, the Blair victory and the changed shape of electoral politics in Britain. His prediction about the "strange death" (p. 1) of the Conservative Party, however, could be a bit too eager. Perhaps the Conservatives of 1997 are more like Labour of 1979, capable of eventual adaptation and recovery, or perhaps like the Conservatives of 1951, who adjusted themselves to the welfare state, rather than like the doomed Liberals of 1918.

There is a larger point here for Gray, namely, that neoliberalism, indeed the Enlightenment itself, has no future. The most profound "endgame" is the death of Westernizing universalism, of which neoliberalism is perhaps a last desperate gasp. Yet, here, too, one may wonder whether Gray's prediction of the death of neoliberalism, especially in the form of global capitalism and market expansion, is a bit premature. Given the seeming inexorability of global economic rationality with its associated technological regimes, it would seem that the end of both neoliberalism and Enlightenment, however damaging, may need to be postponed.

Gray's critique of neoliberalism as a doctrine is trenchant and powerful. Like Michael Oakeshott, whom Gray admires, he finds fault with the fact that it *is* a doctrine. In terms of its content, Gray emphasizes how neoliberal policies of unrestrained markets destroy local community and political sovereignty without any political accountability. He agrees with Margaret Thatcher on one point: he believes Thatcherism brought about great social and cultural, not only political, changes. Neoliberalism prizes mobility without security; destroys both human communities and the environment for the sake of an abstract Enlightenment doctrine of progress unrelated to human well-being; and hinders, rather than enhances, true individual autonomy. By failing to recognize limits to growth and progress, neoliberalism undermines the conditions that make stable and livable human communities possible.

This account is compelling but incomplete. Gray does not quite explain why neoliberalism was renewed in Britain or America or elsewhere, or how it could become politically effective in Britain at that historical juncture. He says that markets are parasitic on cultural traditions they cannot replenish but must destroy. Yet, somewhat akin to Edmund Burke in his *Reflections on the Revolution in France*, Gray does not explain why neoliberalism had such appeal to the middle class and even sections of the working class in Britain. As for American neoliberalism, Gray does not attempt to give a textured account, much less explanation. He caricatures American politics as if American political society were totally understandable by reading the Chicago school of economics, listening to Ronald Reagan, and watching "NYPD Blue." This is not a book to which one can go for subtle and nuanced foreign commentary on American politics.

Gray's treatment of market globalization is selective. It focuses on the ways in which global markets affect local culture, state sovereignty, and environmental risks, though without much consideration of how concentration of economic power, both within firms and in global systems, produce these outcomes. There is also little mention of the effects of global markets or of international free trade agreements on working-class security and empowerment. In any case, Gray grudgingly accepts the tendencies toward globalization. One may ask whether this means that global market expansion is not an endgame—whether, however damaging to culture and nature, it is the Enlightenment project with a future?

To his great credit Gray is one of a very few political theorists to address Green issues, as distinct from Green social movements or Green politics. He sees clearly that human relations with nature and the environment are not just parameters for politics, but themselves profoundly political. The connection he draws between traditional conservatism and elements of Green thought is original and persuasive. Drawing upon his previous critique of neoliberal antienvironmentalism in *Beyond the New Right* (1993), he demonstrates the connections between neoliberal hostility to human diversity and its utter disregard of environmental diversity and limits.

His treatment of Green theories, however, though sharply pointed, is not especially complex or nuanced. His characterizations of Left-Green thought and his attempts to refute socialist Green theory by reference to the failures of "actually existing socialism" in the Soviet Union are convincing only to believers. His depiction of mainstream Green theory as utopian is similarly overstated and perhaps shows a lack of familiarity with recent literature, much of it American, on environmental protection and democratic theory. Gray implicitly positions himself within Green theory as a neo-Malthusian, insisting on the need to recognize inherent limits and scarcities in nature and society alike. His neo-Malthusian argument for drastic population reduction omits the coercive measures that would be regrettably necessary to bring about this end.

Gray views the Enlightenment as wholly on the side of human exploitation of the natural environment. In particular, he problematizes the relation between Green theory and natural science. In this he departs from most neo-Malthusian

thinking and joins the company of ecofeminists, critical theorists, and postmodernists who express skepticism or even hostility toward natural science. (Somehow Darwinist perspectives, which he recommends, are not tarred with the scientific brush.) Gray seemingly is willing to go along with the project of undermining the authority of science in environmental matters, trusting that an ethical or even mystical notion of natural limits will somehow provide a basis for Green politics. Nevertheless, even though the sciences and technology are thoroughly implicated in the environmental crisis, it is hard to think that such issues as global climate change and acid rain can be fully understood or acted upon without reference to physical and biological discourse or evidence. Perhaps, too, for political as well as scientific reasons the undermining of the credibility of the natural sciences in environmental matters should not be undertaken lightly.

Finally, Gray's exploration of the late modern, post-Enlightenment world is strangely silent on issues of democracy and justice. One wishes that Gray had explored the meaning of democracy, that is, popular self-rule, in terms of the complex, multicultural world he projects. His disinterest in problems and dilemmas of democracy is especially notable in light of his sensitivity to the public costs of neoliberalism, which is incapable of conceiving of any meaningful kind of public life or democratic politics. Similarly, Gray writes as if class, differential power, and associated questions of social justice have no place in post-Tory British politics or in pluralist political cultures. Even granting the existence of essential "moral scarcity" (p. 42), does he think that aspirations for social justice will no longer matter in late modernity? Furthermore, neo-Malthusian environmental policies like population control would surely raise the most profound and serious questions of legitimacy and social justice.

Nevertheless, Gray's engaged approach to political theory in this volume at least allows us to think about such questions. This collection of papers succeeds in showing by example and illustration how theory can matter for ideology and practice, and how economy and polity matter for culture. If the analysis is often sweeping and incomplete, the book is nevertheless unfailingly stimulating and eminently worth reading.

The Political and Legal Philosophy of James Wilson 1742–1798. By Mark David Hall. Columbia: University of Missouri Press, 1997. 228p. $37.50.

George W. Carey, *Georgetown University*

Mark David Hall observes at the outset that "James Wilson is perhaps the most underrated founder" (p. 1). Many, if not most, familiar with Wilson's contributions to our founding and our political tradition would delete the "perhaps." As Hall notes in his opening chapter, Wilson signed both the Declaration of Independence and the Constitution, being one of only six men to enjoy this distinction. He intermittently represented Pennsylvania in the Congress under the Articles, always seeking ways to strengthen the feeble confederacy, and he provided the legal arguments for colonial self-government, anticipating by several years the positions of Adams and Jefferson and furnishing the theoretical framework for what was later to become the British Commonwealth. He was the first to articulate the general outlines and principles of the Philadelphia Constitution—later developed more fully in *The Federalist*, on the occasion of his famous "State House Yard Speech" (October 1788)—anticipating the major arguments that would arise in the ratification debates. He led the Federalist forces in the Pennsylvania ratifying convention, providing commentary on the nature of the Constitution that is still pertinent. He was long a highly prominent lawyer/politician in Pennsylvania, assuming the principal role in drafting the Pennsylvania Constitution of 1790; and, inter alia, he served as an associate justice on the first Supreme Court until his death in 1798, setting forth a conception of our federal union that predominates today.

The case for placing him in the front ranks of our founding fathers rests to a large degree, however, on other grounds. His education in Scotland, prior to his emigration to the colonies in 1765, came at the height of its "Enlightenment." At what point Wilson's formal education ended is still far from clear, but we do know that he was very familiar with the writings of leading Scottish Enlightenment thinkers, particularly Thomas Reid and Francis Hutcheson. We know, moreover, from his law lectures at the College of Philadelphia in the early 1790s that these readings and studies provided the basis for his development of a comprehensive and coherent theory concerning the nature of society and its origins, the sources of law and morality, as well as the meaning and importance of constitutionalism. Thus, Wilson more than any other founder possessed a systematic and relatively comprehensive moral and political philosophy that, in many instances, determined his stance on specific issues in the political arena. His unique status in this regard acquires special significance because he was so highly influential at the Philadelphia Convention—arguably more so than Madison—in shaping the form and substance of the Constitution.

Hall acknowledges that others have dealt with one or another aspect of Wilson's political thought and contributions. His avowed purpose, however, is a grander one, that is, to provide the first "comprehensive and systematic analysis of Wilson's political philosophy," while endeavoring to show how his "political theory influenced his concrete contributions to the creation of the American republic" (p. 1). The key chapters in this undertaking are those dealing with Wilson's understanding of the natural law, his moral epistemology, and his belief in popular sovereignty and majority rule, which rendered him "one of the foremost democrats of the founding era" (p. 90). As Hall ably demonstrates, these elements of Wilson's thought are interrelated. His belief in popular sovereignty, for instance, derives from his conviction that God is the author of "natural laws" that "may be known through different means, either moral sense, reason, or the Scriptures" (p. 42). He believed as well that individuals are endowed by God with a "moral sense" that enables them to apprehend the natural law and the "first principles of morality" (p. 74). Thus, as Hall points out, Wilson had faith in "majorities" to enact "laws in agreement with the natural law" (p. 90). And, consonant with his belief in majority rule, he also held "that the fundamental, underlying principle of government is the consent of the people" (p. 100).

Hall observes that controversy has arisen among scholars over whether Wilson is a "classical republican" or "Lockean liberal," the two major schools of thought that presumably informed American political thought during the founding era. Hall, however, does not believe Wilson "fits neatly into one category or the other" (p. 192). He differed from most classical republicans in opposing a "mixed regime" and in advocating a strong national government over an extensive territory. What is more, he looked with favor on the development of a commercial republic, an anathema to the classical republicans. At the same time, Hall notes, while Wilson shared many of Locke's views, particularly those relating to the state of nature and the social contract, "his natural law theory owed more to Scholasticism and Re-

formed Protestantism than it did to Locke," and his "moral epistemology" derived from "Scottish moral sense theory, not Locke's empirical epistemology" (p. 191). In sum, Hall finds that while Wilson "borrowed ideas from both the classical republicans and Locke," he "also relied heavily on the Christian natural law tradition, Reformed Protestantism, the Scottish Enlightenment, and continental jurists" (p. 192). This, of course, is further and substantial evidence that the classical republican/Lockean liberal paradigm is far too narrow for understanding the theoretical foundations of the Constitution.

Likewise, Hall believes that many close students of Wilson, perhaps bowing to the trend of twentieth-century scholarship that treats "the founders' religious claims as mere rhetorical flourish" (p. 36), have chosen to ignore Wilson's "clear attempt to found his moral and legal theory upon a traditional Christian conception of natural law" (p. 37). For this reason, Hall contends, these students also neglect critical aspects of Wilson's thought that derive from his religious orientation, among the more important of these being the limitations that natural law places upon the legitimate scope of government. Indeed, one of the most significant themes in Hall's analysis is that a complete understanding and appreciation of Wilson's teachings come only with a recognition of their Christian natural law foundations.

While, as Hall acknowledges, Wilson's thought stands in need of further analysis, particularly in light of our contemporary problems, his fine book will serve as a much needed starting point.

Our Elusive Constitution: Silences, Paradoxes, Priorities. By Daniel N. Hoffman. Albany: State University of New York Press, 1997. 297p. $19.95.

Austin Sarat, *Amherst College*

One indicator of the continuing viability of the American Constitution as a force in our political and culture life is the ongoing preoccupation of scholars with figuring it out, getting it right, producing new ideas about how to divine its meaning. The viability of the Constitution is taken for granted in our political system, where its power is as much to be found in its taken-for-granted quality as in the majestic pronouncements made in its name. Why we do not just have a paper or symbolic constitution, or one that is not relegated to the margins of our political system, is infrequently examined and remarked upon. One of the real virtues of our elusive Constitution is that it reminds us that while its meanings may be elusive, it is a central force in our political life. In this book Hoffman presents a lively, if somewhat idiosyncratic, tour through the Constitution, covering some well-worn terrain but also touching on some issues, such as nationalism, that are rarely taken up in treatments of it. His argument is respectful of traditional approaches, though he claims to be interested in a constitutive approach to law. His analysis is almost unfailingly progressive in its tendencies, though it is frequently solidly conventional.

Hoffman's starting point is the perception that the Constitution is irrelevant to our political life. This perception, he notes, arises from a misunderstanding of the constitutional process. The Constitution works, he notes, in that it expresses meaningful principles, "understood and supported" by elites and mass publics. Much of this book is, in fact, an effort to understand and explore those principles. Crucial to those principles, Hoffman contends, is the distinction between law and politics; the conceptions of the people, the state, and its powers that animate the Constitution; and our conceptions of personhood and rights. Each of these elements is taken up in its own section of the book, in which Hoffman engages with the major literature, articulating his own views through carefully reasoned criticisms of the views of others.

On the distinction between law and politics, Hoffman resists the extreme deference view, namely, that the courts should avoid any and all political questions. He argues that the status of judicial review in regard to such questions has to be judged in light of the extreme difficulty of amending the Constitution imposed by Article V. He endorses Bruce Ackerman's theory of "dualist democracy" as a way of articulating standards to judge the validity of various governmental and popular actions. Yet, Hoffman notes, any theory of the Constitution must accept that in regard to the law/politics distinction the Constitution's meanings will be at once "clear and unclear, settled and open to revision" (p. 45).

Some of the most interesting writing in the book is found when Hoffman tries to wrestle with the identity of the "People" from whom the Constitution derives its authority. Claims of presidential prerogative and demands for deference are sometimes made in the name of the People. Yet, Hoffman suggests that such claims must, in their extreme form, be resisted. In addition, some of our most crucial political issues involve questions of who is included in the "People." On these issues the Constitution itself is virtually silent, leaving them to the play of ordinary politics. In the end, Hoffman rejects both a pure "nationalist" view of the People and a "liberal" debunking of nationalism while calling for the further articulation of a liberal form of nationalism.

In the last section of the book Hoffman turns to consider ideas of personhood and rights. If there is to be a coherent theory of rights, he suggests, that theory must derive from an idea of what it means to be a person. He argues that there four major categories of rights ought to be ranked as fundamental: the right to life, the right of privacy, political rights, and socioeconomic rights. The last of these is, of course, the most controversial, suggesting as it does the priority of positive as opposed to simply negative rights. In his argument about persons and rights, Hoffman marks his distance from legal positivism, but he also somewhat surprisingly rejects natural law. This distance and this rejection are carried out through a review of various ideas of personhood deriving from philosophy, history, and science. From this review comes the argument that if we are to take rights seriously, then our ideas of personhood must be historically situated as well as attentive to the social prerequisites of personhood, including minimal subsistence needs, education, and "effective access to the economic and political processes that make for self-respect, social integration, and representation in the public sphere" (p. 231). Here Hoffman is at his most unconventional and his most radical. Here his writing is provocative even where it is not persuasive.

Those looking for a systematic, integrated treatment of either doctrinal or interpretive issues relating to the Constitution will not find it in *Our Elusive Constitution*. Instead, readers will find a cook's tour of major and substantial issues pertaining to the viability of the Constitution as a factor in shaping American politics. They will find Hoffman to be a serviceable guide, obviously well versed in the major debates about our Constitution. Throughout, this book is more pastiche than plan, more of a pointing to areas to which everyone must attend than a presentation of arguments and conclusions to which everyone will assent.

Greed, Chaos, and Governance: Using Public Choice to Improve Public Law. By Jerry L. Mashaw. New Haven, CT: Yale University Press, 1997. 231p. $28.00.

Lee Epstein, *Washington University in St. Louis*

For well over a year now, political scientists have been engaged in a serious debate over the value of rational choice theory for the study of law and courts. On the one extreme are those who assert that it can be of enormous use in helping us to unravel the complexities of judicial decisions. On the other are scholars who suggest that it holds little promise in enabling us to develop plausible insights, explanations, and predictions.

This intelligent volume promises to play an important role in these debates. Mashaw views rational choice as neither a panacea nor a dismal failure. Rather, he "*attempts*" to "claim and hold elusive middle ground between public choice's champions and its detractors—to demonstrate in a series of public law settings both the insights that spring from taking public choice ideas seriously and the necessity for maintaining a critical distance from theoretical enthusiasms that sometimes lack internal coherence, not to mention empirical support" (p. viii).

If *Greed, Chaos, and Governance* did nothing more than "attempt" to stake this middle ground, it would make an important contribution. But Mashaw more than succeeds: He ably demonstrates—through the force of logic, coupled with a remarkable grasp of many literatures—why ardent supporters ought to temper their enthusiasm or at least redirect it and why hardcore detractors should reconsider some of the lessons choice theory has offered.

His chapter on judicial review ("Public Choice and Rationality Review") provides substantial evidence of my claim. As Mashaw rightfully notes, scholars and judges have spent decades, really centuries, trying to tell a "persuasive" story that would justify the judicial invalidation of actions taken by elected officials. Could public choice prove useful in helping to establish such an account? To this Mashaw replies, as he does to many other such questions throughout the volume, with a "yes but." The "yes" is as follows: Because public choice theory provides certain insights into how legislatures produce laws—mainly through greed (that is, as the result of private interest deals) or through chaos (that is, as artifacts of voting procedures)—it may help us to identify the circumstances that warrant active and serious monitoring. The "but" is equally straightforward: We should not necessarily equate lessons to learn with ideas to adopt or arguments to use. As Mashaw puts it, "perspectives from the public choice tool kit are useful in thinking about constitutional doctrine. But each must be labeled 'Handle with care' " (p. 52).

To drive home these points Mashaw delineates and critiques the positions of analysts advocating and opposing active judicial monitoring of legislatures. First there are the "true believers," those public choice theorists who argue that laws come about mainly through greed or chaos and, thus, see the need for aggressive judicial review. Mashaw clearly sympathizes with these views: He thinks that courts have taken the countermajoritarian "problem" too seriously, leading them to avoid judicial review in circumstances in which it may be warranted. But he also sees the flaws with this approach, primarily that the mere recognition that we live in a "highly compromised form of democracy" does not provide a good answer to the question about how courts should exercise judicial review. Next there are the "proceduralists," those scholars who agree that many laws are not well designed but believe that the remedy is procedural in nature: Legislatures should revamp their procedures to make better laws (that is, to become something of quasi-judicial bodies), with the job of judges mainly to police these procedures. While Mashaw acknowledges the value of this approach, namely, that it can be useful in helping to identify patently arbitrary abuses of power, he also points out a significant deficit: It ignores the lessons of greed accounts of the legislative process. Finally, there are the conventionalists, those who either take direct aim at the true believer view by arguing that it greatly exaggerates the problems of the legislative process or that it will lead judges to strike down virtually all legislation, thereby returning us to the days of *Lochner*. But this approach, as Mashaw points out, also ignores some of the central insights of the public choice literature; and, in some sense, it begs basic questions of how and how much courts should invoke judicial review.

In short, Mashaw's perspective is that the true believers overstate their case, while the proceduralists and conventionalists do not take the public choice story seriously enough. To put it another way, the public choice literature has "sounded an alarm" about the legislative process (albeit one that may be excessively loud), which other approaches have been hesitant to heed at least in part out of a fear that judges will respond with a *Lochneresque* zeal. But is this necessarily so? Can these visions be reconciled? Drawing on key insights of all three approaches (as well basic ideas embodied in the Constitution), while taking into account their deficiencies, Mashaw provides an answer: "public regardingness," which turns on both substantive and process concerns. Or, as the author puts it:

> I would have courts look for a combination of substantive and decision-process "danger signals" that together suggest that legislation is essentially private-regarding—that it benefits some group in ways that cannot convincingly be explained in terms of a broad range of possible public purposes, or in terms of a well-functioning democratic process. Methodologically, the argument is for viewing both the Constitution and constitutional cases whole; for taking seriously . . . the complicated as well as simple-minded ways in which legislation fails in it most basic function—to pursue the public welfare (p. 79).

How would this approach work? What will courts consider a "public purpose"? Mashaw leaves these and many other specific questions open, but to me the important lesson of this chapter and the others is a far more general one: A middle ground between the champions of public choice and its critics does exist, and Mashaw has managed to locate it by taking account of both the strengths and weaknesses of the theory.

In fact, my only concern with this volume is that political scientists who study law and courts will read it. Surely, it houses something for everyone: the advocates of public choice; the detractors; and even those who want to learn more about the theory, for Mashaw does a masterful job in explaining some of its key insights. At the same time, though, he does not invoke much in the way of quantitative evidence—a potential turnoff to members of this rather observationally oriented group.

But to ignore *Greed, Chaos,* and *Governance* on this ground would be to miss out on a great read. And on a volume that could not be more relevant to current debates within the field.

Machiavelli, Leonardo, and the Science of Power. By Roger D. Masters. Notre Dame and London: University of Notre Dame Press, 1996. 366p. $32.95.

Albert Somit, *Southern Illinois University*

Roger Masters argues quite a few theses in this impressively erudite and wide-ranging book. Space being limited, I will focus on the four I regard as most important. First (chap. 1), and in my mind both regrettably and unnecessarily, Machiavelli's "new way" of looking at political behavior (as evidenced in *The Prince*) was significantly influenced by Leonardo da Vinci. Second (chap. 2), a close reading of *The Prince* reveals that, contrary to general opinion, it is philosophically consistent with the earlier *Discourses on Titus Livy*, and Machiavelli should be credited with having "laid the foundation" for the "constitutional" or "democratic" state (p. 5). Third (chaps. 3–6), Machiavelli's then-novel depiction of human nature and political behavior is still "valid in the light of contemporary natural science" (p. 5). Fourth (chap. 7), Machiavelli and Da Vinci made important contributions to the "emergence of modernity" and to the "specifically modern" way of looking at politics, art, science, and technology (p. 7). Let me address these ideas in the order stated.

The Da Vinci–Machiavelli Connection. As Masters notes, others have commented on certain similarities between da Vinci and Machiavelli; he, however, is the first to argue that the former significantly influenced the thinking of the latter. That argument rests on two contentions: (1) Da Vinci and Machiavelli were personal acquaintances, and (2) even if this was not the case, Machiavelli was familiar with Da Vinci's writings.

One fact is indisputable: Da Vinci was serving as Cesare Borgia's architect and military engineer in 1502, when Machiavelli was Florence's emissary to the Borgia court. But did they ever meet? There is no hard evidence of such a meeting. Furthermore, Masters acknowledges there is not "a single explicit reference to Machiavelli in the voluminous *Notebooks* of Leonardo"; conversely, "Machiavelli never discusses Leonardo in his writings—even though there would have been ample reason to do so" (p. 12).

Still, the absence of evidence does not constitute evidence of absence. Undaunted, Masters suggest that this very silence "may actually reinforce the substantive importance of the contacts between them" (p. 12). Finally, after distinguishing between a "maximal" (they were close friends) and a minimal (they may have met) interpretation of their relationship, he decides that "the truth must be somewhere between" (p. 13). I am puzzled both by this formulation (the minimal position should be that they *never* met) and by the logic of his conclusion.

Whatever the personal relationship of the two men, Masters insists, Machiavelli's thinking was "particularly shaped" by his acquaintance with Da Vinci's writings. That claim is based essentially on two items of evidence: (1) similarities in their ideas of military strategy and technology and (2) the description of a storm in Machiavelli's *Florentine Histories*, on the one hand, and, on the other, "striking drawings of such a storm as well as verbal descriptions of similar cataclysms" in Leonardo's *Notebooks* (p. 24). I do not find this "evidence" very persuasive, a problem to which I will later return.

The Discourses *and* The Prince*: Which Is the Real Machiavelli?* Most political philosophers have difficulty in reconciling the prorepublican Machiavelli of the *Discourses* with the Machiavelli of *The Prince*. Masters undertakes to square the circle by a "close reading" of the later work. Such a reading, he reminds us, calls for an awareness of "multiple levels of meanings," a sensitivity to inverted meanings, interpretations based "on the assumption that the author was intentionally devious" (p. 39), and the perceptivity to draw meaningful inferences from what is *not* said. As Masters himself warns, "to be sure, great care is needed when reading in this manner" (p. 47).

The not altogether surprising fruit of such a reading is that, when so considered, the two works actually reflect the same underlying republican convictions. Has Masters successfully resolved a long-standing problem in political philosophy? I think not. Nonetheless, I can enthusiastically commend this particular chapter as an excellent example of what an able practitioner of what some would call a "Straussian" reading can accomplish in the way of textual exegesis.

Human Nature as Seen by Machiavelli and Contemporary Science. How does Machiavelli's understanding of human nature stand up in the light of contemporary scientific knowledge? Masters devotes almost 80 pages to this question, drawing knowledgeably on both the natural (ethology, primatology, behavioral ecology, sociobiology, evolutionary biology, neuroscience) and the social (game theory, public opinion and communication, economics, and at some length his own work in nonverbal communication) sciences. There is, he finds, "an extraordinary congruence" (p. 210) between Machiavelli's views and the findings of modern biological science and, implicitly, of social science as well. I would surely concur.

Machiavelli, Da Vinci, and Modernity. The attributes which collectively distinguish "modernity" from previous epochs are a concern with personal security, mobility, predictability, comfort, and progress. Did Machiavelli and Da Vinci contribute significantly to the "emergence of modernity"?

Certainly, Machiavelli did; in Masters's opinion, probably moreso than anyone else: "When contrasted to Machiavelli, let alone the thought of antiquity, moderns from Hobbes and Locke to Hegel, Marx, and Nietzsche appear to base their understanding of human life on oversimplified concepts, erroneous assumptions, and biased interpretations of the facts" (pp. 162–3).

Masters also views Da Vinci as an important contributor to modernity, with his principal legacy being "the *scope* and the *interrelatedness* of the new ways of thinking he sought to introduce to virtually every human endeavor" (p. 170, emphasis in original). These "new ways of thinking" were to have a major effect on subsequent—and eventually modern—art, science, and technology.

Others, of course, have previously discussed the importance of Machiavelli and Da Vinci, respectively, for our present way of looking at the world. Masters does so, though, from a quite different perspective. And that brings me reluctantly back to what I regard as an unfortunate flaw in an impressive work of scholarship and imagination—his contention that Machiavelli was significantly influenced by Da Vinci, and his consequent decision to make that highly problematic contention his point of departure. There is no hard evidence for the purported influence, and the book does not require that thesis. Relatively little in the rest of the volume would have to change if Masters had instead opted to assess Machiavelli's and Da Vinci's respective contributions to posterity and modernity, rather than posit a close connection.

Masters's reading of *The Prince* may also evoke some skepticism, but his imaginative interpretation is manifestly a matter of opinion and, in any event, comes later in the book. But the dubious (at least to me) claim of a Da Vinci–Machiavelli linkage which opens the volume may, I fear, deter some readers from proceeding farther. If so, they will miss a first-rate, albeit often idiosyncratic, essay in Western intellectual history.

Democratic Temperament: The Legacy of William James. By Joshua Miller. Lawrence: University Press of Kansas, 1997. 172p. $29.95.

Timothy V. Kaufman-Osborn, *Whitman College*

Joshua Miller's *Democratic Temperament* is a welcome addition to the revival of American pragmatism. For the most part, that resurgence has concentrated on the work of John Dewey and his suspect heir, Richard Rorty. Miller's concern, in contrast, is with the work of William James. Countering the caricature of James as a radical individualist for whom political concerns are irrelevant, Miller seeks to tease from his texts something akin to a coherent theory of participatory democracy. That theory, in turn, informs Miller's observations on various contemporary political dilemmas, including the sorry state of public education in the United States, the limits of tolerance in liberal political orders, the relationship between gender and democratic citizenship, and so forth.

As Miller is quick to admit, it is no easy trick to squeeze from James any sort of consistent political theory, let alone one that qualifies as democratic. In part, this is due to the fact that James never bothered to elaborate his political views in a systematic fashion. This problem is still more vexing because those texts that do take up political concerns are littered with what appear to be chronic ambiguities and/or outright contradictions. Most notably, James never really confronted the tension between his egalitarian celebration of everyday experience and his elitist pretensions, which are nicely captured in his characterization of ordinary folk as that "herd of nullities whose votes count for zero in the march of events" (p. 23).

"What method of interpretation is appropriate for a thinker with so many contradictions" (p. 8)? Miller deploys a variety of strategies in answering this question. Sometimes, for example, he seeks to reconcile apparent inconsistencies and so render James more or less coherent. He is persuasive, for instance, when he argues that James's brief on behalf of intellectual genius can plausibly be construed as an effort "to augment, not replace, democracy" (p. 106). At other times, Miller leaves James's contradictions unresolved but then, in an interesting move, goes on to show how they are indicative of some larger unresolved cultural or political incoherence. For example, noting the tension between liberalism's desire to constrain action within established constitutional channels and radical democracy's tendency to punch through those same boundaries, Miller argues that "James embodies the conflict between liberalism and democracy on the issue of political action" (p. 16). Finally, there are times when Miller simply concedes the impossibility of overcoming James's inveterate penchant for disorderly thinking: "Although James offered pluralism as a source of both action and respect, the psychological roots of the one are incompatible with the other. He understood that action usually springs from a passionate belief in a principle and that this passionate belief leads the typical political actor to resist pluralism" (pp. 81–2).

Leaving aside the question of whether the effort to render James consistent sometimes generates strained readings, and leaving aside the question of whether the attempt to render James systematic is somehow at odds with his affirmation of life's "buzzing and blooming confusion," Miller's struggle to "rescue James as a democrat" (p. 23) raises another and perhaps more significant issue. Miller's aim is to salvage not just any sort of democrat, but rather a democrat with clear participatory stripes. By participatory democracy, Miller means something quite familiar to those of us who are creatures of the late 1960s: a commitment to substantive equality, a challenge to the bureaucratization of political life, a celebration of localism, a criticism of received authority relations within the classroom, and so on. These are worthy goals, and Miller does a very credible job of showing how James can shed light on what it means to seek their accomplishment within the confines of a profoundly antidemocratic order.

What, though, is lost by assimilating James to this commonplace, albeit critical, conception of democracy? What we lose, I would suggest, is an opportunity to consider James as a radical (as opposed to participatory) democrat, where the term "radical" signifies an effort to defamiliarize our conventional understandings of the very nature of the ideal we mean to pursue. Such an accomplishment might be possible if we, as Miller generally does not, were to pay less attention to James's express claims about politics and more to the political import of his philosophical, that is, his ostensibly nonpolitical, texts.

Miller takes a tentative step in this direction when he elicits from James's antifoundational epistemological commitments a generous conception of mutual respect, one that goes well beyond liberalism's pinched defense of toleration for those who are different. He does not, however, do much with James's ontological writings and, more particularly, with the essays contained in *A Pluralistic Universe* and *Essays in Radical Empiricism*. While I am reluctant to criticize Miller on the grounds that he failed to write the book I think he should have written, I do believe that the relational ontology James sketches in these texts opens up new and potentially radical ways of thinking about the promise of democracy.

For example, as I read them, the ontological pluralism espoused in these volumes invites us to call into question the dreary opposition between liberals and communitarians. It does so by suggesting, first, that difference is etched into the very structure of what James dubbed the "multiverse" and, second, that such difference is itself a fruit of the complex webs of affiliation from which it is teased. While an exploration of this quasi-Nietzschean insight will not overcome the contradictions in James's expressly political writings, it may help us think our way through some of the thornier dilemmas confronting democratic theory and democratic politics today. Think, for instance, about identity politics and its alleged threat to the possibilities of broadly based democratic action. If, as James's ontology intimates, the identities of those given to such politics are creatures of those to whom they define themselves in opposition, then does collective action really require the achievement of an unambiguous shared identity on the part of its participants? And does not this insight open up promising ways of thinking about the prospects for various forms of coalitional politics among those otherwise all too quick to demonize each other?

Because Miller is given to a communitarian vision of participatory democracy, he is indisposed to explore such possibilities. He comes closest to doing so in his brief discussion of William Connolly's argument on behalf of adversarial democracy based on an agonism of differences; on that account, which could be construed as a possible articulation of the political import of James's ontology, opponents respect the antagonists whose identities are acknowledged as essential to the constitution of their own. Such a possibility, Miller says, "seems impossible to realize" because "it is difficult to imagine political relationships forged by gratitude for alternative interpretations of oneself" (pp. 73–4). In this world, Miller's point is perhaps well taken. In a world that took James's pluralistic ontology more seriously, it may have appeared less so.

In his conclusion, Miller recapitulates his central aim as

follows: "I have not tried to claim that James was a political theorist rather than a philosopher. Instead, I have argued that political concerns were central to his intellectual work and that studying James while remaining aware of those concerns is useful for political theorists and anyone interested in the future of democracy" (p. 119). This Miller has done, and done very well. While I remain convinced that a more provocative, and perhaps more disruptive, conception of democracy lurks within pragmatism, Miller has performed a genuine service for those who would enlist James in the cause of participatory democracy.

The Last Word. By Thomas Nagel. New York: Oxford University Press, 1997. 147p. $19.95.

Ruth Lessl Shively, *Texas A&M University*

In disputes about justification, some theorists would give the last word to the relativistic qualifier, making statements of truth valid only from the perspective of the person or group. Others would give the last word to an unqualified notion of reason, giving statements of truth a validity independent of any particular perspective. Thomas Nagel's *Last Word* makes a remarkably clear and forceful case for the latter rationalist position as against the former subjectivist position.

The argument rests on a kind of restatement of Descartes's *cogito*. There are built-in limits to subjectivist doubt because challenges to the independent validity of reason must themselves assume the independent validity of reason. Or any explanation of reason deriving from *outside* the mind—as produced by natural or nonrational forces—can itself be explained only from *inside* the mind, as having its own independent validity.

The argument is an old one, but Nagel explores and extends it in new and fruitful ways, applying its insights to the fields of language, logic, science, and ethics as well as demonstrating why each of these forms of knowledge must be grounded in immediate, first-order thought about the world. In the process, he not only refutes a number of common mistakes about reason—in particular, the mistake of thinking that we side-step the Cartesian problem by replacing objective judgments with other judgments about rules of practice, standards of rational acceptability, or forms of consensus—but also, in what was for me the most interesting part of the book, he speculates about possible reasons and possible cures for subjectivism's continuing appeal among modern intellectuals.

One such reason has to do with the fact that subjectivism is so appealingly simple. The idea that all counterclaims can be denied with a simple and endlessly generalizable response—"That's just what you think"—has a fundamentalistic attractiveness to it. While, in contrast, the defense of rationalism must always be a complex and piecemeal endeavor, a more demanding (though ultimately more rewarding) attempt to show that, within each field of knowledge, an objectivist account of the evidence is needed.

The appeal of subjectivism also arises out of a certain reductionist impulse in modern explanation, for insofar as modern thinkers make the explanation of things hinge on their reduction to local and finite terms, they ensure subjectivist (or locally and finitely defined) conclusions. And, as such, they ensure against rationalist explanations that refuse to make reason into something nonrational or that conceive of reason as a capacity for grasping the universal and (sometimes) infinite principle from within the local and finite perspective. As Nagel points out, however, the reductionist explanation is itself dependent on an irreducibly nonlocal, objective understanding of reason; doubt about reason presupposes reason's independent validity. And as such, reason's independent validity cannot be coherently doubted.

Finally and most intriguingly, Nagel connects the appeal of subjectivism to a "fear of religion" that preys upon modern intellectuals, himself included. This fear works against rationalism, he says, because it is hard to envision a rationalist world—a world in which there is "a natural sympathy between the deepest truths of nature and the deepest layers of the human mind" (p. 130)—that is not religious. Or there seems less threat of a divine presence in a world that is driven by natural accidents, mindless mechanics, and other safely "dead" principles. But since the antirationalist option is, again on Cartesian grounds, self-refuting, Nagel suggests that there is no choice but to embrace a more "mind-friendly" cosmology, though he assures the nervous atheist that this can be done in a nonreligious way.

I have just two small quibbles with this book. One has to do with Nagel's description of his position as Cartesian or Platonic when it seems to me to clearly imply and require an Aristotelian (or similar) framework. (Curiously, Aristotle is featured on the book's cover but never mentioned within.) Some such framework is implied, I think, by Nagel's decidedly realist take on first principles, that is, by his important recognition that the attempt to prove these principles is fundamentally misguided and that if we are to think at all, we must simply accept the mind's adequacy to the world. "We begin from the idea that there is some way the world is," he writes, for example, "an idea to which there is no intelligible alternative and which cannot be subordinated to or derived from anything else" (p. 81). This and other insights are reminiscent of Etienne Gilson's brilliant, though neglected, argument against Cartesian idealism in *Thomist Realism and the Critique of Knowledge* (1939). As Gilson rightly observed, one either begins with the realist assumption of an intelligible world or with an idealist assumption of doubt about an intelligible world, in which case one can never come into contact with that world.

This is not to deny that Nagel's use of Descartes's *cogito* helps illuminate the logical problem of subjectivism or that Plato's idea of harmony provides a nice metaphor for the mind's correspondence to nature. It is simply that the idealist limitations of both philosophies keep them from providing the positive explanation of reason that the thesis requires: a realist explanation, that is, one which assumes the mind's capacity to grasp what is necessary or universal in the body's contingent and particular experience. For only then can the act of knowing unite the mind and the senses, or the universal and the particular, in a way that avoids both empty rationalism and irrational empiricism.

A second minor complaint concerns Nagel's dismissal of the theistic explanation for reason; he argues that the concept of God does not provide an explanation or is a mere "placeholder" for an explanation because it is itself inexplicable. The problem here is that Nagel has already refuted this kind of objection when lodged against his own position. He rightly argues that to object to reason on the grounds we cannot strictly explain it in naturalistic terms is to misunderstand the irreducible nature of the concept: the fact that it cannot be so explained without losing its meaning or validity and that, as such, it is justified in a different way, by showing it to be necessary to intelligible thought and action. On the same grounds, then, the concept of God would be justified, or would be something more than an empty placeholder, if it turned out to be irreducible and yet necessary to intelligible thought and action. One may argue against this conclusion, of

course, but the point is that this is the argument to be defeated.

These quibbles aside, Nagel's argument is impressive. And while it does not deal directly with political questions, it is a much-needed corrective to certain pervasive tendencies in political theory today. In particular, it provides a necessary challenge to the often uncritical privileging of subjectivism that pervades the discipline and calls attention to the many ways in which this tendency (or the subjectivist's easy dismissal of rival claims as merely rival opinions) undermines our ability to engage fruitfully in argument with others.

Foucault's Discipline: The Politics of Subjectivity. By John S. Ransom. Durham, NC: Duke University Press, 1997. 225p. $49.95 cloth, $16.95 paper.

Michael Mahon, *Boston University*

Michel Foucault called a 1978 Sorbonne lecture "What Is Critique?" because it would have been immodest of him to dare to entitle it "What Is Aufklärung?" John S. Ransom in his excellent new interpretation shrugs off considerations of modesty and boldly lays bare the relationship between critique and Enlightenment, significantly advancing our understanding of this seminal thinker's work. Foucault's version of Enlightenment, Ransom argues, requires the separation of critique from utopian visions, of liberation as goal from Revolution as event, of oppositional thinking from normative ethics, establishing critique as an end in itself rather than as a means to some other purpose. Ransom emphasizes three prongs in Foucault's notion of critique, recognizing Foucault's intellectual debts to Heidegger, Nietzsche, and Kant. From Heidegger, Foucauldian critique transforms our "ready-to-hand," unthinking, and unexamined mode of being into "present-to-hand" subject matter for critical evaluation. Nietzsche revealed the critical power of historical genealogy, enabling careful scrutiny of thoughtlessly accepted conventions. Kant contributes to Foucault's critique a rootedness in the world and the present and an almost entirely negative understanding of Enlightenment as an exit, a way out, an escape. Kant thereby frees the critic from any alleged obligation to anchor critique in a positive utopian alternative to the present.

That, according to Ransom, is precisely what Foucault's best-known critics demand of him. Ransom, not unfairly, tends to lump these critics together—Jürgen Habermas, Charles Taylor, Thomas McCarthy, Nancy Frazer—because, while each contributes her or his own nuances, their criticisms of Foucault share a distinctive family resemblance. Their common bottom line is expressed most concisely by Frazer ("Foucault on Modern Power," *Praxis International* 1 [October 1981]: 286; Ransom, p. 171): "What Foucault needs and needs desperately, are normative criteria for distinguishing acceptable from unacceptable forms of power." But in order for such norms to have the universality demanded by the critics, Ransom argues, they require grounding in a stable and universal human subjectivity that Foucault explicitly and repeatedly denies us. What effective liberation movements "desperately need," says Ransom, borrowing Frazer's language, is not to reveal universally applicable norms, nor to penetrate the eternal truth of our subjectivity to ground such norms, but to create new modes of being producing new norms.

Perhaps Ransom's most important contributions result from his willingness to take seriously what others only pay lip service to: the old news that Foucault's work is Nietzsche-inspired. A common misinterpretation of Nietzschean genealogy (even among Foucault's sympathizers) is that it attempts to reveal that power was operating in the formation of, say, Christianity or the modern prison system, and that this presence of power at the origin calls forth its denunciation. But such a revelation is no revelation at all for Nietzsche and Foucault. The assertion that power operates at the origin of anything is, for them, true but trivial. The important question is the *how* of power. The realities that confront us have been made, and they can be unmade if we understand how they have been made. Foucault's claim—also true but trivial, but not often enough recognized as such—is that administrators and police chiefs, prison wardens and social workers, flunkies and bureaucrats, rarely engage in political philosophy, rarely concern themselves with normative criteria, questions of the human essence, or the design of utopian fantasies. But the products of their pens more often see the light of day. Genealogy, after all, as both Nietzsche and Foucault maintain, is gray, meticulous, and patiently documentary, not a matter of looking about in the blue. *Pudenda origo.*

Ransom more effectively delineates various uses of genealogy than any other Foucault interpreter. Much too briefly, conflating what Ransom so carefully distinguishes, we might call them negative and positive genealogical strategies. A historical genealogy of battle lines and contingency "prods us to become aware of unquestioned assumptions, thoughtless gestures, seemingly legitimate institutions, and unexamined modes of thought by revealing their contingent and (sometimes) violent foundations" (p. 95). A more positive, political genealogy narrates a troublesome history, often previously concealed, in order to advance the formation of a responsive community of action, retrieving previously disqualified or insufficiently elaborated "subjugated knowledges." This richer, nuanced understanding of genealogy illuminates anew the centrality of Foucault's commitment to freedom.

Ransom's interpretation of Nietzsche occasionally psychologizes Nietzschean genealogy. Foucault carefully separates Nietzschean genealogy from Paul Rée's English tendency to seek psychological origins, while Ransom portrays Nietzsche too much like Rée, tracing "psychological roots and effects" (p. 5). The index is annoyingly incomplete, at least for this reviewer's purposes. But to elaborate on such criticisms would be to nitpick and detract unfairly from the outstanding contribution of this well-written and well-researched, rigorously thoughtful work.

The Anglo-Marxists: A Study in Ideology and Culture. By Edwin A. Roberts. Lanham, MD: Rowman & Littlefield, 1996. 320p. $67.50 cloth, $24.95 paper.

Bradley J. Macdonald, *Colorado State University*

For students of British Marxism, Edwin Roberts's book will be a welcome historical study of generally overlooked Marxist theorists. Focusing primarily on the period between the 1930s and 1950s, Roberts deftly combines textual exegesis of the work of major Marxist figures (in particular, John Lewis, J. D. Bernal, J. B. S. Haldane, and Maurice Cornforth) with a clear rendering of the vicissitudes of the British communist political movement (in which all these theorists were important participants). In this respect, the reader is treated to a clarification of the intimate relationship that existed between the development of Marxist theory in Britain and the fortunes of the Communist Party of Great Britain (CPGB). As an historical study of this nature, the book is a needed addition to scholarship on British political history and British political theory.

The historical value of the study clearly surfaces in the middle five chapters. Roberts begins his narrative by looking at the roots and subsequent flourishing of Marxist theory in Britain during the pre–Communist Party days (roughly between 1883 and 1920). In this context, Roberts provides a concise yet subtle historical rendering of the growth of a rich Marxist tradition among differing elements within the British socialist movement: The first modern English socialists (which included such notables as H. M. Hyndman and William Morris) appropriated Marx's writings into their somewhat eccentric positions. Tied to the growth of working class education, "proletarian autodidacts" like T. A. Jackson and Tom Bell began to articulate Marxism as a "proletarian science" closely associated with their life experiences. During the early twentieth century, the development of the first avowedly Marxist parties (e.g., Socialist Labour Party, British Socialist Party, and Socialist Party of Great Britain) provided ample organizational space in which to disseminate Marxist theory. All in all, the pre–Communist Party era was a theoretically productive period, one in which Marxist ideas were being increasingly integrated within the discourses of the British Left. It was also a period in which Marxism began to enter into diverse arenas of English intellectual life.

Yet, according to Roberts it is not until the founding of the CPGB in 1920—when Marxism could finally become organizationally sound and theoretically codified—that we perceive the truly productive reverberations Marxist theory had on other intellectual fields. For members of the CPGB, Roberts notes, "the program for working-class liberation was a unified science of society being put into practice by the government of the USSR. The job for Marxist intellectuals in the Communist Party era would be to translate this new doctrine into a British context" (p. 55). Such theoretical "clarity" and cultural translation is brought out by Roberts in separate chapters that detail, respectively, the careers and writings of John Lewis, the "red scientists" (J. D. Bernal, J. B. S. Haldane), and Maurice Cornforth. Each of these thinkers applied Marxist theory in the British context by wedding it to indigenous intellectual traditions, be it Greenite idealism, British scientific discourses, or analytic philosophy. Perry Anderson's famous argument about the poverty of a Marxist tradition in Britain is laid to rest. Robert's study allows one to see a rich British tradition of Marxist thought that has been developing since the turn of the century, one that is interesting and important in its own right.

Aside from the historical value of these theorists for understanding a particular period in the British Marxist tradition, why should one look to them? What theoretical issues, if any, do they raise that show them to be important innovators in this tradition? Moreover, what does the analysis of this trajectory add to our understanding of the tradition of Western Marxism in general? It is to the credit of the author that these questions are not ignored and that he links his historical excavation of these theorists to larger methodological and conceptual issues that would be of interest to students of Western Marxism in general. Yet, it is precisely in attempting to answer these larger questions that the work's argumentative structure begins to strain, tending to leave the reader with pages of historical description and textual exegesis without any definite sense of the larger relevance and significance.

For the author, two overall goals drive his historical narrative. First, Roberts wishes to settle a *metatheoretical* issue of how we can understand hyphenated forms of Marxism in general. In this respect, the period between the 1930s and 1950s offers one a telling case study of the ways in which Marxism takes on unique hybrid forms. In Chapter 1, Roberts attempts to lay out some methodological criteria for use in this matter: "We can define a hyphenated Marxism as a school of thought constructed by those seeking to resolve a question through the means of a Marxian method with the hope of achieving the verification of a Marxian principle while doing this within a framework of some non-Marxist theory that must be imported into Marxism in order to solve the problem" (p. 10). Leaving aside the issue of its conceptual cogency, such a definition begs the question of whether there are such distinct realms of theory within different schools of Marxism. Indeed, many of the practitioners of these schools of "hyphenated Marxism" assume their theory to be intimately Marxist. The problem with this definitional assumption clearly arises when Roberts points to well-known schools like existential Marxism and structural Marxism as good examples of this hybrid form. As students of Western Marxism are aware, both Sartre and Althusser would balk at the claim that their form of Marxism is embedded in a "non-Marxist" framework (e.g., Althusser saw his structuralist position as reflected in Marx's mature work itself).

After taunting the reader with such a definition, Roberts argues that there are forms of hyphenated Marxism that are culture specific (e.g., "Austro-Marxism"), and it is ultimately this type of Marxism that is more directly related to his area of study. Moreover, Roberts sees his work as definitively showing that there is a unique and specific tradition called "Anglo-Marxism" (see pp. xiii, 1, 15, 270). What allows us to define a particular tradition as "Anglo-Marxism" is that it is a form of Marxism which both draws upon traditions unique to British intellectual life and responds to unique political issues associated with that particular context. While Roberts is on firmer conceptual ground here, one wonders—particularly after consuming the historical details he lays out—if the term "Anglo-Marxism" really helps us that much: While the theorists he covers are all English, they all drew upon a form of Marxism that was manifestly Soviet inspired (particularly, as Roberts notes, Lenin's and Stalin's interpretations of Marx), and each drew upon different, seemingly antagonistic, indigenous traditions (e.g., Greenite idealism for Lewis; analytic philosophy for Cornforth). Unfortunately, after all is said and done, one is left more with doubts than assurances concerning the unique and specific character of "Anglo-Marxism."

Roberts's second goal is the interesting *genealogical* claim that this early Anglo-Marxist trajectory seems to be an important theoretical precursor to the recently influential school of analytical Marxism, a school most clearly represented in the works of G. A. Cohen, Norman Geras, and Alex Callinicos. While the author puts forward this position tentatively at first, he does argue that Cornforth's espousal of a form of "scientific empiricism," his attentiveness to the "logical structure of arguments," and his rejection of Hegelian metaphysics are paralleled in the concepts and methods of present-day analytical Marxists (pp. 6–9, 241–2). On the surface there are clear "family resemblances" between Cornforth's position and that of a G. A. Cohen. Moreover, they are both English. Yet, sharing both a nationality and a general goal of being conceptually and logically rigorous in one's theory does not necessarily establish a school of Marxist thought, let alone point to the necessary continuities associated with a unique Anglo-Marxist tradition.

While not ultimately successful at achieving these larger goals, Roberts has produced a work that elucidates a deep reservoir of Marxist thought in twentieth-century Britain. In so doing, he has offered the reader a picture of a more integrated and interconnected British Marxist culture than one may have assumed.

Nomos XXXVIII: Political Order. Edited by Ian Shapiro and Russell Hardin. New York: New York University Press, 1996. 532p. $45.00.

Brian Barry, *London School of Economics*

My picture of the way in which books in the Nomos series come into existence is this. The editor starts off with a vision of a sleek, unified volume in which all the contributions will relate in predetermined ways to the announced theme. Three papers are then commissioned for the annual meeting of the American Society for Political and Legal Philosophy, and their authors (as is the way with authors) proceed to write about whatever they had in mind to write about, while the discussants (two per paper) ride off on hobby-horses of their own. To get back to the original plan, the editor then casts around for additional authors, who may or may not provide what was hoped for. The result of all this is the plump volume that members of the ASPLP look forward to receiving just when memories of the meeting are beginning to fade (in this case it took place in 1993).

The Introduction to *Political Order* (pp. 1–15) suggests that this time the editors just gave up. In the past, the introductory essay has on occasion been a substantial free-standing review of the topic. Here, general introductory remarks cover only one page of text and identify ten "questions about political order" (p. 1). This may seem like a lot of questions for eighteen chapters, but the editors prudently hedge their bets further by saying: "Our authors address these *and related questions* in all their richness and complexity" (p. 2, emphasis added). The rest of the Introduction simply summarizes the chapters, enhancing rather than assuaging the impression one gets from reading them of (to borrow an expression quoted on p. 116 from Walter Dean Burnham) "one damned thing after another."

If we forget about coherence, however, and take a hint from most of the authors in not worrying too much about political order, then we can get quite a lot out of the book. Two chapters would make very useful student assignments: Norman Schofield's brilliantly succinct (albeit sometimes cryptic) survey of the entire rational choice program and its bearing on the issue of stability versus chaos, and Robert Dahl's summary of his own work and that of others on the conditions of stable democracy. There are also solid chapters by Pasquale Pasquino on early history of the concept, Nicholas Miller on the conditions under which majority rule oppresses minorities, Thomas Christiano on the requirements of deliberative democracy, Elizabeth Kiss on nationalism, and Richard Arneson and Ian Shapiro on *Yoder*. Leaving aside those to be mentioned later, the other chapters—slighter but also of value—are by Deborah Satz, Lainie Friedman Ross and David Schmidtz, and Robert Goodin.

The most original and stimulating contribution is James Scott's "State Simplifications: Nature, Space, and People." The basic idea here is that, in order to make sense of the societies they are trying to govern, state officials have to impose drastic analytical simplifications, but they are then driven to change the reality to fit: "The utopian (immanent) tendency of the modern state, continually frustrated, is to reduce the chaotic, disorderly social reality beneath it to something more closely resembling the administrative grid of its observations" (p. 57). Starting with Prussian and Saxon scientific forestry in the late eighteenth century, Scott develops a critique of what he calls "high modernism," whose exponents include the founders of the TVA, Le Corbusier, Lenin, and (an extended example) Nyerere. He points out that the results are most tragic in the (ex-) second and third worlds, where forces capable of restricting crazy schemes (such as collectivization, the Great Leap Forward, Virgin Lands, huge dam projects, and so on) are lacking. (On a more modest scale, recall that Le Corbusier's only really big chance to mess things up came in India. Even in France, the most technocratic first-world society, his most grandiose projects were frustrated.)

Scott draws a contrast between this "utopian modernist vision" and "mētis", which is "the kind of practical skills acquired in responding to a constantly changing natural and human environment" (p. 75). To any political theorist, this sounds uncannily like Michael Oakeshott sailing the boundless sea, and though he is not mentioned, his contrast between "rationalism" and following traditional practices is very similar to Scott's distinction. Thus, mētis is relevant where "skill is typically acquired through practice (often apprenticeship) and a developed 'feel' or 'knack' for strategy" and in situations that "resist simplification to deductive principles which can successfully be conveyed through book-learning" (p. 76). The major difference is that Scott's examples are more persuasive than Oakeshott's strained attempt to portray Clement Attlee as a reckless revolutionary.

A serious miscalculation is the 74 pages allotted to an exchange between Karen Orren and Stephen Skowronek, who get a joint statement and a rebuttal, and two of their victims, Walter Dean Burnham and Morris Fiorina. It is hard to tell what if anything is at stake here, if only because "intercurrence," advanced as a theoretical panacea, does not appear to be a theory as against (to adapt Burnham) the assertion of "one damned thing at the same time as another." In any case, a dispute that can be properly evaluated only by those with a minute knowledge of the origins and history of the ICC is surely out of place in *Nomos*.

Finally, a few curiosities. John Gray tells us that he has changed his mind, but scarcely why, and the phenomenon itself occurs too often to be significant. Shelley Burtt apparently has no difficulty with the desire of the *Mozert* case parents to avoid their children's exposure to the major literary and scientific achievements of Western civilization on the ground that "they will not be able to shield their children entirely from the country's secular, highly commercialized mass culture" (p. 433)—as if toys and television (her own two examples) were a substitute! This is sad, but not as sad as Jennifer Nedelsky's revelation that the only time she has ever felt free of the fear of rape was at the Michigan Women's Folk Festival (p. 494). I suspect, however, that even Canadians (many of whose ancestors fled the promise of "life, liberty, and the pursuit of happiness" and are still dubious of numbers two and three) will eventually balk at the remedies she proposes, which would eliminate basic legal safeguards such as *men rea*.

In sum, this is a pudding containing one silver dollar, a number of plums, and some bits that should have been caught in the seive.

The Quest for Evolutionary Socialism: Eduard Bernstein and Social Democracy. By Manfred B. Steger. Cambridge: Cambridge University Press. 1997. 287p. $64.95.

Edwin A. Roberts, *California State University, Long Beach*

The best scholarship often serves a dual purpose, in helping to place ideas and events in a proper context for those who have not done research in a particular area and in challenging our perceptions about their meaning and nature. At times this results in a mixed reaction: We admire a book at one level but are in disagreement at another. My reaction to this new political biography of Eduard Bernstein is that while I

can admire it as a comprehensive overview of the life and thought of one of the most influential figures of the twentieth-century European Left, I reject the book's central premise, as I believe it is demonstrably false.

Eduard Bernstein (1850–1932) was a journalist, editor, and leading figure in the German Social Democratic Party (SPD) from the 1880s until his death. He is most famous for the book *Die Voroussetzungen des Sozialismus und die Afgaben der Sozialdemokratie [Evolutionary Socialism]* (1899). In it he advocated revision of such key elements of classical Marxist theory as the inevitable breakdown of capitalist production, the growing antagonism and polarization of social classes, and the need for a socialist revolution. In their place, he defended the notion of a growing cooperation between workers and the middle class, the gradual evolution of capitalism into a system of rational socialized production, and the need to defend socialism as a Kantian ethical imperative, rather than on a scientific analysis of the system's inherent contradictions.

To this point, there has been a general agreement among scholars that Bernstein's actions were motivated mainly by his belief that the SPD's reformist practices did not match its revolutionary rhetoric, and that the two should be brought more in line. At the same time, scholars with varying degrees of sympathy for Bernstein, such as George Lichtheim, David McLellan, Lucio Colletti, and Leszek Kolakowski, have testified to his basic lack of rigor in conceptualizing problems and his misunderstandings about key elements of Marxist theory. Against this tendency, Steger argues that Bernstein's work did represent a coherent (and even superior) alternative to Marxism as a basis for socialism. His thesis is that Bernstein's model of evolutionary socialism was a pioneering effort to reconceptualize the relationship between liberal and socialist theory, allowing for a more constructive understanding of the problems of democratization, political morality, international relations, and social reform. In this, Bernstein becomes the initiator of a new synthesis combining the historically necessary ideal of socialism with the "eternal values" of liberalism (p. 8).

Steger calls Bernstein's project a *quest* for two reasons, one stated, the other implied. According to Steger, "the term quest connotes an almost romantic adventurous journey with no fixed destination yet guided by a noble vision" (p. 15). What this implies is his realization (and acceptance) of the fact that Bernstein never articulated a coherent theory of what he understood as evolutionary socialism. It is in this area of defending Bernstein as a significant (rather than influential) socialist thinker that I find fault with Steger's work.

In the Introduction, Steger quotes Rosa Luxemburg, who in her famous polemic against Bernstein, *Reform or Revolution?*, commented that his work amounted to "a pile of rubbish, in which the debris of all systems, the pieces of thought of various great and small minds, find a common resting place" (p. 5). Steger's study is an attempt to cast doubt on this judgment. Yet, often, despite the author's best efforts, Bernstein appears as a thinker of extraordinary shallowness, whose eclectic approach to theoretical problems seems to have caused confusion among many would-be followers and aggravation to those who knew better.

Steger claims that Bernstein's intellectual limitations can be seen as irrelevant to his power as a political theorist because, Steger maintains, history has vindicated his desire to build a grand coalition between socialists and liberals which, Steger argues, would have helped Germany become a stable liberal democracy that presumably would have evolved into a working form of socialism (p. 10). This lesson, Steger claims, is made all the more relevant and urgent due to the current crisis of socialism caused by the collapse of the Soviet empire and the debilitation of European social democracy. In saying this Steger identifies his work with the genre of fin-de-siècle or endism. Relating his work to the various post theories of the recent past, such as postmodernism, post-Marxism, and postliberal democracy, Bernstein's evolutionary socialist quest is presented by Steger as the only workable vision for our postsocialist era.

This is the first major work on Bernstein in English since Peter Gay's *The Dilemma of Democratic Socialism* (1952). Writing at the height of the Cold War, Gay attempted to build up Bernstein as an alternative to Marx, as one whose legacy was simply ceded to Soviet communism. The dilemma to which Gay's title referred was in building a coherent democratic socialist theory out of Bernstein's writings that could match the power and success he conceded to the Soviet authoritarian model. Steger's book represents a similar ideological maneuver, in that we are supposed to link the collapse of the Soviet empire with the failure of Marxism. Once again, the specter of Bernstein is raised as providing an alternative to Marx and the only hope for socialist renewal. Instead of being the patron saint of Cold War social democrats, as in Gay, Bernstein is read by Steger as anticipating many of the concerns of post-Marxists, postliberal democrats, and communitarians.

Steger argues that his is a hermenutical, political, and historical work in that he wants us to understand Bernstein as the product of his era and of the emergent political circumstance of his age. I am in full agreement with him on this, but I would add, in agreement with L. Colletti, that Bernstein's views are not only incomprehensible but also irrelevant outside the context of the struggles within European Marxism in the pre-1914 era. Unless one is addressing the type of mechanical, fatalistic, and determinist Marxism at which Bernstein's claims were directed, his view offers little of consequence. Yet, even in this context, I would argue that Bernstein was neither an original nor a very profound thinker.

Steger wants us to see that, regardless of his own views, Bernstein did not so much revise Marx as adopt an alternative outlook. This is true, but what is even more interesting is that he did not so much develop an original argument of his own as simply borrow the ideas of non-Marxist thinkers, virtually without qualification, and concoct them into an eclectic stew of often conflicting claims. His economics derived from Fabian capitulations to neoclassicalism. As with the former, the latter's subjectivism, individualism, and anti-socialist premises were conveniently overlooked. His politics were an odd combination of idealist and utilitarian strains of liberalism, which took far too seriously the ideological self-image of nineteenth-century Britain as the vanguard of civilization and liberty. His philosophical positions, which Steger continually vaunts as a rejection of Marxism's Hegelian *residue*, were taken directly from the vulgarized neo-Kantianism of F. A. Lange, a charlatan whom Marx himself had exposed as having not the slightest idea as to the substance of the Hegelian dialectic or Marx's own critical reading of it (see Marx, *Letter to Kugelmann*, 27 January 1870). All of this Bernstein connected to an aggressive positivism that was as reductionist, deterministic, and scientistic as anything found among the orthodox Marxists he denounced.

A major example of Bernstein's theoretical poverty is found in one of his only original theses, that the concentration and centralization of capital would lead toward greater freedom and form the basis for the evolution toward socialism. Bernstein has been proven wrong on both counts. The tendency of capitalist concentration has become the hallmark

of the modern economy, with buy-outs, mergers, and government-subsidized monopolies playing the leading role in capital expansion and accumulation. Yet, these developments have shown no tendency in favor of spreading either democracy or socialism. Instead, the drive for profit maximization and capital accumulation has tended to enhance the priorities of privatization and the upward concentration of wealth. This is something that the most sophisticated Marxists (such as Luxemburg) have always understood.

Steger highlights a number Bernstein's interesting attempts at ideological compromises with the German bourgeoisie, most of which resulted in odd theoretical concoctions of dubious political and intellectual value. In the years just before World War I, he tried to appease middle-class opinion with a theory he called "socialist colonialism," which argued that workers and the bourgeoisie had a common interest in colonialism as members of a *higher civilization*, and it was necessary to tame savage cultures (p. 207). Steger explains this theory well but then takes the questionable step of comparing it favorably against orthodox Marxist theories of imperialism (i.e., Kautsky and Hilferding), simply because it was less antagonistic to liberal opinion.

A strong indictment of Steger's thesis is that he himself provides abundant proof that the majority of German liberals never wanted an alliance with the socialist movement and continued to make compromises with the forces of reaction. This, in fact, is the best element of his book. Despite his shortcoming in understanding Bernstein's theoretical failures, Steger shows himself a first-rate political historian in explaining the struggle between liberals and the SPD over the direction of German social reform between 1904 and 1922. He provides one of the clearest analyses available in English of how the victory of *Praktiker* (pragmatic opportunists) within the leadership of the SPD and the main currents of German liberalism helped to frustrate and destroy the hope of significant elements of the population for real reform (pp. 189–97). As a consequence, significant elements of the population became frustrated and drifted toward reactionary nationalism and chauvinism as substitutes for social justice and political equality. I recommend Steger's book expressly for those chapters in which he brings this material to life. I remain perplexed, however, as to how this could be used as evidence to support his case for Bernstein's evolutionary socialism/liberalism.

Bernstein himself did oppose the *Praktiker*, yet he did more to encourage this tendency than any other thinker. Steger realizes this and provides a fitting summary attesting to Bernstein's failings as a political thinker. Says Steger: "Bernstein had a poor eye for the tendency of reformism to breed technocrats who follow electoral imperatives and let their political activities be guided chiefly by self-interest and the immediate material concerns of their constituencies" (pp. 13–4). In admitting this, Steger should also accept that these flaws are inherent in liberal ideology and congruent with capitalist society. The principal legacy of Marxism is in making the case for socialism dependent on the transcending of liberalism and the capitalist system. The deficiency in Bernstein's work was in losing sight of this, and it remains the central flaw in this otherwise learned book.

Free Markets and Social Justice. By Cass R. Sunstein. New York: Oxford University Press, 1997. 416p. $35.00.

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In politics as in political science and legal scholarship, the world sometimes seems to be divided into those who think that for the sake of efficiency as well as justice markets must be free from regulation by morals and those who believe that, considerations of efficiency notwithstanding, justice demands that morals govern markets. In his instructive and admirably balanced new book, Cass Sunstein contends that, for all concerned, this is a bad way for the world to be divided.

Sunstein sets out to show the superiority of a third view: Markets and morals exercise a reciprocal influence on each other, and a respectable political science and a responsible jurisprudence must grasp the complicated relationship between them. In support of his thesis, Sunstein examines a remarkable range of ideas and issues: the ambiguity of preferences; the need to devise empirical measures of human well-being to solve problems of adjudication and public policy that arise in the modern welfare state; the complex origins, the pervasive influence, and the political regulation of social norms; why markets alone cannot put an end to discrimination; free speech issues raised by the internet; constitution making in Eastern Europe; the relation among property rights, democracy, and constitutionalism; neglected consequences and complicated trade-offs in the regulation of the environment and health; and the project of using the legal system to democratize America. In making his case, Sunstein is conscientious about introducing qualifications to his claims; drawing out problems of implementation inhering in his reforms, and identifying dangers associated with his programs, though occasionally he blurs the distinction between openly acknowledging a difficulty and responding to it by scaling back his theoretical ambitions or revising his political programs.

The two crucial and connected points, to which Sunstein returns again and again, are that markets are complex institutions and that morals are an irreducible element of social and political life. It is not exactly that there is no such thing as a free market or that morals are everywhere, but that free markets cannot be understood in isolation from beliefs and practices, especially beliefs about what is just and good for human beings and practices that prepare or prevent one from participating effectively in commercial, democratic society. Moreover, because they depend on and are partly constituted by law, free markets are not only more complex but also less autonomous than they sometimes appear. For example, the law of property provides rules of entitlement, and the law of contract establishes rules of transfer. And these fundamental legal rules are themselves not made by markets but, rather, play a constitutive role in making market relations possible and efficient.

Sunstein has a practical motive for bringing into focus the array of background conditions necessary to make markets work. He wants to show that current market thinking is insufficiently attuned to the harmful effects of background conditions that are unjust. But he makes this point as a friend of markets, and in the same spirit he seeks to show that current thinking about social justice is inadequately alert to the benefits markets confer. Indeed, for those who are inclined to see nothing but selfishness and exploitation in market relations, his message is that more appreciation must be mustered for the role that markets play in promoting the prosperity that is one of the key preconditions for a life of liberty.

Another leading theme of Sunstein's book is that legal controversies and disputes about public policy often turn on empirical questions that seldom receive the attention they deserve. Sunstein practices what he preaches, displaying a refreshing command of facts and figures. Although he occasionally disappoints by drawing back from making the prescriptions he thinks judges and government officials should be

capable of issuing. Sunstein does offer a respectable assortment of ideas for making the law, on the basis of better empirical research, more responsive to the claims of efficiency as well as social justice. For example, on the grounds that human well-being is to a significant extent capable of empirical measurement and should be taken into account in policy planning, regulatory reform, and democratic deliberation by the people, Sunstein recommends that "the American government should compile and distribute an annual 'quality of life' report, including among other things, per capita income, poverty, housing, unemployment, average weekly earnings, inflation, child mortality, longevity, subjection to violent crime, literacy, and educational attainment" (p. 123).

Sunstein, in short, envisages an active, energetic, and big government that both respects the requirements of the market and plays a major role in securing the material well-being, and promoting the autonomy, of its citizens. Given the precarious balancing act that he has set out to perform, it is perhaps not surprising that the lucidity with which he presses his cases sometimes works to obscure a certain slippage in his argument. So, for example, while he is right that much which is treated as natural or inscribed in the order of things is in fact a function of custom and law, he occasionally wrongly implies that beliefs and practices that can be shown to have a human origin or to have undergone an alteration in history are therefore proper objects for state action. Similarly, he sometimes argues that if a preference is indirectly or inevitably affected by law, then the law is justified in directly regulating it. But from the fact that law affects the formation of a preference it is invalid to infer that "it is *fully* legitimate for government and law to try to shape preferences in the right way, not only through education, but also (for example) through laws forbidding racial discrimination, environmental degradation, and sexual harassment, and through efforts to encourage attention to public issues and to diverse points of view." (p. 5, emphasis added). Gliding on occasion too quickly over the imperfect but necessary distinction between regulating behavior and regulating belief, Sunstein sometimes understates the importance of considerations that compel caution when it comes to putting government in the business of preference regulation: The cumbersome apparatus of government may be poorly suited to the complex task of forming preferences; even if it could form preferences effectively, government may be in a bad position to determine which preferences are the right ones; and using government to form directly the right preferences is rarely more than a short step away from using the organs of the state to supervise thought.

Sunstein himself emphasizes that inviting government to form the right preferences involves "serious risks of overreaching" (p. 20). In fact, in his discussion of "the project of norm management" (p. 59), he confronts the concern that using the law to make men and women moral invades individual liberty. He replies that government action designed to alter norms can be justified to promote liberal ends, such as material welfare or personal autonomy. Government, he stresses, should use the least intrusive means available, beginning with the simple provision of information and including campaigns of persuasion; taxing or subsidizing choices; imposition of time, place, and manner restrictions on particular activities; and, on some occasions, if necessary, straightforward coercion through legal prohibitions (pp. 56–7). Despite his forthright acknowledgment that the regulation of beliefs by government involves dangers, Sunstein in this case may not give the dangers their due. For while he is right to point out that a project is not invalidated just because it is liable to abuse, it also needs to be noted that to win support for a project it is not sufficient to show that in principle it may yield benefits. In short, Sunstein does not quite manage to dispel the anxiety that what he delicately refers to as "norm management" must be implemented and enforced by what others would be inclined to call "thought police."

Sunstein's enthusiasm for norm management is one expression of his commitment to using law to promote democracy: "Few matters deserve higher priority than institutional changes designed to increase the democratic character of the modern state" (p. 318). To this end, he proposes three general reforms which, he convincingly argues, would promote efficiency as well as democracy: more education, the replacement of command-and-control regulation with economic incentives, and decentralization.

At the same time, Sunstein's own extended analysis of the presuppositions and background conditions that make markets work leads one to wonder whether every "increase in the democratic character of the modern state" would be good for democracy. The powerful arguments that Sunstein himself develops to show that free markets depend upon legal regulations and social restraints and that market practices must always be evaluated with a view to "human interests" or "human purposes" suggest, contrary to John Dewey's principle, that more democracy is not always good for democracy.

Indeed, by refusing to treat free markets as ends in themselves and through the subtle assessment of markets' vices as well as virtues, Sunstein invites the thought that democracy, like markets, has vices as well as virtues, and more democracy, like more freedom in the marketplace, does not automatically guarantee greater efficiency or greater human well-being. His general approach also encourages the supposition that democracy is good in terms of the human purposes it serves and that sometimes regulation and restraint is what is required for democracy's defense.

America's Bachelor Uncle: Thoreau and the American Polity. By Bob Pepperman Taylor. Lawrence: University Press of Kansas, 1996. 180p. $25.00.

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Although the author says he was often exasperated with Thoreau, he seems to have cured himself of any lingering irritation. The attitudes that make others cross are downplayed. Even the appeal to an avuncular, indulgent identity, "America's bachelor uncle," provides a soft focus on Thoreau's wilder edges. Thoreau's political implications are both expounded and ardently defended, with the significant exception of some of the claims about "nature."

One finds intelligent, informed accounts not only of the classics, *Walden* and "Civil Disobedience," but also of several of the less well-known pieces as well. Ostensibly travel writing, *A Week on the Concord and Merrimack Rivers* is, Taylor shows, Thoreau's voyage back in memory to America's founding in bloodshed between Indian and white. This is not the usual preachy message. Taylor notes how Thoreau accommodated himself to the fierce virtues of morally ambivalent characters like the Indian fighter and hater, Captain Lovewell, or Hannah Dustan ("this awful woman," Hawthorne called her), who out of revenge for the murder of her child, killed and scalped ten Indians, including six children! Hawthorne's and others' "less satisfactory moral responses" are due, Taylor speculates, to their not having Thoreau's hardheaded realism or his sense of "the flow and brutality of history" (p. 30). In this century, of course, such "historicists" have ended up justifying terrible deeds. We might instead consider Thoreau an old-fashioned antiquarian: Greek in his

admiration for the *Iliad*, that ancient story of aristocratic warriors, as a model of American beginnings; Roman in a praise of America's rude founders that matches the spirit of Tacitus' contrast of the barbarian virtues of the Germans to Italian decadence.

Taylor organizes his chapters around five "representative types" found in Thoreau. In *The Maine Woods*, we have (1) defeated Indians and (2) the morally worse but spiritually superior conquerors, white frontier settlers. Economic ambitions led to the eradication of a frontier that made such distinctive virtues possible. This tale bears a remarkable likeness to Frederick Jackson Turner's own adaptation of Tacitus, a yarn in which Western settlers, instructed by both the "civilized" East and the "savage" frontier, become admirably independent characters tragically unable to reproduce themselves with the passing of frontier society.

In *Cape Cod*, we are shown (3) America's not always successful communal results of the puritan experiment in living, as Thoreau once put it, "with high and earnest purpose" (p. 55). The "idealistic" Cape villagers were cruelly indifferent to a shipwreck of Irish immigrants, given to cold bare-bones charity, distrustful of outsiders, and willfully ignorant of anything not part of the history of their own people. Oddly, Taylor seems defensive about the recurring charge that in such misanthropic accounts there was more than a little Nietzschean attitudinizing. Sure, I know Nietzsche came later, but both Nietzsche and the American transcendentalists were philosophical responses to a common heritage, German philosophical Idealism and, behind this, pietistic Christianity, both given to agonizing self-critical accounts of the radical good and evil in human nature. Like Nietzsche, Thoreau is self-critical enough to be ambivalent about the "good" and is tempted to claim that moral idealism masks cruelty and hypocrisy in comparison to which "barbarian" wildness looks healthy.

Taylor comes close to saying this himself about Thoreau, beginning with his claim for Thoreau's ambivalent admiration of Captain Lovewell and Hannah Dustan and continuing through the elaboration of his five types, including (4) the vigorously amoral capitalist (the subject, Taylor says, of *Walden*) and (5) the wild men of rebellion like the antislavery warrior, John Brown. Thoreau was not, we are assured, morally squeamish about "violent resistance to injustice" (p. 113). Brown is apparently "the model of the free American, a symbol of our national promise" (p. 107). I think we can see—though Taylor does not claim this—how appreciation for Brown is all of a piece with admiration for Captain Lovewell. As Thoreau wrote in his *Journal* (Taylor makes it the epigram to the chapter on Brown), "I wish my neighbors were wilder" (p. 99). Nevertheless, Thoreau's claim that nature was "an alternative source of inspiration" and instruction in "independence" disturbs the author. They were "not always appropriate attitudes" (pp. 127–8). But does not Thoreau's admiration for Brown's incendiarism require some such escape hatch from conventional moral judgment? Thoreau writes: "I love the wild not less than the good." Taylor finds this "startling" (p. 94). He valiantly strives to bring morality and nature together in Thoreau, but this collapses a major tension in the thinker's work.

Taylor gives us an old-time radical approach to *Walden*. Thoreau is now a programmatic anticapitalist "at war with the political economy of the age" (p. 91). Maybe, but as Taylor notes, Thoreau also finds the following virtues in such an economy: "energy, drive, ingenuity, and industry," along with "enterprise and bravery," which is "unexpectedly confident and serene, alert, adventurous, and unwearied" (p. 96). This last is about as elegant an update of the Homeric warrior as one could desire.

Taylor nods approvingly at a Rabbi Nahman tale in which the master of prayer rails: "Chasing money is 'not the purpose of life at all' . . . the purpose of life is to be engaged in Torah and prayer." Of course, Taylor concludes, the "moral equipment" of normal people is too "limited" for them to get the message (pp. 76–7). This is a deeply illiberal claim. It lacks respect for the autonomy of persons to choose their own good. Is Thoreau in the same authoritarian boat? He proclaims that the nation has been "ruined by luxury and heedless expense . . . the only cure . . . is a rigid economy, a stern and more than Spartan simplicity of life and elevation of purpose" (p. 122). As a recommendation that some try "voluntary poverty" (p. 91) this is all well and good, but as a political program, this could commend itself only to the old Khmer Rouge. Thoreau (and Taylor) complain: "There are very few halls for free worship and free speech in this country" (p. 85). America may be damned for many reasons, but surely not for this! In any event, if the capitalist economy is such a roadblock to freedom, is the alternative (as Taylor hints at but never quite says) still socialism?

Not despite my misgivings but because of them, I can recommend this provocative interpretation of Thoreau. It is eminently readable and opened my eyes to new ways of understanding a complex thinker, even if ultimately I departed from the conclusions of the author. Taylor is at war with the other commentators, and this, too, makes for lively reading. One, Nancy Rosenblum, is an often-cited foil to his views. It is a pity he never sets out more systematically either her claims or his considered response to them. A paper edition ought to do this as well as correct page 125, where Rousseau is the clearly intended reference, not Plato.

Republican Paradoxes and Liberal Anxieties: Retrieving Neglected Fragments of Political Theory. By Ronald J. Terchek. Lanham, MD: Rowman & Littlefield, 1997. 275p. $67.50 cloth, $24.95 paper.

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Surveys of contemporary political theory often dwell on the so-called debate between liberals and communitarians. Drawing—sometimes loosely—upon the tradition that is usually said to begin with John Locke's *Two Treatises of Government*, the former develop elaborate theories of rights to justify or criticize contemporary practices and understandings. The latter construct a critical response to the currently dominant language of rights from materials taken especially from "modern" (i.e., Machiavellian and Rousseauean) reconstructions of classical republican political practice. As a debate, this conflict is not particularly satisfying. On the one hand, liberals and their libertarian cousins continue to write and speak in terms of rights, largely ignoring challenges from the communitarian quarter. On the other hand, communitarians spend a great deal of time insisting upon the poverty of what they regard as a myopic focus on individual rights, at the expense of such considerations as the common good, citizenship, and virtue.

Terchek contributes to this debate by attempting to build a bridge between the two sides, contending that they can learn, not only from one another but also even more from their own traditions. Thus, he finds in the liberal tradition a concern with the kind of civic and personal virtue necessary to sustain a political order devoted primarily to the protection of rights and in the communitarian (or, more precisely, republican) tradition a hard-nosed and pragmatic concern with giving

individuals concrete stakes in their political order. Focusing on the works of "strong republicans"—Aristotle, Machiavelli, and Rousseau—and "anxious liberals"—Locke, Smith, and John Stuart Mill—Terchek attempts to bring these "fragments" of the past to bear on contemporary political questions.

On one level, he succeeds. The liberal and republican traditions offer rich materials for reflection, which have been mined only selectively by clever but ultimately shallow contemporary theorists. Liberals have paid insufficient attention to the "civic" dimension of their tradition, assuming that an appropriately rigorous theory of rights would be sufficient to guide an activist government—led, perhaps, by the judiciary—in the construction of a generous welfare state. Recent work (see, e.g., Lorraine Smith Pangle and Thomas L. Pangle, *The Learning of Liberty*, 1993) has reminded us of how much serious thought liberals once gave to the question of civic education. Unlike contemporary liberals, who may well be overconfident about the capacity of people to listen to reason and heed the lessons of theory, "anxious liberals" were well aware of our capacity to abuse the rights we have gained or been granted. Communitarians, in turn, have often been "high-minded" in their emphases on moral virtue and common traditions, writing as if the material concerns of ordinary people were of little moment and as if "tradition [could] serve as the foundation, superstructure, and glue of the good society" (p. 50). Even a cursory glance at the works of "classical republicans" would correct this oversight; it would be impossible to assume that someone like Machiavelli overlooked the role of self-interest in politics.

This is not to say, however, that Terchek's contribution to and correction of the contemporary discourse is altogether satisfying. The fragments he draws from the republican and liberal traditions may well serve as correctives to the oversights and excesses of the two contemporary camps, but they are more politically salutary than theoretically adequate. Thus, for example, Terchek's account of Machiavelli the republican (heavily influenced by Pocock's *The Machiavellian Moment*, 1975, and Skinner's *The Foundation of Modern Political Thought*, 1978) shows little evidence of a careful critical encounter with *The Prince*, let alone a serious effort to reconcile the teachings of that work with *The Discourses*, an ostensibly more "republican" book. Where one might expect an emphasis on the role of ambitious political leaders, even in the sectarian or partisan politics of republics, one gets instead a much less plausible account of how the people more or less spontaneously rise to assert or protect their liberty (see, e.g., pp. 104–7).

A similar flaw mars Terchek's account of Locke. One difficulty with which any student of the *Second Treatise* must come to grips is to explain why there would ever be a need to abandon the state of nature for civil society, if our natural reason were such an effective guide to leading our lives together. Terchek offers a very ingenious explanation, reminiscent of Rousseau's *Second Discourse*, focusing on the way in which the invention of money (something that, at least in his account, seems accidental) opens up possibilities that exceed the rational control of ordinary human beings (see pp. 132–4). While this version of events paves the way for his argument about the roles of education and Christian culture in enabling us to reassert rational control over our impulses (pp. 134–7), it overlooks evidence in the *Second Treatise* itself that would point in a different direction. Indeed, there are plentiful indications of the frailty of human reason even before money arrives on the scene. Why, for example, talk about enforcing the law of nature and the problems posed by "the boundless extravagancy of . . . will" (see, e.g., *Second Treatise*, paras. 7–8, 11) if everyone so readily listens to his or her reason? For those who attend to the hints early in the work, the later claim that human beings are in general no great observers of the law of nature (*Second Treatise*, para. 124) comes as much less of a surprise than it would to one who followed Terchek's account. An interpretation that relied somewhat less on the actual or potential strength of reason would also pay greater attention to the role that might be played by an appeal to particular passions, such as fear and the desire for comfortable self-preservation, in the establishment and maintenance of a civic order. While this would not diminish the power of Terchek's insistence upon the importance of education and Christian culture in supporting liberalism, it might shed more light on the character of both the education and the religion that Locke's liberalism requires. In sum, a more careful reading of Locke might yield a richer and more nuanced—although politically less immediately useful—account of the anxieties to which thoughtful liberals address themselves.

It is tempting to conclude that Terchek is to some extent guilty of the same fault with which he taxes contemporary liberals and communitarians. While his interpretation of the traditions emphasizes different elements than they do, he, no more than they, enables us to learn everything the traditions may have to teach us. Whenever the concerns of contemporary political discourse are preeminent in our approach to our philosophical heritage, we hear what we want to hear and see what we want to see. We may be edified and edifying, but we do not truly confront the challenges posed by the thought of the past. By inviting us to return to the past, Terchek has done us a service. But we need a deeper immersion in those texts and traditions than he offers us.

Social Justice in a Diverse Society. By Tom R. Tyler, Robert J. Boeckmann, Heather J. Smith, and Yuen J. Huo. Boulder, CO: Westview Press, 1997. 305p. $60.00 cloth, $24.00 paper.

Grant Reeher, *Syracuse University*

This book is a cumulation and synthesis of extant social psychological research on the beliefs and feelings about social justice held by persons as individuals and as members of groups, as well as the behavioral effect of those beliefs and feelings. It would be useful as a reference guide for political scientists conducting research or teaching in social justice, from either a theoretical or empirical perspective. It also would be useful as a text in graduate courses about or related to justice and might even serve well in an upper-level undergraduate course.

The authors begin with the claim, thoroughly substantiated, that beliefs and feelings about justice matter both for other important beliefs and feelings and for behavior: "Judgments about justice mediate between objective circumstances and people's reactions to particular events or issues" (p. 6). And these judgments matter in an enormous variety of contexts, which include "almost all settings in which people interact with one another, either as individuals or in groups" (p. 9).

The findings are organized around two overlapping categories: four historical "waves" of justice research and five key topics concerning justice research writ large. The four waves are relative deprivation, distributive justice (in terms of outcomes, mostly of goods, pay, and promotions), procedural justice (both resolving conflicts and making allocations), and retributive justice. The five topics are the ways in which judgments about justice influence other feelings and atti-

tudes; the criteria employed to assess justice; the behavioral consequences of perceptions of justice and injustice; the motivations behind caring about justice; and the circumstances under which concerns about justice matter most. Though the book seems intended for audiences beyond the United States, most examples come from this country. The authors present much useful material on all these waves and topics. Indeed, perhaps the book's main strength is its extensive review of the research; the references section is 35 pages of small type.

Although overall substantive themes are somewhat difficult to tease out of the book, for reasons described below, three in particular struck me as the most important. First, social justice matters to people not simply for rational, self-interested reasons, and their views about justice are not simply rationalizations for self-interest. This casts further empirical doubt on rational choice models of human motivation and behavior. Second, whether people think of themselves as individuals or as members of groups is critical in their justice judgments and their tendency to act upon them. Third, in general, judgments about justice, and their behavioral influence, are extremely complicated; there are a number of intervening variables in any given situation.

At times the authors dwell too much in the details of the research they report. The book would have benefited from a higher level of synthesis built around overarching themes and arguments. Even within chapters, the organization seems to be driven more by the particular research projects described than by an overall conceptual scheme.

I have other quibbles. First, the title, *Social Justice in a Diverse Society*, suggests something different from what is actually delivered. The topic of multiculturalism, diversity, and social justice is not directly addressed until the last 16 pages.

Second, the influence of socioeconomically based political power on the subjective evaluations of justice is underemphasized. The use of this kind of power by the advantaged on the disadvantaged influences not only the content of justice beliefs but also the perceptions of when justice claims are appropriate, the conceptions of self and others that figure importantly in justice judgments, and the tendency to act on those judgments. Perhaps this underemphasis is due to the social-psychological versus political science focus of the work, or to the heavy reliance on experimental research. Nonetheless, it is a problem that haunts almost the entire book. The book is somewhat stronger in this regard when it treats the behavioral consequences (and nonconsequences) of justice beliefs.

Third, the effectiveness of the book is lessened by several instances of careless reasoning and use of substantiating examples. I offer just a few samples here, from the chapter on retributive justice: On the incompleteness of equity as a principle in retributive justice, the authors invoke the phenomenon of returning a punch with a harder punch; on the limits of using compensation when a crime involves "immoral behavior," the authors invoke the relatively greater grief over having irreplaceable personal items stolen from a home rather than replaceable impersonal items of monetary value; on policies toward rule breaking, the authors state that spending on prisons makes society more poorly educated because prisons are funded by the same funds spent on public education; on the notion of "global association" in moral responsibility, the authors invoke the Israeli practice of dynamiting the homes of the families of terrorists, without considering deterrence effects. There are similar instances of these problems in other chapters.

Despite these drawbacks, I still find this book to be an important and useful reference source for political scientists working on justice. It will remain on the shelf next to my desk.

Randolph Bourne and the Politics of Cultural Radicalism. By Leslie J. Vaughan. Lawrence: University Press of Kansas, 1997. 268p. $35.00.

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In the raging controversies of our time over issues of social and political identity, difference, and cultural pluralism, currency often trumps historical memory. To be sure, it would be foolish to dismiss the significance and relevance of various postmodern perspectives at this fin-de-siècle moment. Still, much of the contemporary debate on American culture and its connection to political radicalism and social protest has been confined to a historical context that rarely reaches farther back than the 1960s.

In her timely study of the legendary political essayist Randolph Bourne, Leslie J. Vaughan, a frequent contributor to such prominent publications as the *Atlantic Monthly*, the *New Republic*, and the *Masses*, and leading critic of U. S. military involvement in Europe, fortunately bucks this trend. She skillfully expands the narrow historical boundaries of the present American political discourse on multiculturalism, localism, and the limits of liberalism by locating its roots in the "little rebellion" that swept New York's Greenwich Village in the decade preceding World War I. Characterizing the hunchbacked and self-identified "outsider" and "social misfit" (who died poor and lonely in 1918 at the age of 32) as "a confirmed modernist who anticipated the 'post-modern,' " Vaughan seeks to show that Bourne's thought is valuable for today's discussions "precisely because it offered a new political discourse and set of cultural possibilities for American society at the height of the modern age" (pp. 4–5).

Rather than relying on a conventional biographical format that adheres to a strictly chronological framework, the author chose to organize her book along thematic lines. After three short introductory chapters, the book presents Bourne's life and work as texts to be interpreted within the political economy and psychosocial history of their time. Chapter 4 assesses Bourne's use of irony as a stance of political engagement, skepticism, and commitment to radical dissent that reflected his unwillingness to resolve irreconcilable theoretical contradictions. Chapter 5 deals with Bourne's "philosophy of youth," particularly as it emerged in his confrontation with Walter Lippmann, who, in his famous *Drift and Mastery*, defended the scientism and positivism of the new professional business administrators in charge of streamlining production and rationalizing employment practices. In a particularly readable section of this chapter, Vaughan offers an innovative interpretation of Bourne's arguments in favor of feminizing American culture. Chapters 6 and 7 revisit Bourne's famous opposition to the Great War and its virulent evocations of ethnonationalism, famously captured in his witty observation that "war is the health of the state." Seeking to reposition Bourne's thought at the center of debates about the nature and limits of American liberalism, Vaughan offers an insightful reading of his "prefigurative politics" of a "democratic transnationalism" as a variant of a "cultural politics beyond militarism, and importantly, beyond liberalism" (p. 141). In the final chapter, Vaughan elaborates on her most ambitious and controversial thesis, suggesting that Bourne's turn to cultural issues did not signal a retreat from political engagement but, to the contrary, represented an intrinsic part of his political theory. While she concedes that Bourne's cultural radicalism may be interpreted as

merely offering a "creative politics worked out in the unmapped space of the impossible," she confidently insists that his close attention to cultural issues made "sense immanently as a political critique" (p. 144). In other words, like the subject of her biography, Vaughan rejects the liberal bifurcation of culture and politics, claiming that "cultural solutions could stimulate political responses . . . for the transforming influences of architecture and city planning" (p. 171).

On this issue, the author seeks to defend Bourne against the dominant views of Bourne scholars that emerged in the 1960s with Christopher Lasch's influential study, *The New Radicalism in America 1889–1963: The Intellectual as a Social Type* (1967). Lasch cast Bourne and his fellow "middle-class radicals" as politically alienated neophytes with an infantile craving for oceanic wholeness and unity who subscribed to the naïve radicalism of a "personal politics," which soon ended as an embarrassing retreat into "despair" and "distance" (see also Michael Walzer, *The Company of Critics*, 1988). Conversely, Vaughan argues that Bourne's political temper, although largely anti-institutional, was neither defeatist nor irredeemably negative, but that it represented a viable position from "below the battle." In it she finds a resemblance to a Nietzschean "creative skepticism" that consciously moves between the "is" and the "ought" without dissolving tensions. Indeed, throughout the book, the author highlights the Nietzschean influence on Bourne, apparent in his insistence that, more than the Apollonian will to rationality, order, and self-control, a vital American culture requires the Dionysian impulse toward chaos, creative desire, spiritual adventure, and sinister imaginative excursions.

Yet, Vaughan's attempt to repoliticize Bourne's cultural radicalism reveals the sole systemic weakness of the book: the lack of historical evidence for her central claim that his cultural critique related intimately to actual social and political practice. This is not to say that such evidence does not exist; but beyond some vague references to "neighborhood-based politics of halfway houses, settlements, experimental schools and cooperatives," Vaughan offers little proof that Bourne's call for the pursuit of creative alternatives to impersonal bureaucratic politics touched the lives of working people in tangible ways. For example, did Bourne entertain close relationships to radical political movements such as the Industrial Workers of the World or other anarchist groups? Did he actively participate in radical political action? Did he involve himself in party politics? Unfortunately, Vaughan keeps her readers in the dark about these important political questions, although Bruce Clayton's excellent biography (*Forgotten Prophet: The Life of Randolph Bourne*, 1984) suggests that Bourne was deeply influenced by workers' struggles in the early decades of the twentieth century, such as the long, bitter strike of silk workers in Patterson, New Jersey, and the bloody Wobbly strike at Lawrence, Massachusetts. In order to give her thesis some teeth, Vaughan should have moved beyond rather general statements, noting that "Bourne's political commitments were constantly in flux, contingent on specific circumstances but relying on 'intellectual suspense' to test them critically" (p. 169), and instead have shown more concretely how Bourne applied those "political commitments" in his short life.

Overall, however, *Randolph Bourne and the Politics of Cultural Radicalism* clearly is an important contribution to the study of modern American political thought. Vaughan's excellent grasp of the workings of psychosocial dynamics at the dawn of the twentieth century, combined with her lucid exposition of the connections and tensions among American liberalism, multiculturalism, aesthetics, and democracy, make her book valuable to both a general audience and specialists in the discipline. The greatest virtue of Vaughan's study lies in her expansion of the parameters of the current cultural debates to include important, but often sorely neglected, American political thinkers who stood at the cusp of the modern and postmodern worlds.

The Career of Toleration: John Locke, Jonas Proast, and After. By Richard Vernon. Montreal and Kingston, Canada: McGill-Queen's University Press, 1997. 164p. $44.95.

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Intellectual history and political theory are hard to differentiate, and when each is done well, they are mutually supportive. At their core, however, are goals which distinguish them and place their aspirations in tension. Intellectual history is primarily concerned with understanding the historical thought of various authors, whereas political theory seeks to extract from them enduring political themes and conceptual maps. The underlying tension between these two ways of understanding is increased when the contemporary student employs the abstract tools of analytical philosophy in his or her exploration. Such tensions are further heightened when the theorist hopes to find historical lessons apposite for contemporary liberal political life.

Notwithstanding these tensions, Richard Vernon uses the tools of analytic philosophy to extract from a seventeenth-century political-theological debate between John Locke and Jonas Proast (a theologically orthodox chaplain at All Souls College) lessons he believes useful to late-twentieth-century liberalism. As he writes in the Introduction, his aims are "to reconstruct the grounds of [Locke's and Proast's dispute, and] . . . draw attention to the long-term importance of some of their arguments and to evaluate their relative strength," so as to illuminate better the contemporary debate between liberals and academic communitarians (pp. 4, 15). Vernon believes that to ignore the continuing relevance of the Locke-Proast exchange would be "to impose an artificial barrier between the late seventeenth-century and ourselves, and, moreover, to miss the way the seventeenth-century political debate continues to influence and shape our own" (p. 15).

His argument is divided into the Introduction, seven chapters, and a brief but rewarding conclusion. He opens with a rich discussion of belief and its relationship to the will as well as the role of compulsion in shaping belief. Next, he broadly considers the character of "public reason" as found in the thought of Kant, Rawls, Gerald Dworkin, Nagel, and indirectly Locke. In a particularly strong chapter, the third, Vernon draws out the relationship between tolerance and skepticism (both philosophical and political). The fourth chapter considers rather abstractly the "slippery slope" and "can of worms" arguments and then selectively applies them to the Locke-Proast debate. The fifth chapter, in which Vernon wishes to test Stephen Holmes's thesis concerning the enduring structure of illiberal thought, is the least successful but one in which he still manages to provide some thoughtful asides on the Christian character of Mill's thought (p. 103). In chapter 6, Vernon compares Locke's defenses of toleration to that of Berlin and Raz and finds Locke's to be superior. Chapter 7, in which the liberal character of contemporary "deliberative democratic" thought is considered and compared with Locke, is arguably Vernon's strongest. Finally, in the Conclusion, Vernon considers Dunn's remark that he " 'cannot conceive of constructing an analysis of any issue in contemporary political theory around the affirmation or negation of anything which Locke says about political matters' " (p. 143). Vernon recognizes the Protestant partic-

ularism of Locke's thought but nonetheless finds, in opposition to Dunn, that Locke's case for toleration can be usefully extracted from its original Protestant context.

Vernon fails to achieve much of either of his declared goals. He neither illuminates the nature of the seventeenth-century debate between Locke and Proast nor shows how their debate can clarify the debate between liberalism and communalism, either with contemporary communitarians (who are never discussed after the Introduction) or with traditional conservatives (who are treated in a most perfunctory manner). What he does do with gusto and always with intelligence is argue for the superiority of a Lockean liberalism to contemporary liberal alternatives. In particular, his criticism of Amy Gutmann and Dennis Thompson for their misappropriation of Locke for putatively democratic ends deserves careful consideration. Reading this book, then, is less like engaging a sustained examination of the seventeenth-century Locke and Proast or even contemporary liberalism and more like having an extended conversation with an intelligent and engaging colleague, with John Locke and contemporary liberalism forming a persistent background. In the process, your understanding of both of these is sure to be advanced and challenged, but in an unsystematic manner that at times will prove frustrating. By contemporary intellectual standards, however, this is nonetheless an inviting opportunity.

Vernon's problems are largely self-imposed. Because he wants to use Locke's version of liberalism to do battle with other contemporary liberals, Vernon is forced to minimize the differences that separate the seventeenth and twentieth century. Thus, until the conclusion, he avoids drawing attention to the theological assumptions that undergird Locke's defense of toleration, and this necessarily detracts from the historical accuracy and value of his considerations. Conversely, his final willingness to recognize that Locke's position rests on critical theistic (if not Christian) foundations, along with the selective nature of his attacks on prominent liberal theorists, limits the power of his contemporary case. Surely, then, with a more coherent presentation, without the pretense of illuminating the actual seventeenth-century debate, and a fuller recognition of the difficulty of extracting a secular "Lockeanism" from its theistic or Christian foundations in John Locke, Vernon could have accomplished much more in advancing the case for a Lockean-derived liberalism.

Yet, there would still remain good reasons to follow Dunn in questioning how "useful" the historical Locke is to contemporary secular theorists. For example, what are we today to make of a theory of toleration that rests on theological grounds which even Locke would have been unable in his own day to defend on rational grounds, that is, if anyone had asked? Vernon fails to note that Locke's case for the inviolability of each individual's religious conscience rests on Locke's belief that the only kind of adoration acceptable to God is devotion freely given and that, accordingly, each individual is responsible for negotiating his or her own salvation.

Indeed, so much of Locke's argument for toleration turns on his unorthodox Protestant beliefs about the nature of salvation, its centrality to a good human life, and what is needed for its achievement that it is hard to accept Vernon's claim that Locke's own views of rights and toleration can readily be extracted from their original Christian foundations. He concludes, nonetheless, by holding that "although [his Protestantism] clearly is the most basic fact about Locke, it is not, as far as his theory of toleration is concerned, ultimately essential to understanding what the theory entails" (p. 144). Thus, the essentially Protestant-inspired character of Locke's thought is finally recognized but neither explored nor found to be delimiting. But is not this to be expected? How else could Locke's teachings be made fully accessible to those who deny God's existence and normative ordering of the universe or, at the very least, do not share Locke's faith and confidence concerning what pleases God and what is necessary for salvation (*sola fide*) and eternal life at His side?

Vernon also consistently makes other assumptions, following Locke, regarding human nature and what is needed for human flourishing. Humans, at their essence, are decision makers, although imperfect ones. And as such, Vernon finds that much in Locke's selective Protestant perspective of the limitation on public or political morality should enjoy broad secular support. The private sphere, in light of Locke's and Vernon's view of human beings, is therefore to be much protected, and public life is to be greatly limited in its reach and aspirations. But what goes unconsidered is the possibility that human flourishing is best achieved in ways different from those Locke suggests. And just as Locke may be wrong about Christianity and God's predilections, he also may be wrong about the necessary private nature of moral activity. In short, a defining feature of community and public life goes unconsidered by Locke and, although having promised to do so, is similarly overlooked by Vernon. In an examination of only liberalism, one need not consider this alternative; but given that Vernon set out to compare the conservative thought of Proast to Locke's liberalism and to elucidate contemporary communitarianism, one rightly expects a fairer treatment of the communal side.

This promised two-sided debate never materialized and seems not, in truth, to be the essential goal of this book. More likely, Vernon wished to defend a version of liberalism based on certain guidelines discoverable in Locke against alternative forms of liberalism. And toward this goal, Vernon has made a most respectable beginning.

American Politics

Renewing Presidential Politics: Campaigns, Media, and the Public Interest. By Bruce Buchanan. Lanham, MD: Rowman & Littlefield, 1996. 209p. $57.50 cloth, $19.95 paper.

Emmett H. Buell, Jr., *Denison University*

This book derives from Buchanan's work with the John and Mary Markle Foundation on media coverage of presidential politics. It should appeal to critics of the often mean-spirited, dishonest, and vapid campaigns for the presidency waged in recent years, as well as those concerned about the power of an increasingly judgmental press. Many of Buchanan's points ring true, especially those pertaining to the news media, but eventually even the most receptive reader will likely find the argument flawed and the proposed solution implausible.

The central problem of modern presidential campaigning, according to Buchanan, is that the candidates, their handlers, and the two major parties are driven by perverse incentives to regard voters as fools and to seek partisan advantage at the expense of resolving major problems of public policy. Rather than persuade likely voters with logic and facts, presidential campaigns manipulate them by caricaturing the opposition in unfair attacks, by appealing to prejudice and other base

emotions through demagoguery, by misrepresenting policy options or otherwise avoiding disclosure of their true positions, and by pandering to voters' worst instincts. Buchanan regard television as especially conducive to such tactics. This unrestrained competition harms the public interest by repelling independents and depressing turnout among the least informed and most apathetic. Buchanan clearly believes that contemporary campaigns delegitimate the electoral process for millions of potential voters and thereby corrode the foundations of American democracy. The news media only exacerbate these problems by fixating on the horse race at the expense of substantive coverage and by exaggerating the degree of conflict even beyond what already occurs.

These failings lead Buchanan to ask whether "good campaigns can somehow be made to happen, and bad campaigns be avoided, without the unpredictable assistance of galvanic personalities or economic crises" (p. 18). A campaign is "good" to the extent that it "repatriates the lower orders" and thereby heightens turnout to a level closer to that of other Western democracies (p. 30). Likewise, a good campaign induces voters to resist the standard tactics of manipulation, negativity, deception, and pandering. The more voters are motivated to inform themselves, Buchanan avows, the less they rely on Downsian shortcuts or simple schema to make up their mind. This also obliges the press to adjust its coverage accordingly. None of these improvements will occur, he maintains, if left to the unchecked instincts of candidates obsessed with winning or to the restraint of media organizations attentive only to process and conflict.

Having conceded in several places that we cannot abridge the freedom of the press to report the horse race, of candidates to campaign badly, of citizens to minimize their political obligations, and of voters to stay at home or employ shortcuts in deciding whom to support, Buchanan maintains that good campaigns will come about only when more voters regard themselves as "owners" of the electoral process rather than as spectators or passive participants. To initiate this transformation a "small cadre" of political independents must take charge. These elites should be "averse to negative partisan politics," exhibit sufficient stamina to keep up with the relevant facts, foster a spirit of national community, and set out clear standards for the media, candidates, and voters to follow (pp. 175–6).

Buchanan's proposed resolution seems unlikely to travel far beyond the foundation world, and his reliance on a civic-minded elite to empower the least active elements of the potential electorate is hardly the only inconsistency in this book. Another is his celebration of the 1992 presidential campaign as decidedly superior to that of 1988 because voters in 1992 focused on the economy rather than on such distractions as the flag salute and harbor pollution. Several chapters later, however, we learn that the content analysis of 1992 press coverage commissioned by his foundation showed that the media repeatedly misrepresented the state of the economy by not reporting clear signs of recovery. In other words, the media helped Clinton manipulate the basis for retrospective assessment of Bush's economic performance.

It is not too great an inferential leap to hold that 1992 really appealed to Buchanan because the Republicans dared not wage another Willie Horton campaign. Indeed, he treats all race-related issues as interchangeable with the symbolism of Willie Horton. Race, he maintains, should not be evoked even indirectly in a campaign if it crystallizes partisan support instead of evoking "some authentic exploration of future policy options" (p. 82). Later (p. 111) he argues that issues with racial "implications" should not be raised when they exacerbate social tensions or divert attention from the "clearest and most useful conceptualization" of race relations.

Donning these shackles would virtually rule out the discussion of race in any form that offended any number of citizens. One wonders whether the public interest would be served by avoiding the most enduring question of American politics for the sake of not giving offense. Would that rule have served the public interest had it been somehow enforced in the 1950s and 1960s? Should it be followed today even though most Americans oppose racial preference in employment, college admissions, and awarding of government contracts? Is a clear standard like that set out in California's Proposition 209 to be dismissed because a minority deems it not useful?

For an argument based on the proposition that voters should not be treated as boobs, Buchanan either ignores or discounts a substantial literature on voting, especially retrospective voting, showing that voters, in V.O. Key's words, "are not fools." Only a footnote acknowledges that abstention may make sense for many voters, and even it is wholly innocent of Downsian rationality. Instead, we get a Cloward and Piven argument that low turnout is ipso facto bad and likely due to elite efforts to keep it low. The credibility of this claim is not well served by the fact that, in this age of postcard and motor-voter registration, we have just experienced the second lowest turnout of the century in a presidential election. A more balanced and comprehensive analysis would have better served Buchanan's case.

(Dis)Entitling the Poor: The Warren Court, Welfare Rights, and the American Political Tradition. By Elizabeth Bussiere. University Park: Pennsylvania State University Press, 1997. 222p. $26.50.

Wayne D. Moore, *Virginia Tech*

According to the author, "the central argument of this book is that the Warren Court declined to 'constitutionalize' a right to welfare primarily because of the impact of legal doctrines" (pp. 157–8). More specifically, Elizabeth Bussiere claims that the Warren Court was boxed in by the "double standard" that it inherited from its predecessors, especially the Stone Court. The earlier Court's separation of "economic justice" from "individual rights" in connection with repudiating *Lochner*-era precedents and announcing a new interpretive framework in footnote 4 of *Carolene Product* (1938) purportedly "unleashed new and unforeseen problems for its successor, the Warren Court" (p. 121). Despite the justices' "liberal persuasion," efforts to establish "a constitutionally guaranteed right to welfare" were "doomed" (p. 116).

Much of the book, however, supports an opposing conclusion. Chapters on artisan republicanism and natural rights in Jacksonian America and on "maternalist" movements for pensions in the Progressive Era are designed, among other things, to rebut claims that the justices' denial of welfare rights was "an inevitable by-product of an inegalitarian market society" (p. 157). This line of reasoning depends on a premise that the Warren Court might have chosen a different course from the one it did, and Bussiere occasionally suggests as much. A chapter on the New Deal explores linkages between positive economic rights and constitutional guarantees, implicitly suggesting doctrinal bridges to alternative conceptualizations of welfare rights. A chapter on the Warren Court's "New Equal Protection" concludes that in the late 1960s AFDC mothers had "reasons for optimism" because "the Warren Court had shown itself to be hospitable to poor people's legal grievances" (p. 98). The two chapters

reviewing the Court's refusal to enforce a substantive constitutional "right to life" point toward viable alternatives to the majority's decision in *Dandridge* v. *Williams* (1970) and similar cases. The book's conclusion refers to general changes in "the political winds" during the 1970s that might have influenced the Court's decision (pp. 156–7). Bussiere suggests in closing, moreover, that "the language of maternalism might have served welfare recipients well," if only those advocating their cause had been willing to make such an appeal (p. 164).

Considering these internal tensions, and for reasons explained more fully below, the book's central causal claims strike this reviewer as overstated. *(Dis)Entitling the Poor* persuasively demonstrates that judges in the 1960s and 1970s were *constrained* by predominant norms of constitutional adjudication. It is not clear, however, that the Warren Court's refusal to enforce a substantive "right to life" was based *primarily* on doctrinal considerations.

In any event much of Bussiere's book is instructive independently of its primary causal claims. Self-consciously aligning itself with public law scholarship within the genre of the "new institutionalism," the book weaves together descriptive and normative analysis. Following a version of the methodological strategy suggested by Rogers M. Smith ("Political Jurisprudence, the 'New Institutionalism,' and the Future of Public Law," *APSR* 82 (March 1988):89–108), *(Dis)Entitling the Poor* deals with four main sets of issues. First, the book treats patterns of judicial reasoning as "structures" or "institutions" that independently affect political choices. Second, it places current practices within historical context. Third, Bussiere's interpretive narrative focuses on a selection of judicial decisions that she treats as exemplary. Fourth, she explores ways that judicial decisions shaped constitutional doctrines, which in turn constrained further deliberations. (Cf. Smith 1988, 101–3.)

Viewed in relation to these objectives, Bussiere's book is both ambitious and thoughtful. It contains solid scholarship and contributes substantially to the existing literature on welfare rights. More generally, this work informs analysis of relationships between patterns of constitutional rhetoric and judicial decision making.

But the book's contribution to understanding the constitutional politics of welfare rights falls short in at least two significant respects. First, Bussiere does not adequately explore the plausibility of alternative decisions in the 1970s or since then. This omission is especially troublesome because the initial chapters lay the groundwork for critical analysis of this sort. More generally, Bussiere leaves largely unexamined the ways that earlier conceptions of welfare rights call into question her equation of constitutional meanings with the Constitution's *judicial* interpretation and enforcement. Again, this omission is peculiar because the materials she uses to exemplify conceptions of welfare rights based on arguments from natural rights, republicanism, and maternalist perspectives direct attention primarily to how rights of the poor may be secured through *legislation*.

For a book that depends so heavily on claims that constitutional doctrine has been politically determinative, the analysis of judicial interpretive precedents is disappointing. For example, Bussiere characterizes the logic of footnote 4 of *Carolene Products* as committing the justices to scrutinize procedural fairness in the making of laws. Although the footnote has received important proceduralist glosses, it is not clear that the footnote as a whole—and especially its first and third paragraphs—is concerned only (or even primarily) with lawmaking procedures. More important, Bussiere does not adequately account for judicial decisions since the New Deal—most notably *Roe* v. *Wade* (1973)—that are difficult to square with various conceptions of the famous *Carolene* footnote and its underlying principles or with a general commitment to "proceduralism." Exploring this type of issue would lead, among other things, to questions about why the judges sought to guard some substantive constitutional liberties but not others, including the kinds of welfare rights of concern to Bussiere. How were the doctrinal hurdles overcome in some cases but not others?

These issues might be approached, alternatively, through a more thorough examination of dissenting arguments or scholarly literature advocating a greater role for judges guarding welfare rights. If judicial doctrine was so confining, how could such arguments be maintained coherently? Or have these out-of-favor arguments been fundamentally misconceived? (Cf. Mark Graber, "The Clintonification of American Law: Abortion, Welfare and Liberal Constitutional Theory," *Ohio State Law Journal* 58(October 1997):731–818.)

Bussiere also misses opportunities to explore problems of institutional capacity. The book (p. 150) quotes Justice Potter Stewart's comment in *Dandridge* v. *Williams* (p. 487) that "intractable economic, social, and even philosophical problems presented by public welfare assistance programs are not the business *of this Court*" (emphasis added). Stewart did not deny that welfare rights are properly the business of *other government officials*. On a related matter, Bussiere claims that "the Roosevelt Court's thoroughgoing repudiation of natural-law reasoning rendered a 'natural' right to subsistence very flimsy as a matter of constitutional interpretation" (p. 149). Neither President Franklin Roosevelt nor the justices denied, however, that legislative and executive officials could play important roles in securing constitutional rights, including those conceived as "natural" in character. On the contrary, much of the president's case for the New Deal, as summarized in chapter 4 of *(Dis)Entitling the Poor*, may readily be understood as resting on "naturalist" reasoning. Roosevelt's argument that rights of the poor should be secured through legislation, moreover, echoed earlier workingmen's and maternalist arguments for *legislative* solutions to problems of economic inequity. In this respect the New Deal precedents complement rather than contradict Bussiere's examples from the Jacksonian and Progressive eras.

The Warren Court's refusal to articulate and enforce a sweeping constitutional right to subsistence was doubtless politically significant. As Susan E. Lawrence (*The Poor in Court*, 1990) has pointed out, however, decisions like *Dandridge* v. *Williams* have not preempted innovations by the legislative and executive branches for the benefit of the poor. R. Shep Melnick (*Between the Lines*, 1994) has emphasized, moreover, ways that public support for welfare rights has been greater for programs supported by the more electorally accountable branches than for those announced by judicial fiat. He has also suggested ways that legislation securing welfare rights implicates fundamental problems of constitutional meaning and structure. Bussiere has done the field of constitutional studies a service by casting additional light on these issues and underscoring their importance.

Cheap Seats: The Democratic Party's Advantage in U.S. House Elections. By James E. Campbell. Columbus: Ohio State University Press, 1996. 336p. $60.00 cloth, $22.00 paper.

Mark E. Rush, *Washington and Lee University*

This is unquestionably a book that any scholar of American politics and, perhaps, electoral systems more generally ought

to read. It presents clear evidence that the Democratic Party has benefited from recurring advantages produced by the single-member district system of elections. *Cheap Seats* is clearly written and presents equally clear, straightforward data analysis that is accessible to even the less quantitatively oriented student of politics. That having been said, this reviewer must take issue with some of Campbell's conclusions and analysis. Campbell's data do not lie, but I do not see that his conclusions follow clearly from his analysis.

Campbell seeks to analyze why the Democrats managed to dominate congressional elections from 1954 to 1992. He attributes this to their control of "cheap seats," that is, those in which turnout is quite low and, therefore, candidates need to garner fewer votes for victory. Throughout, he carefully presents evidence and constructs an argument showing that Democrats do, indeed, benefit from their control of a disproportionate number of cheap seats. Perhaps the most damning evidence he presents is in Table 5.2, where he shows that aggregate turnout in Maine's two congressional districts was higher than that of seven districts in New York—all of which were held by Democrats.

Even when one takes into account the swing ratio, the Democrats' control of the overwhelming majority of these cheap seats actually amplified their margin of control in the House, even in years when they did not do as well among voters. The question that this begs, of course, is what we should do about all this. Despite Campbell's suggestions, one is left believing that—short of dispensing with single-member districts—precious little can be done to alter the turnout patterns he observes.

Campbell shows that there is a high correlation between victorious Democrats and low-turnout congressional districts. Evidence has shown, however, that increasing turnout in those districts would have an effect on their electoral outcomes (e.g., Wolfinger and Rosenstone, *Who Votes?* 1980). As well, while evidence suggests that the presence of a challenger in such low-turnout (and frequently uncontested) districts might increase turnout and significantly alter the partisan balance of the district's electoral outcome, it does not indicate that this would make that outcome any more doubtful (Rush, *Does Redistricting Make a Difference?* 1993).

Campbell himself acknowledges that the Democrats' propensity to hold cheap seats is due in significant part to socioeconomic factors: poorer, less educated voters tend to vote democratic (p. 142). As well, he points out that gerrymandering plays little role in delivering cheap seats to the Democrats because such voters tend to be concentrated into discrete geographic areas. So, as long as we draw legislative districts on the basis of equality of population, some of those districts are inevitably going to have lower turnouts (and correspondingly stronger pro-Democratic biases) than others.

At first, Campbell's analysis is quite alarming—especially when he presents data such as that comparing Maine and New York or, for example, when he shows that there was almost an 8:1 difference between districts with the highest and lowest turnout in 1992. Clearly, candidates who need to win a district in which only 50,000 votes are cast have it much easier than those who must win in districts in which 300,000 residents vote.

Yet, while there is no disputing Campbell's evidence, his conclusions need further qualification. Noticeably absent from Campbell's analysis is any discussion of campaign spending. Perhaps the roughly 70,000 voters who turn out in a Queens congressional district are a lot harder to get out in the first place than the 250,000 who flock to the polls in a Maine district. In other words, while there may be fewer votes cast, competition for them may be no less fierce. If victory in Queens requires spending sums comparable to what is spent in Maine, then the thrust of Campbell's analysis could be reversed: The candidate in Queens gets much less "bang for the buck" than one in Maine. A quick look at Federal Election Commission data for the 1996 congressional election offers no conclusion about this, at least for the Maine and New York districts. Nonetheless, any conclusions about the "cheapness" of a congressional seat would seem to call for an analysis of spending patterns as well.

Differential rates of turnout are thus the core of Campbell's analysis. He shows that this has resulted in a skewing of the partisan flavor of the House because the Democrats' margin of seats in the House clearly was larger than their electoral margin would have suggested. As well, he points out that this same cheap seat phenomenon dampened the swing ratio's effect and actually diminished the Republican electoral fortunes in 1994. The same number of votes for the Democrats would have produced a significantly larger seat majority.

Campbell makes four suggestions to resolve the problems he sees in the electoral system. (1) We can change the way we allot population among districts, perhaps apportioning on the basis of voting age population or population eligible to vote. (2) We can draw fewer districts—by either converting to multimember districts or decreasing the size of the House—in order to minimize the interdistrict turnout disparity. (3) We can have a turnout-sensitive strategy of reapportionment. (4) We can seek to increase turnout.

Surprisingly, Campbell does not call for an outright conversion to proportional representation. This certainly would alleviate the effect of turnout discrepancies as well as the problems of bias and responsiveness that he notes in his swing-ratio analysis. Furthermore, it obviously would resolve many of the ongoing gerrymandering disputes that arise in a district-based system.

Clearly, reapportioning voters on the basis of voting age population may help alleviate some of the turnout disparities among districts. A turnout-sensitive reapportionment policy, however, would be difficult if not impossible to implement for obvious reasons, despite his evidence (p. 213) that district turnout is fairly constant from one year to the next. Add or subtract a challenger or retire an incumbent, and turnout may change significantly. Finally, *decreasing* the size of the House hardly seems practicable. While it might alleviate interdistrict turnout disparities, it would certainly make the process of drawing district lines even more contentious than it already is.

In sum, while one can take issue with his prescriptions, Campbell does provide a clear diagnosis that *something* is wrong with the electoral system. There is a price to be paid if we are to retain the single-member system of elections and insist that the right to vote requires equipopulous voting districts. Campbell has offered yet another reason to question whether the price is worth it.

Roosevelt's Warrior: Harold L. Ickes and the New Deal. By Jeanne Nienaber Clarke. Baltimore, MD: Johns Hopkins University Press. 1996. 414p. $67.00.

Matthew J. Dickinson, *Harvard University*

Jeanne Nienaber Clarke's biography of Harold L. Ickes focuses primarily on 1933–39, when he served not only as Franklin Delano Roosevelt's Secretary of the Interior but also as director of the Public Works Administration (PWA) and as oil production "czar" charged with ameliorating the

boom-and-bust tendencies of the industry. For the most part, the book's strength is not in any new historical material uncovered or any new interpretations offered; much of what Clarke recounts has been covered before in previous biographies of Ickes, by his posthumously published and sanitized "secret" diaries, and the voluminous New Deal historical corpus more generally. Instead, it lies in the literate way in which she assimilates and presents a large body of material about an important and exceedingly complex New Deal figure.

To the less historically minded among us, of course, Ickes is perhaps better known of late as the father of Harold M. Ickes, President Clinton's former White House deputy chief of staff and senior campaign strategist. It turns out that the characteristics Clarke attributes to the father—a devotion to liberal causes, a combative temperament, and extreme loyalty to the incumbent Democratic president—characterize the son as well. Yet, their career paths, as I discuss below, greatly diverge. Conceivably, Clarke could have used this father-son link to address some of the changes in presidential politics in the intervening decades. Instead, the book says almost nothing about the younger Ickes or of presidential politics today.

In Clarke's defense, the contribution of the senior Ickes to FDR's presidency and the New Deal more generally is a story worth telling in its own right. Ickes was a life-long Progressive who played an influential role in a variety of critical policy areas, including civil rights (he moved to desegregate the Department of the Interior even as FDR, fearful of losing the support of the southern wing of the Democratic party, largely avoided the race issue), natural resource conservation (Ickes vastly expanded the acreage of the national park system), and foreign policy (Ickes was an early and vocal advocate for U.S. intervention in Europe and the Far East, and he worked determinedly to block Japanese and German access to U.S. natural resources).

Moreover, "Honest" Harold had a well-deserved reputation for integrity, at least in his public affairs; he largely avoided even the whiff of fiscal scandal while sponsoring as PWA director millions of dollars in large-scale capital projects, ranging from dams and bridges to parks and public housing, and working in his capacity as oil administrator to adjudicate disputes in the oil industry.

His public reputation, however, belied a rather sordid personal life, as Clarke makes quite clear. For the most part, except for a brief lapse into psychoanalysis near the end of the book, her coverage of Ickes's private affairs is straightforward and largely nonjudgmental. This even-handedness is facilitated by her judicious use of his unpublished memoirs and unedited diaries, which allow Ickes to speak for himself. These personal glimpses provide a darker twist to the label "curmudgeon" that Ickes rather pridefully bestowed upon himself in *The Autobiography of a Curmudgeon* (1943).

His first marriage was a troubled one, punctuated near its end by affairs with other women. These affairs included, evidently, a brief liaison with his wife's daughter from a previous marriage and, later, a lengthy relationship with a married woman, begun during the delicate political period in which he was taking over the Department of the Interior in 1933. He was evidently more relieved than remorseful when his wife, who had earlier attempted suicide, died in a horrific automobile accident in 1935. Nor did he appear particularly guilt ridden over her inexplicable decision to leave everything to him (if it was *her* decision—Clarke's accounts hints at Ickes's possible duplicitousness here); he made no effort to share this inheritance with any of her children, and his stinginess may have contributed to the suicide of one of her sons. His second wife, Jane Dahlman, whom Ickes married in 1938, was almost 40 years his junior. By all accounts it was a happy union, but the age disparity provided more fodder for his critics, including Cissy Patterson, an influential member of a publishing family who publicly turned against Ickes in her editorials after he broke off their relationship to marry Dahlman.

Ickes had two children by his second marriage, including Harold M. Ickes, Clinton's White House aide. Despite their shared temperaments and political ideologies, the two Ickes's respective careers took largely separate paths, due primarily to changes in presidential politics during the intervening 60 years. As noted above, Clarke is not particularly concerned with chronicling those changes, but the contrast between the two careers, and the context in which they unfold, are in fact quite instructive. Three developments in particular stand out: the eclipse of the presidential cabinet by the White House staff as the primary source of presidential advice and administrative support; the dramatic drop in public support for a large, activist national government; and the gradual blurring of the distinction between politicians' private and public lives.

Consider the erosion of the demarcation between public and private activities during the last six decades. It is doubtful that Harold L. would have survived more than four years in office under Clinton, much less the fourteen he served under FDR. Harold M., of course, served less than a term in Clinton's White House, his fall from political grace precipitated in part by intense political scrutiny of some ethically questionable activities, most notably possible campaign-related fundraising illegalities. But, as Clarke makes clear, the personal activities of Harold L. occasionally spilled over into the public realm as well, as when he placed a woman with whom he was having an affair on the payroll at Interior. For the most part, his public reputation remained unscathed by his private controversies, but given today's in-depth media scrutiny of politicians' private lives, one wonders whether FDR would have tolerated those questionable personal activities to the degree that he did.

A second change in presidential politics is the eclipse in political influence of the president's cabinet by the White House staff. The elder Ickes wielded influence by virtue of his position as Secretary of the Interior. This guaranteed him the president's ear; no White House aide dared obstruct his access to FDR or attempted to make him a lackey of the White House staff. Today, despite the media stories at the start of every presidential term concerning the importance of presidential cabinet choices, real power resides in the West Wing, among the White House staff. Indeed, Harold M.'s influence came not from any executive function but from his position as a senior member of Clinton's personal staff.

This decline of cabinet prestige may have hurt presidents politically. Ickes, by virtue of his political prominence, served FDR ably as both stalking horse and political lightning rod; he could advocate policies, such as racial integration in public facilities, that FDR privately supported but could not, for political reasons, publicly embrace. But to perform these functions credibly, Ickes had to be viewed as somewhat independent of FDR, rather than as simply his mouthpiece. For this reason, FDR gave Ickes extensive freedom to run Interior as he saw fit. Most of FDR's successors, however, have failed to appreciate the benefits that can accrue from having secretaries with independent bases of power.

The third change apparent from Clarke's work is the erosion of political support for "big" government. Ickes believed that, in the hands of the Democratic Party, government could be an instrument to protect the public from the worst excesses of unbridled capitalism. Today, of course, Clinton has refashioned the Democratic Party in the Repub-

lican mold, in the process moving it away from the Progressive policies Ickes embraced. Such Republican me-tooism has helped Clinton win reelection and retain popularity, but it also contributed to Harold M.'s departure from the White House, a decision that one can imagine the senior Ickes would have supported.

Although Clarke largely eschews these types of comparisons, in her concluding chapter she tries to place Ickes in some historical context and come to grips with both his public and private persona. Her rendering suggests a much more complicated figure than the historical caricature of him as Progressive-cum-curmudgeon would suggest. Despite his questionable personal ethics, he was a man of unbending political principles who was not afraid to take on controversial issues. One cannot help but wonder if politics today precludes the selection of public officials who possess such strength of political convictions and the willingness to act on them.

Party Decline in America: Policy, Politics, and the Fiscal State. By John J. Coleman. Princeton, NJ: Princeton University Press, 1996. 253p. $39.95.

Political Parties and the Maintenance of Liberal Democracy. By Kelly D. Patterson. New York: Columbia University Press, 1996. 189p. $45.00 cloth, $16.50 paper.

William Crotty, *Northeastern University*

The Coleman and Patterson books are alike in that both approach an explanation of political party performance from distinctive policy perspectives, employing these to develop innovative and broadly inclusive explanations of party performance in the United States. Both also explore patterns of party behavior over time, and both focus primarily on the same historical period (post–World War II America). And both deal in broad if different terms with "party decline" theses, offering explanations for party continuity and change and assessing in varying forms the parties' contributions to representation in a democratic state. Both, in addition, are broadly ambitious works—in conception, execution, and the importance of the questions asked. Beyond these similarities, the two books diverge significantly in approach and argument.

The Coleman work, the more thematically comprehensive of the two, has theoretical assumptions and emphases less familiar to parties' scholars. Coleman approaches parties from a "state-centered," political economy perspective. It is one that has received more attention in the political development literature, and this study can be considered an example of the approach.

Party Decline is not an easy book to read or to synthesize, and it is not as thematically clear in presenting its major lines of argumentation as one might wish. The study incorporates an abundance of various types of data and secondary analyses, assessments of previous and related studies, and reinterpretations of other work and historical explanations in advancing its points. Basically, Coleman contends that the dominant policy concerns of a given period and the state-determined institutions for resolving these predict, and in fact predetermine, the relevance of the parties' role in governance. The parties' importance in representing voters, providing viable policy alternatives, and shaping contrasting legislative programs depends on the central issues confronting the state. Parties thus can be more or less relevant in different policy eras depending primarily on the economic demands the nation is experiencing.

Coleman develops his basic thesis in a multilayered series of explanations. The contention is that from the late nineteenth century to the inception of the New Deal, the state was in a process of expansion, a point not in debate. During these decades, Coleman argues, trade policy dominated political discourse, and the major parties offered distinctive visions and policy alternatives of substance. These positions held meaning for their clientele groups and for candidates for public office, and they had consequences for the development of state responsibilities.

This all changed during the 1930s with the self-conscious deemphasizing of the parties' effect on government and a distancing of the parties from relevant policymaking concerns. During this period and proceeding up to the 1990s, macroeconomic policy took precedence on the country's political agenda, a category of issues that the parties found difficult (if not impossible) to encapsulate in campaigns and over which mass electorates could exercise little control. Consequently, the political parties declined in significance during more recent decades.

The expansion and operations of the state from the 1940s on, the manner in which the political parties responded in both the Congress and in mobilizing electorates, and the congruence on fiscal policy in times of crisis and recession all combined to decrease the parties' centrality to decision making and the perception of their relevance by voters and political and economic elites. "In the end, the constraints of the fiscal state left the parties looking less relevant. With deference to the president, comparatively little to say on monetary policy, the acceptance of 'automatic' policy, and consensus over the response to economic problems, the parties failed to convince a growing portion of the public that they deserved attention or support" (pp. 138–9).

As for the present and the future, Coleman suggests political institutions can change and with them the parties' effectiveness. The "possibilities for party in American politics look better in the mid-1990s than they have since the establishment of the fiscal state" (p. 206). But, and this is the fundamental message, "no inherent logic of history requires party collapse and no such law demands renewal or ensures that parties will follow the pattern or path that is best for democracy or the parties' long-term relevance" (p. 207). Decisions now being made as to the direction state-economic relations will take in the future will provide the context for party operations and the breadth and quality of their contributions to democratic representation.

Again, *Party Decline* is an ambitious study and a bold argument. The use of data and the argumentation are rich and layered. State-based and macro-policy emphases are not new to explanations of party relevance—Walter Dean Burnham, Thomas Ferguson, and Martin Shefter, among others, have been developing variations of the approach for years. The limited concentration on one set of policy imperatives, and the state's response to these, and the promotion of the idea that the political parties are creatures dependent primarily on the dominant policy concerns of the state are difficult to accept as definitive explanations for their behavior. Parties can be captured by elites and economic interests (although this is not what Coleman is contending), and they do deal, with varying degrees of conviction, with the important issues of the day. These factors are valid, if partial, explanations for their actions. Political parties serve a variety of social and political needs within a society.

The approach obviously has significant utility for enriching our appreciation of party and state interaction, and the focus on policy issues is welcome. These dimensions of party operations are ignored or more often deemphasized in more conventional studies. Enlarging the context for exploring

party behavior is a contribution. Less convincing are single-factor explanations for broad patterns of behavior that respond to a variety of social impulses. These seem more delimiting and deterministic than is necessary. American politics is more open, pluralistic, and complex than single-theme, economically derived explanations would allow.

Political Parties takes a different tack. Patterson examines the linkages among the nomination, electoral, and governing coalitions as found in the commitments made at each stage of the policymaking process, seen here as a more bottom-up (as contrasted with Coleman's top-down) operation. In examining the period 1952–92, Patterson contrasts "the policy positions of presidential candidates . . . to the position outlined in party platforms and to those of party leaders in the Congress. Comparing party platforms to positions taken by candidates is one method of probing the connection between the nominating coalition and the electoral coalition . . . comparing the policy positions of the presidential candidates to the publicly stated positions of both parties' congressional leaders yields considerable evidence about the relationship between presidential candidates' electoral coalition and other vital components of the governing coalition" (pp. xxi–xxii).

The basic relevance of Patterson's study hinges on the acceptance of these linkages as critical to party performance and effect and the adequacy of the indicators of policy commitments—platforms, campaign rhetoric, and the positioning of legislative leaders—as the key associations and best representations of coalitional preferences. Patterson uses these measures, for example, to explore a concern with the quality of democratic representation, which is seen as the product of electoral competition, endorsed or redirected in accordance with election outcomes.

The tenor of Patterson's research is more explicitly empirical and the conceptualization and use of time frames significantly different than in Coleman's work. Policy formation is tied through successive party leaders and coalitional commitments more directly to the politically active concerns of the public. There is a conception of accountability in this and an assumption that the actions of government are largely determined by the results of elections and the priorities of the winning coalition.

Patterson is also more explicitly concerned with examining the arguments as to the decline of the parties and their implications. While accepting the belief that the contemporary parties are less effective and successful than they might have been in the past, it is Patterson's contention that the linkages examined have not eroded significantly and that as a consequence the parties remain, through these interrelationship, viable contributors to the process of democratic representation. The message is that the linkages among coalitions remain strong and cohesive at the same time that the broader parties and their operations have fallen into disrepute.

Consistent with the approach taken, Patterson reviews four decades of presidential campaigns and includes a range of proposals in the analysis: in effect, all issues of prominence at the various stages designated as significant to the campaign and to coalitional relationships. The study finds a stability over time in party positions and, for example, little difference in cohesiveness or divisiveness between primary and general election coalitions in the pre- and postreform eras. Patterson argues that the electoral landscape has been transformed since 1952 to the extent that "the conduct of campaigns today bears little or no resemblance" to those of earlier decades, "yet the relative stability of the levels of agreement between candidates and platforms attests to the strength of the links between the nominating coalitions and the party apparatus" (pp. 58–9).

The research also shows an increase in emphasis on redistributive policies, taken as an indication that the parties are not as weak or their ideological base as non-differentiating or bloodless as the party decline literature might lead one to expect. Employing the measures developed, a comparison of rates of agreement and disagreement over policy implementation among party elites finds "that possibilities for governance have not been seriously compromised" by changes that have taken place in party decision-making structures or, more broadly, the political environment (p. 107). The conclusion follows that "the ballyhooed crisis of government accompanying the decline of political parties in the United States seems not to have occurred" (p. 117); "the American version of democracy is humming right along" (p. 119).

There are a number of surprises along the way. In 1968, Hubert Humphrey, judged by the number of issues put forward (and not the adequacy, clarity, or effect of the proposals), placed a greater emphasis on "law and order" appeals than did his opponent, Richard Nixon, who built much of his campaign on this theme. A number of indicators showed less intraparty conflict among Democrats than Republicans, and the ultimate reformer and ultraliberal, George McGovern, had more in common with traditional Democratic Party values in 1972 than the rhetoric of the times would suggest.

In sum, then, the two studies in actuality are quite different. Both deal primarily with the same period, and both use a policy-driven perspective—approached in dissimilar and unrelated manners—as the point of departure for assessing the adequacy of the political parties as factors of consequence in democratic policymaking. Overall, though, the conceptual designs, the data sources and the way in which they are incorporated into the analysis, the assumptions underlying the research strategies, and the conclusions reached have little in common. Each study makes its individual statement; each represents a serious effort to address fundamental questions of representation; and each is comprehensive, thoughtful, and provocative enough to invite a revisiting of the events of the last half century and their meaning for understanding why political parties survive as well as the forces that define their contributions.

Bureaucracy and Self-Government: Reconsidering the Role of Public Administration in American Politics. By Brian J. Cook. Baltimore, MD: Johns Hopkins University Press, 1996. 201p. $45.00 cloth, $14.95 paper.

Richard T. Green, *University of Wyoming*

Brian Cook offers readers in public administration and political science an important new work on the problem of reconciling bureaucracy with democracy. In *Bureaucracy and Self-Government*, he addresses historically the American penchant for treating the public administration as a mere instrument of public will. Cook argues that our emphasis on absolute instrumentalism in public rhetoric has led to increasing disregard for the "constitutive" dimension of the public administration. Disregard for this dimension, he believes, has led to a strained politics of manipulation by the contending branches of government in order to make the public bureaucracy do their bidding. This has resulted in abuse to public agency capacities for serving vital democratic functions.

The constitutive dimensions of the public administration

involves its formative effect upon both the public policy. The myriad institutions of the public administration help define the ways in which we interact socially as well as politically and help us to deliberate upon and determine the proper ends and means of public policy. They create a multitude of ties between the people and the superintending branches, wherein they are ideally situated to exert educative, restraining, and stabilizing influence in both directions.

This latter role is rooted in the Greek conception of "practical reason." This is a form of wisdom that emphasizes relating the ideal to the real, and it can be gained only through the combination of education and tempering experience. Public agencies provide fertile ground for cultivating practical wisdom in service to democratic governance because they routinely bring policy to the street and represent constituencies to the superintending branches. They are suited to fostering ongoing discussion of ends and means among citizens and public officials and to articulating realistic capabilities for bringing appropriate ends and means to fruition. In short, the public administration helps constitute public spaces wherein citizenry may interact meaningfully with their government. Together they can give accountable form to public opinion.

Supporting this constitutive dimension is problematic, however, because our democratic spirit leads many to challenge the idea that anything other than the people and their directly elected representatives should define our way of life. Cook confronts this challenge by examining the prudent correctives underlying our constitutional system. The founders designed that system to bring restraint and stability to both the people and their government for the sake of protecting individual rights and preserving the republican form of government for posterity.

In the course of the founding debates on how to ensure these things, the role of the public administration received far more attention than most popular and scholarly works admit today. Cook's historical research reveals that the founders recognized the constitutive dimension as essential to the role public administration would play in a system of shared, checked, and balanced powers. They feared a purely instrumental view of subordinate officials would lead to tyranny by one branch or another. In essence, Cook concludes that all three branches were viewed as forming the superstructure of the public administration, and that the public administration should play a vital role in winning the allegiance of the people to the new republic.

Despite this important conclusion, however, Cook explains that, on the whole, the public rhetoric of the founders focused more upon the instrumental conception than the constitutive. In short, they played to the democratic spirit of the people with overly simplistic but compelling images of what popular government would be like, in order to secure ratification.

The instrumental image then manifested itself in some key decisions of the first Congress. The famous "decision of 1789" is particularly important. Congress determined that the president should have, by statute, the sole power to remove the secretary of foreign affairs (now our secretary of state). Although the congressional debate over this issue reflected sophisticated appreciation for the constitutive dimension of the public administration, the final decision clearly favored the instrumental view. Moreover, that decision, perhaps more than any other, institutionalized the instrumental view for posterity. From that point on, most of our politicians, judges, and administrators have viewed the president as holding sole power of removal over *all* principal officers of public administration. That has reinforced the instrumental image of the subordinate public administration and has set in motion a series of increasingly strained political battles among the branches of government for instrumental control over the bureaucracy.

Cook devotes much of his book (chaps. 2–5) to tightly written and fascinating analyses of these battles in the Founding, Jacksonian, Progressive Reform, New Deal, and post–New Deal eras. He shows how politicians adopted increasingly disingenuous and expedient political tactics that pinned blame for failures upon the other branches and ultimately upon the most convenient target—the subordinate bureaucracy. Thus, ironically, a strictly instrumentalist view of public administration leads inevitably to the public impression that government is an illegitimate bungler. These chapters stand on their own as valuable historical accounts for politicians and public administrators alike, for they explain much about how their relationships evolved to the current day. They should be required reading for all high public officials (elected and administrative) as well as for all students of government.

Cook's remaining chapters (6–7) briefly outline how we might recover a constitutive view of the public administration. He concedes that the task is daunting, and his suggestions may therefore seem somewhat benign and even "Pollyannaish" (p. 170). Nevertheless, he presents a thought-provoking analysis.

He argues that a vital first step "must be a broad-based, concerted effort to fashion a constitutional theory of public administration for the American regime" (p. 179). Toward this end he examines some important theoretical work by authors such as Stephen Elkins, Martha Derthick, Theodore Lowi, and John Rohr. Each provides important insights on the problems of discretionary administration in democratic societies. In the end, Cook concludes that Lowi's conception of juridical democracy (in *The End of Liberalism*, 1979) and Rohr's theory of public administration (in *To Run a Constitution*, 1986) provide a good starting point for broader public discussion of an appropriate constitutional theory. Lowi's rule formalism puts the principle of rule of law center stage, and Rohr's theory puts the discretion-conferring oath to uphold the Constitution squarely upon the public administration as a balance wheel of the system. Cook notes that each approach presents weaknesses, but in combination they provide the insights needed to help restore a healthy balance between the instrumental and constitutive dimensions of governance.

Finally, Cook hopes that opportunities for recovering constitutive administration exist in the current period of political flux and reassessment. He admits doubt about this, however, because most political and administrative reformers seem devoted to purely instrumental and cynical conceptions of the bureaucracy. Beyond this, he argues, we should engage in an educational program that brings awareness of the constitutive dimension to the public and their officials at all levels. Doing so, Cook believes, will lead eventually to a more appreciative and involved public.

How we do this remains problematic. Cook's suggestions here are weak, and he admits it. He hopes that additional works as influential as Lowi's *End of Liberalism* may emerge to boost political reflection. He also hopes for a rejuvenation of classical political science that emphasized the inculcation of practical wisdom. I agree that this would help, but I see no indication at all that the discipline of political science is headed in that direction. Public administration programs more often languish than prosper in the current disciplinary environment. A classical political science will probably have to emerge from some other source.

Though Cook ends his book on slim hopes, the work overall represents a valuable addition to the literature on bureaucratic discretion in our democratic system. It should certainly become mandatory reading in public administration programs and political science departments and, it is hoped, among public officials as well. I profited greatly from it and expect that all those who read it will share similar convictions.

Home Team: Professional Sports and the American Metropolis. By Michael N. Danielson. Princeton, NJ: Princeton University Press, 1997. 397p. $29.95.

Charles C. Euchner, *Boston Redevelopment Authority*

For almost two decades, cities in North America have confronted a problem that seems like a unique crisis but in fact is part of a pervasive trend in modern sports: A professional franchise threatens to move to another location if it does not receive hundreds of millions of dollars in benefits from public authorities. The challenge seems unique because of the emotional stake that local political elites, downtown businesses, boosters, and fans have in the team. What invariably ensues is a protracted negotiation in which the team holds most of the cards, and public authorities can only react and improvise. The result is an ongoing public subsidization of a booming industry.

Michael Danielson has produced the most thorough and evenhanded analysis yet of the politics of sports and cities. He focuses on the way that sports teams foster and exploit a sense of "place," particularly in cities where dynamic economic growth or decline undermines a more organic and abiding civic identity. Danielson carefully analyzes the operations of the major sports leagues in baseball, football, basketball, and hockey and the way they interact with state and local governments, on issues such as stadiums and expansion, and other industries with a major stake in sports (like television, movies and amusement parks, the fast-food industry, breweries, and newspapers). He pays special attention to the not-so-inevitable changes in sports over the last century and highlights decisions that have significantly shifted the course of sports-city relationships. Ultimately, Danielson concludes that the narrow business interests of team owners trump all other factors. He quotes the late Robert Irsay, who in the dark of night moved the Baltimore Colts to Indianapolis in 1984: "This is my team. I own it, and I'll do whatever I want with it" (p. 67).

At the center of the dynamic of sports-city politics is the monopolistic character of leagues. This monopolism operates at many levels. First, the major sports leagues deliberately restrict the supply of teams at the same time they encourage more and more cities to enter the bidding for teams. Second, once teams are in the leagues, they enjoy almost total control over their geographic territory. Third, the leagues dominate access to broadcasting revenues; if you cannot get on TV, then you cannot be viable in major league sports. Fourth, the leagues enter into labor agreements that are a kind of Faustian bargain: In exchange for control over labor against potential rival leagues, the leagues pay exorbitant player salaries. Fifth, team owners reinforce their control over sports (and manipulate their tax liabilities) by combining far-flung businesses from TV to restaurants and beer and liquor companies.

The leagues' extraordinary control over all aspects of sports politics is often obscured by the emotionalism and symbolism unleashed when a local authority proposes to build a new stadium and the team owner threatens to move to another city. The political is personal when it comes to battles over local sports teams; figures like Al Davis, Robert Irsay, George Steinbrenner, Ted Turner, Wayne Huizenga, and Jerry Reinsdorf take center stage, and the more important systemic forces get shunted to the side in local debates.

Even to people who have followed the business of sports for years, the financial dimension of sports politics is stunning. As Danielson notes, public authorities give franchises new stadiums at little or no rent, practice facilities, guaranteed revenues from ticket sales or broadcasting deals, free or subsidized land, a range of development opportunities, tax breaks, and a host of expensive infrastructure improvements, not to mention outright grants, worth tens of millions of dollars. As the gap between the supply and demand for teams grows, the cost rises dramatically. As recently as 1984, a brand new domed stadium in Minneapolis cost taxpayers $60 million—a lot of money, to be sure, but not a budget buster. As the century ends, the owner of one of the richest teams in sports, the New York Yankees, is demanding a facility that could cost as much as $1.2 billion.

As Danielson notes, the economic arguments for such largesse—that teams bring millions in new spending to the city, which ten multiplies in the local economy—are flawed. So why does this expensive game continue? The usual answer is that fans/voters would raise Cain if a mayor let a team move. But this argument is belied by the lack of electoral punishment to mayors who presided over team transfers and by numerous voter rejections of stadium deals. The real answer lies in the sports industry's monopolism, which gives teams decisive leverage in their dealings with urban officials. The one-shot patronage of new stadiums for well-connected urban elites (e.g., banks, unions, developers) and the incessant boosterism of newspapers and other sports supporters also helps the franchise's cause.

The usual public policy proposals to confront professional sports include government regulation, forcing leagues to expand, encouraging rival leagues, and fostering cooperative arrangements that bind teams to cities. But never do policymakers consider attacking the heart of the problem, namely, the leagues'; monopolistic control. Danielson does not explore a number of ways that restructuring of leagues could change the balance of power in sports politics. Such restructuring might include overturning league rules about public ownership, territorial exclusivity, player sales, minor league development, and the relationships between the big leagues and the minor leagues and collegiate sports associations. The boldest move would be to break up the big leagues, Ma Bell style.

Danielson's greatest virtue is also his biggest shortcoming. His study offers the most careful, detailed, thorough, and even-tempered study of the sports business in a burgeoning academic and journalistic literature. But he often fails to connect the dots and boldly highlight the underlying forces that drive the whole process. Still, no one can study the politics and economic of sports without turning first to Danielson. That is a considerable achievement.

Networks of Champions: Leadership, Access, and Advocacy in the U.S. House of Representatives. By Christine A. DeGregorio. Ann Arbor: University of Michigan Press, 1997. 185p. $39.50.

Robin Kolodny, *Temple University*

Christine deGregorio has assumed a major task in this work, which explores the relationship between Congress and the organized interests who wish to influence its work. To do this, DeGregorio undertakes a two-front approach: examining the

assumptions we have about both the nature of leadership in Congress and the extent of influence the interest community has with our nations's lawmakers.

DeGregorio's central argument is that we do not appreciate the complex relationships between leaders and groups. Furthermore, we credit formal party leaders in Congress with a monopoly on power in Congress and underestimate the important informational role advocacy groups can play in the public policy process. The author first wants to reexamine notions of what it means to be a leader in the House, including an examination of the role of congressional staff, and next to test whether groups that have influence over lawmakers have such influence because of their resource (money) advantages. To do this, DeGregorio has used a somewhat unconventional method: elite focused interviews of 97 members of the advocacy community. She has asked these advocates to identify House leaders on one of six public policy issues featured in the 100th Congress and then to assess their own satisfaction with the results they garnered. The 97 respondents represent a broad cross-section of the advocacy community in both sector (public or private) and type of group, as well as the resources each group possesses. The six bills studied also provide a good mix of policy arenas, complexity, scope, political conflict, and salience. DeGregorio's method is meticulously discussed in two appendixes.

On the topic of House leaders, DeGregorio finds that advocates cite committee and subcommittee leaders as being much more important than either formal party leaders or Rules Committee members, and she mentions (but never really elaborates) that some members without portfolio (obscure, that is) also make this list. She also finds that senior level staff members, especially those on committee staff, also are mentioned, though not as much as some cynical congressional observers might think. In this sense, DeGregorio has made one of the contributions she claims: a broadening of the definition of congressional leadership. While the concept of leadership itself is inherently problematic, due to the lack of a reliable followership among individually elected representatives, DeGregorio has shown that a number of individuals assume leadership roles in the formulation of public policy and that different individuals will emerge as leaders based on the topic under consideration and the stage of the legislation in the political process. By proving that formal party leaders are not the focal point of advocates' congressional partnerships, DeGregorio exposes the limits of traditional leadership studies.

The other side of the project is to examine the role of the advocates in the political process. Here DeGregorio asks several intriguing questions about organized interests resources, access, and success. In addition to the traditional interest group categories of professional/occupational groups, corporations, and public interest concerns, the executive branch is included in the mix. DeGregorio makes a convincing case for the inclusion of executive branch officials as organized interests rather than simple extensions of the president. More important, she demonstrates that information from advocates is a more valuable commodity to many officeholders than other resources, such as campaign contributions, in gaining access to lawmakers. Indeed, the relationship between lawmakers and advocates is portrayed as one of complex interdependence, with lawmakers courting advocacy group endorsements for legislative initiatives as much as the reverse occurs. DeGregorio's argument is entirely on target here, and her discussion of the advocates' awareness of their own role as political leverage between lawmakers is quite insightful.

DeGregorio's study is thoughtful and carefully executed. Still, there are several things I wish the author had done differently. First, I wish DeGregorio had given us more examples of the kinds of relationships she was describing. She spoke to 97 advocates about six different legislative arenas and only gives a few short pages of description on the particulars. Without a fuller discussion of these interactions, I am left wondering about the nature of her findings. For example, under what conditions do "obscure" members become leaders? What difference does it make that advocates make their initiatives primarily toward committee and subcommittee leaders (that is, do we know that any of their efforts remained part of the final bill)? How did such interactions enhance the fortunes of the advocacy groups and the leaders themselves? With the short text of 143 pages, I wish the author had given us another twenty of thirty pages with some insight into the hundreds of examples she must have at her disposal. Second, I wish she had spent more time examining the congressional leadership aspects of her study. The book predominantly covers the issues of access and advocacy over leadership. Part of this reflects the bias of the study: 97 advocates, but no members of Congress. Though DeGregorio begins her inquiry with the help of senior level staff, it would have been nice to have more staff or members interviewed as a control. As it stands, the data may suffer from underlying bias from the advocates, especially when it comes to describing their success at gaining access and their own level of satisfaction with their lobbying efforts. Though I think DeGregorio's analysis could be more complete, students of interest groups, congressional committees, and congressional leadership will find much food for thought.

Disunited States: What's at Stake as Washington Fades and the States Take the Lead. By John D. Donahue. New York: Basic Books, 1997. 256p. $25.00.

Richard Piper, *University of Tampa*

John Donahue's book is the latest contribution to what the author accurately describes as the "endless argument" over the proper balance between the national government and the states in the federal system of the Untied States. Like recent works on the subject by Paul E. Peterson (*The Price of Federalism*, 1995) and Alice M. Rivlin (*Reviving the American Dream*, 1992), this book addresses both political scientists and a broader audience of policymakers, students, and concerned citizens, and it does so in a fashion that is both readable and informative. Its greatest contrast with most other recent books on federalism, and even more so with much of the prevailing conventional wisdom among politicians in Washington and the state capitals, lies in its emphasis on the risks of decentralization. It is less novel in this respect than its author suggests, however.

Where Peterson and Rivlin both distinguish between "developmental" and "redistributive" public policies and favor devolution to the states of primary responsibilities in the former domain, Donahue casts doubt on the existence of a clear dichotomy between these two spheres of public policy. Although he reinforces much of what Peterson, Rivlin, and others have suggested about the perils of a "race to the bottom" among state-centered social welfare programs and about the obstacles to effective state redistribution of wealth in general, Donahue also warns of danger in state-driven developmental realms, such as business attraction, education, and job training. In this respect, his book resembles David Robertson and Dennis Judd's *The Development of American Public Policy* (1989), though, surprisingly, Donahue never cites it.

Donahue's major theme is that the increasingly global market economy and its potential threats to "America's future as a middle-class culture" should be focusing our efforts more on enhancing the effectiveness of the national government than on devolving powers to the state governments. In his view, the state governments not only are unable to promote egalitarian redistribution of wealth in an era of widening gaps between the rich and the poor but also engage in unwarranted competitive courtship of capital and demonstrate serious inadequacies in the skill-building areas of education and job training. Of these claims, those relating to state industrial policies are the best documented and most convincing. Donahue devotes two chapters and an appendix to analyses of the industrial development and business-attraction policies of the states. The appendix provides the interested reader with a useful survey of the scholarly literature on the subject, despite some omissions.

A strength of this book is that it seeks to place the U.S. debate over centralization into a contemporary global context and cities comparative examples more than most other recent works on the subject have done. Yet, the reader is often left yearning for more than the brief comparative evidence that Donahue introduces. For example, Donahue tells us, "even though the European Union falls well short of 'nationhood' by most measures, it maintains tougher limits on internal competition for investment than does the United States" (p. 115). There follows a brief paragraph that offers only fragmentary data, and then Donahue moves on to other matters.

Some additional comparative analysis in this relatively short book (only 169 pages, exclusive of back matter) would have enhanced its value for political science scholars particularly since its major theme is that the globalized economy has created conditions which make further decentralization to the states inadvisable. Even nonscholars would benefit from the addition of evidence concerning how the European Union and national and subnational governments in other parts of the world centralize or decentralize policy functions. Unfortunately, this book offers only enough such data to tantalize the reader. Nonetheless, Donahue is to be commended for at least introducing limited evidence drawn from Europe and elsewhere on the devolution question. This is more than most recent books on American federalism have offered.

One other area in which some additional substance would enhance this book lies in its historical treatment of the subject. Donahue provides a good but brief historical overview in the second chapter and supplements it a little elsewhere. Yet, only the reader who has encountered in other sources the New Deal liberal cases for centralization and against "states' rights" will recognize the extent to which many of Donahue's arguments are reminiscent of them. Although the global context and the administrative capacities of the states have changed dramatically since the 1930s, New Deal era arguments by Luther Gulick, Harold Laski, *New Republic* editors, and others against overreliance on the states are strikingly similar to some of those put forward by Donahue.

Donohue concludes his book with recommendations for a "new balance" between the national government and the states. Although he is careful to recognize complexities and nuances at many points in his book, his conclusions are so heavily weighted in favor of the national government and against the states, and are so polemical in tone, that they may alienate public policymakers who might otherwise find merit in some of his individual recommendations. Perhaps cognizant of this danger, given the present political mood in the United States, Donahue prefaces his suggestions by saying: "Do devolve—where it makes sense" (p. 165). Aside from a lengthy footnote endorsing state primacy in law enforcement, however, and his opposition to congressionally imposed unfunded mandates, Donahue offers little comfort to contemporary decentralizers.

This engaging work should stimulate thought and discussion on the subject of federalism. Its focus on the global economy and its efforts to introduce comparative data are commendable, even though further comparative analysis would shed additional light on the topic. One can hope that this book will be taken seriously by policymakers before they rush headlong into devolution of functions that may be performed more effectively by the national government than by the states. Donahue is at his most persuasive in linking state public policy deficiencies to potential dangers to the American middle class in an increasingly globalized market economy. Readers should give this book their careful attention.

Pursing Power: Latinos and the Political System. Edited by F. Chris Garcia. Notre Dame, IN: University of Notre Dame Press, 1997. 480p. $50.00 cloth, $25.00 paper.

José E. Cruz, *SUNY-Albany*

F. Chris Garcia's *Pursuing Power* is an excellent and useful contribution to the scholarship on Latino politics. It covers the most significant issues that Latinos face in their pursuit of power, and it offers insightful analyses of these issues, including electoral participation and representation; community organizing; the contribution of women; educational, language, and foreign policy issues; immigration; and affirmative action. The concluding section offers an array of provocative perspectives and raises a set of fundamental questions both for research and practice, focused on the future of Latino politics.

Although the book is theoretically suggestive, it does not break new theoretical ground. It does not offer a new model to explain the distinctiveness of the Latino case, as attempted by Rodney Hero in *Latinos and the U.S. Political System: Two-tiered Pluralism* (1992), nor does it spell out the contextual and strategic requirements for the maximization of Latino political participation in the manner of Fernando Guerra and Luis Ricardo Fraga ("Theory, Reality,and Perpetual Potential: Latinos in the 1992 California Elections," in *Ethnic Ironies*, ed. Rodolfo O. de la Garza and Louis DeSipio, 1996). Instead, the material is organized according to the Eastonian political systems framework, although Garcia is quick to point out that this is a heuristic rather than an indication of theoretical bias.

The lack of theoretical focus is understandable in an anthology such as this. Implicitly, however, the book rejects all existing paradigms as inadequate and all the proposed distributions of power that follow from existing paradigms as unacceptable.

Power cannot be foreclosed to Latinos—as structuralists would argue—given the phenomenal gains in political representation obtained during the 1980s and early 1990s. Between 1983 and 1993, for example, the number of Latino elected officials throughout the country increased an average 41%. Indictments of pluralism would include the fact that access was skewed, with most representatives elected at the local level, and was well below the proportion of Latinos in the total population, both nationally and by state. Dominant political coalition theorists should explain why, even though Latino representation in policymaking bodies favorably af-

fects Latino communities, the extent of substantive representation is minimal and uneven. At the national level, Latino constituencies do not seem to have much influence in how Latino representatives vote, and direct substantive representation is minimal.

Remarkably, even though this anthology includes the perspectives and analyses of 21 contributors, it has a coherent argument. All the authors seem to agree that Latinos are an increasingly significant segment of American society but have not been accommodated or appreciated as much as they have been recognized. Their continued socioeconomic and political disadvantages raise the question whether they should adopt "immigrant" or "racial minority" strategies for economic betterment and political incorporation. Furthermore, because the prospects for a more inclusive, egalitarian, and prosperous society depend in large part on how Latinos fare within that society, they ought to play an increasingly important part in the transformation of the social, economic, and political institutions of the United States.

Evidence of the first point can be easily noted in the attention to Latinos paid by such governmental agencies as the Bureau of the Census, such private organizations as the Ford Foundation (which funded the first national political survey of Latinos, conducted in 1989), and corporations that target their products to the burgeoning Latino market. This attention, however, contrasts with the recrudescence of anti-Latino feeling in recent times, best exemplified by the immigration and affirmative action debates, English-only initiatives, and welfare reform.

On the second point, the book strikes an ambivalent note: Yes, Latinos have suffered from discrimination and continue to experience a significant lag in socioeconomic status, but it is also true that advancements have been made in terms of political representation and on the road toward higher prestige and income occupations.

The response on the third point is even less clear. How can Latinos contribute to the transformation of American society and politics? Garcia's answer is somewhat contradictory. He writes that "Latinos' historical determination, diligence, and perseverance inevitably will result in a society in which equality and justice are much closer to being realities for all U.S. citizens" (p. 435), while he recognizes that "as the 1980s drew to a close, it was evident that, although some minor gains had been made, Latinos by and large were disappointed in the changes that had occurred over the past ten years" (p. 436). Thus, his optimistic certainty about the future of Latino politics does not seem warranted by the lessons of the past.

Aside from this discrepancy between retrospect and prospect, the volume raises other equally troubling questions. It is true that if Latinos emulate the example of blacks in claiming status as a racial minority group they will sacrifice long-term incorporation and progress for short-term advantages? How can Latinos speak and act as a homogeneous group given existing divisions along the lines of geographic settlement, national origin, ideology, partisanship, and citizenship status? Is it true that electoral and litigational strategies have reached a point of diminishing returns? If so, then what new conceptualizations, strategies, and approaches should Latinos politics adopt to confront the challenges posed by the current situation? How can Latino researchers reconcile the requirements of truth and policy advocacy? For example, the book establishes that the Voting Rights Act not only has been limited in increasing Latino participation but also may discourage participation by contributing to the formation of safe districts. This needs to be said without undermining the prospects for further governmental intervention.

These are some of the most important questions raised in this well-written, informative, penetrating, and provocative volume. The book does not balance the number of questions it formulates or suggests with the answers it provides, but this is hardly a shortcoming. Instead, it is an indication of the possibilities for future research on Latino politics. For example, the implicit rejection of existing paradigms is a clear cue, and some of the concerns raised regarding the role of ethnic identity, electoral systems, and the policy priorities of the state in fostering or hampering Latino political incorporation are equally suggestive.

If, as one of the contributors indicates, the focus on policy parity of interest-group politics is flawed, then it makes sense to argue that the fundamental challenge to Latino politics in the next century will be to develop "a much broader understanding of community that explicitly includes the public interest" (p. 443). This means that, to succeed, Latino politics must overcome—both in theory and practice—the criticism that it is simply a zero-sum game. *Pursuing Power* is infused with that spirit of community—Latino progress must also be progress for all—but it does not tell us how to make that spirit a reality. The book on Latino politics which does that will be much more than a mere contribution to scholarship.

Religion and the Culture Wars: Dispatches from the Front. By John C. Green, James L. Guth, Corwin E. Smidt, and Lyman A. Kellstedt. Lanham, MD: Rowman & Littlefield, 1996. 369p. $39.95.

Christopher P. Gilbert, *Gustavus Adolphus College*

This volume compiles nearly fifteen years of research by four of the most respected scholars in the field of religion and politics. While the title may suggest a narrow focus on the Christian Right and disputes over contemporary social issues such as abortion, the work ranges over a broader set of topics to develop a theoretical framework from which to understand the political relevance of religion in U.S. politics. The authors use existing data, such as the National Election Studies, as well as their own surveys of voters, activists, and clergy. The material is organized chronologically, and several chapters are reprints of previously published articles; the core chapters described below are new, however, and offer critical insights into fundamental questions of measurement, concept formation, and the salience of religious factors.

The chronological organization of the volume has one clear advantage: It shows the development of theory regarding linkages between religiously motivated social movements and political institutions, such as parties and interest groups. It is clear scholars have come a long way in understanding the Christian Right and its effect. One disadvantage is that the key linking mechanisms, measured through religious commitment and tradition, do not come until chapter 10, which stands out as the most important chapter. The authors argue persuasively that "grasping the essentials of religion is a potent first step in understanding its connection to politics" (p. 175). Using a large 1992 sample and their own battery of questions, they develop measures of religious tradition and religious commitment and test their salience for predicting political attitudes and behaviors. They choose to operationalize religious commitment by carefully sifting out its absence; that is, they identify those individuals who show little to no religious affiliation, belief, salience, ritual observance, and private devotionalism. Such people comprise one-quarter of the American population and show behavior patterns distinct from those of believers. While the religious commitment scale is less successful in revealing differences among high-commitment citizens, additional variables provide im-

proved measures of religious tradition and other components of spiritual life in order to tell the complete story.

Additional chapters demonstrate the importance of studying political activists, particularly with regard to the Christian Right. The authors show that diversity exists within Christian Right organizations; combined with existing tensions within American evangelicalism, and the autonomy of local congregations, their work balances the often facile analysis that projects the Christian Right as a monolithic movement with a simplistic agenda.

Some of these findings are not new to the field; indeed, researchers without background in this subject area would do well to trace the growth of theory and the refinement of concepts that occur through the book. Put another way, this book drives home the idea that there should be no subfield of religion and political behavior that is distinct and separate from the field of mass political behavior generally. Too many researchers have virtually ignored the work of these authors and their counterparts, choosing instead to overlook the relevance of faith; to capture it with poor or too-simple measures such as church attendance or the dated Protestant/Catholic/Jewish/other/none scheme; or to categorize the political relevance of religion solely in terms of the Christian Right movement. The National Election Studies now contain sophisticated questions that explore all the importance facets of religion described in the volume; these measures exist largely because Green and others, David Leege, Kenneth Wald, and other collaborators have continued to demonstrate how such measures matter and how they improve upon earlier formulations. We now understand that church attendance alone contributes little to the political relevance of an individual's religion. Rather, church attendance proxies for personal faith, devotion to liturgy, and the salience of prayer and Scripture reading; most important, the influence of each of these factors differs in terms of depth and the attitude or behavior in question, so much so that attendance alone cannot unpack the full flavor of the connections between an individual's faith and church life and his or her political views.

Despite these key contributions, the authors themselves also fail to note the full implications of their theoretical framework. Specifically, the fact that religion is both a personal and a social activity in the United States necessitates an understanding of the church, in its various forms across denominations and faith traditions, as a politically relevant unit. The authors hold a crucial piece of the puzzle with their firm grasp on theological and historical differences among faith traditions and their knowledge of the political roles played by clergy in several different denominations. Yet, they perform little analysis that measures important aspects of congregations themselves. Pathbreaking research by Wald, Owen, and Hill ("Churches as Political Communities," *APSR* 82(June 1988):531–48) demonstrates the salience of congregations as cue givers and sources of influence. Contextual analysis, perhaps not coincidentally another line of behavior research that has received far too little attention until recently, has clear relevance to the work of Green and others (e.g., see chapter 13 in Robert Huckfeldt and John Sprague, *Citizens, Politics, and Social Communication*, 1995). Their theoretical understanding of churches as political communities—and therefore as relevant contextual units—is perhaps more limited than it could be in this regard, particularly with respect to claims about "the social embodiment of religion and political behavior" (p. 174). If affiliation and involvement are the critical factors that determine the extent to which religion matters in political terms for a citizen, then it stands to reason that the issues of "affiliation with and involvement in what" necessitate operationalization of some aspects of the religious environment. Wald, Owen, and Hill show distinctive "church effects" even when the church's theological orientation fits poorly with its members' beliefs; Huckfeldt and Sprague similarly demonstrate church effects without resort to arguments about theology.

The authors make a lasting contribution in showing indisputably that religion must be considered as a factor *central* to many citizens' political attitudes and actions, peripheral to other citizens, and irrelevant to a large segment of the population. Their work deserves not to be typecast as merely religion and politics; rather, it provides an essential guide to the full comprehension of the multifaceted nature of Americans' political beliefs.

The Road to Nowhere: The Genesis of President Clinton's Plan for Health Security. By Jacob S. Hacker. Princeton, NJ: Princeton University Press, 1997. 248p. $39.95.

Mary E. Guy, *Florida State University*

> Yet the methodology of policy analysis has come to embody an emaciated conception of politics and policymaking. It is fixated on policy solutions, with solutions understood as rational choices to achieve given objectives within fixed constraints. This exclusive preoccupation with solutions prevents a full realization of the political, institutional, and cultural context in which policy ideas must be justified, debated, enacted, and implemented (p. 179).

There you have it. Jacob Hacker's requiem for health reform details the rise and fall of the Enthoven plan, the brainchild of policy experts who were political neophytes. Hacker's assessment is measured and balanced. The 182 pages of narrative, embellished with appendices and footnotes, describe how candidate Clinton became enamored of the Jackson Hole gang and their idea for managed competition.

This is one of the best histories you will find of how the Enthoven plan was born and how it won the day when candidate Clinton decided to advance health reform as a campaign issue in 1992. It positions this attempt at health reform in light of earlier (failed) presidential efforts to bring access to health care in this nation up to a par with that in other Western nations.

The lesson to be learned from this book is that policy does not belong only to the experts. It belongs to all stakeholders, of which there are many when it comes to health issues. And the more complicated a policy reform, the less likely it is to be favored by the public or Congress. This book will be read by two audiences: those who are interested in health policy per se and those who are interested in the policymaking process. It is equally instructive for both.

Health reform is a complicated story that is not finished. According to the U.S. Bureau of the Census, about 36 million Americans were uninsured when candidate Clinton noticed that health care could be the road which would take him to the White House. The numbers have steadily increased, and in 1996 there were almost 42 million uninsured. Thus, the problem continues to escalate. Systemic change failed in 1994. In 1997 the Clinton administration, having learned from the defeat, is attacking the problem by focusing on pieces of the puzzle rather than on the puzzle as a whole. Fighting little battles rather than big ones, health reform now takes the form of step-by-step changes. Thus, as Charles Lindblom explained some time ago, the nation muddles through. Reliance on policy experts who are politically naive has given way to reliance on political experts who know how to sequester policy issues into digestible bites.

The experience of the Clinton health reform initiative was foretold by the Medicare Catastrophic Coverage Act of 1988.

The first major enhancement in Medicare benefits since the program's inception in 1965, the bill was rescinded shortly after its passage. A survey of Medicare recipients conducted in spring 1989, about halfway between the time the legislation had been signed and when it would eventually be repealed, showed that respondents' knowledge about the specifics of the law was vague and often erroneous (Rice et al., "The Medicare Catastrophic Coverage Act: A Post-Mortem," *Health Affairs*, Fall 1990, pp. 75–87). Out of eight questions asked, the typical respondent answered only two correctly. For example, more than 91% erroneously believed that everyone who had Medicare would be required to contribute toward the cost of the new Medicare benefits through an increase in their federal tax payments. Fewer than 40% correctly understood that when the new legislation was fully phased in, Medicare would cover some of the costs associated with prescription drugs. The seeds of the legislation's destruction are both obvious and simple: The elderly are very worried about having to incur out-of-pocket expenses. What the elderly wanted was complete insurance coverage with no deductibles or coinsurance. What the act provided was far less. The lessons provided by the rise and fall of Catastrophic Coverage are clear. First, citizens are likely to misunderstand the purpose as well as the provisions of a proposal in spite of factual information that is provided to them. The second lesson is closely related to the first: Successful legislation will be that which simply and clearly spells out its benefits. Third, success comes when people get what they want most: quality health care with only minimal, if any, out-of-pocket costs.

As the debacle with the catastrophic care legislation shows, Americans are afraid of rising health care costs but are also afraid of health legislation. If Hacker fails in this book, it is because he places too much emphasis on individual actors and ignores the basic truths of health policy: Grand schemes fail, and small steps succeed. Small steps are more discrete, target a specific constituency, and lend themselves to explanation. Big steps are broad, target multiple constituencies, and are difficult to explain to audiences that include both consumers and providers. As Hacker explains, the Clinton plan was developed with impressive intellectual energy coupled with political naiveté. Its advocates were unable to persuade citizens that integrated, managed competition was preferable to the crazy-quilt system to which they had become accustomed.

On the cutting room floor is left the story of how the medical establishment took umbrage with the role of Hillary Rodham Clinton and Ira Magaziner at the helm of the reform initiative because they did not have medical credentials; the fact that the American College of Surgeons filed suit against the First Lady, claiming she had no right to convene the health reform task force; the fact that physicians became more nervous about reform as nurses were brought into the loop in greater numbers than had happened in past health reform initiatives; the fact that physicians lobbied their patients during routine office visits, dissuading them from supporting the reform movement. To make a long story short, the hegemony that physicians' groups were accustomed to exerting in health policy debates was sorely challenged by the Clinton administration's health reform plan.

In order to tell a story in the pages allotted, the author had to cut somewhere, and his focus on the confluence of public opinion polls, health economists, and campaign strategy provides a useful perspective for enlightening a doomed policy initiative. In his effort to present clearly the plan and the policy process through which it moved until it went moribund, Hacker deleted a hefty portion of the action that surrounded the policy debate. The fact that Hillary Clinton became a lightning rod for the proposal's detractors deserves more attention, as do the many little mansions of the medical establishment: insurers, practice groups, hospitals, pharmacies, and each health profession's lobbying effort. Nevertheless, for the part of the story that he tells, Hacker tells it well—better than you will find elsewhere.

This book cuts away enough of the detritus that the reader can learn about the actions of key players and the events that propelled them forward. It is recommended reading for those interested in health policy and for those who have yet to be convinced that there is more to policy analysis than sophisticated equations and the public choice paradigm.

Congress as Public Enemy: Public Attitudes toward Political Institutions. By John R. Hibbing and Elizabeth Theiss-Morse. New York: Cambridge University Press, 1995. $54.95 cloth, $16.95 paper.

Stephen C. Craig, *University of Florida*

Following the Republican sweep in the 1994 midterm, as Bill Clinton was awkwardly proclaiming his continued "relevance" to the process of national policymaking, few would have predicted not only that the president would be reelected two years later but also that he would win handily in a one-sided contest whose outcome never seemed in doubt. There were many factors that contributed to Clinton's success, but one of the most important of these stemmed from the miscalculations made by GOP congressional leaders during their battle with the White House over the 1996 federal budget. House Speaker Newt Gingrich and his allies apparently felt that by drawing a line in the sand and refusing to pass stopgap spending legislation, they could either force the president to make policy concessions or else allow a government shutdown that would cripple his administration once and for all. It did not work. When Clinton's refusal to bend led to a suspension of all but essential government services on two separate occasions during late fall and winter 1996–97, the American public did indeed react with alarm. Most did not, however, blame the president for what had happened. Most blamed Congress and its Republican leadership in particular.

According to John Hibbing and Elizabeth Theiss-Morse, this should not have come as a surprise. In their provocatively titled *Congress as Public Enemy*, the authors suggest that the legislative branch is viewed by citizens as the central institution of contemporary democratic politics (especially in nonparliamentary systems). As a result, it is dissatisfaction with Congress which supposedly drives the overall lack of trust in government that is so widespread among the American public in the 1990s. And what do people so dislike about Congress? Not its partisan or ideological makeup and (for the most part) not its policy outputs. For Hibbing and Theiss-Morse, the problem lies first and foremost with the democratic process itself. Democracy is ugly, and Congress is the institution which reflects that ugliness more than any other. Thus, simply by "being itself," Congress—along with other powerful and independent legislative bodies at the state level and elsewhere around the world—actually contributes to the lack of popular support for the political system as a whole.

Hibbing and Theiss-Morse begin this interesting and well-written volume with a plea for better theory accompanied by better data. Their theoretical argument is twofold. First, taking exception to the Eastonian distinction between diffuse and specific support, they contend that researchers need to specify clearly the possible *objects* of system support and that

among the most important of these objects are the formal institutions of government, including Congress. Second, they hypothesize that the granting or withholding of support will often be based on citizens' attitudes toward the *processes* employed by institutions and decision makers as they go about doing the people's business. In democracies, Hibbing and Theiss-Morse believe that such procedural evaluations tend to be centered on perceptions of efficiency (whether decisions are made "in an expeditious and direct manner") and equity (whether decisions are made "with due concern for all who should have influence" and benefits are allocated "in an equitable and just manner"; see p. 14).

Eschewing standard survey measures such as the ANES trust in government index, the authors opt for a more innovative multimethod approach that uses both qualitative (focus groups) and quantitative (survey research) data. Most of their analysis is based on the latter, from which they learn the following. (1) Approval ratings are substantially higher for Congress as an institution ("no matter who's in office") than for the overall membership, congressional leaders, or even one's own representative; a similar gap between institutional and incumbent evaluations is evident for the presidency and the Supreme Court, but it is much smaller than for Congress. (2) The American public regards Congress as the most powerful institution in our national government but does not believe it uses that power in a positive way to deal with the country's most important problems; to the contrary, there is a sense that Congress is an impediment to presidents who might otherwise be able to accomplish something constructive. (3) There is considerable support for term limits (80% of the national sample), and when respondents are presented with arguments contrary to their own position on the issue, those initially opposing term limits are more likely than supporters to change sides. (4) Approval of the membership of Congress (as opposed to the institution "no matter who's in office") tends to be at least somewhat higher among citizens who think their own representative is doing a good job, majority party identifiers (Democrats at the time), the externally efficacious, the less well-to-do, and, reflecting the importance of procedural concerns, among individuals who disapprove of legislative professionalization (measured by a hodgepodge index that also includes beliefs about congressional "efficiency") and who feel that members attend more to the concerns of special interests than they do to the needs and wishes of ordinary people.

Although scholars have typically stressed the ideological and policy roots of popular discontent, this last point may seem almost axiomatic; there is, after all, a growing recognition that procedural values (including beliefs about who is, and who is not, being represented) matter as well. Of greater interest is the conclusion by Hibbing and Theiss-Morse, based on their focus group data, that many Americans are uncomfortable with some of the qualities that are more or less inherent in the nature of democratic discourse—and in the operation of strong democratic legislatures—that is, "uncertainty, conflicting options, long debate, competing interests, confusion, bargaining, and compromised, imperfect solutions." In short, "people profess a devotion to democracy in the abstract but have little or no appreciation for what a practicing democracy invariably brings with it" (p. 147). There is evidence that citizens are themselves willing to shoulder a portion of the blame for failing to take the initiative and to hold elected leaders accountable for their behavior. Ultimately, though, the real culprits are the politicians and the "Washington system" (of perks, money, and privilege) that corrupts and "seduces members away from fulfilling their responsibility to the public" (p. 62).

Congress as Public Enemy is not, of course, the final word on the subject. Additional research clearly is needed to flesh out our understanding of how citizens perceive and relate to other institutions and other aspects of the political process (such as elections). Furthermore, Hibbing and Theiss-Morse probably are less attentive than they should be to the potential role of policy outcomes (or, attitudinally, what they call "valence dissatisfaction"; see p. 55) in shaping people's feelings about government. Perhaps most crucially, the authors address only briefly the question of why support for Congress, and for government generally, has dropped sharply in recent years. In fact, they raise the theoretical ante by suggesting that current low levels of congressional support are, when viewed historically, more the rule than the exception, that is, "the drop in confidence from 1967 to 1971 [followed by relatively modest changes since then] might look large not because confidence in 1971 was abnormally low but because confidence in 1966–67 was abnormally high" (p. 35). If this is true for Congress, could it also be true for other dimensions of public support? And if it is, does that mean we have been spinning our collective wheels in a misguided effort to account for the emergence of the "angry voter" in American politics? While the authors make no such claim, it seems to follow implicitly from the logic of their argument.

There is, then, considerable work yet to be done as we continue to seek a better understanding of the relationship between rulers and ruled in the United States. In the meantime, *Congress as Public Enemy* is an important and timely report that should be of interest to anyone who studies public opinion, attitude measurement (especially in terms of the complementary use of survey research and focus groups), or American politics generally.

Gender, Families, and State: Child Support Policy in the United States. By Jyl J. Josephson. Lanham, MD: Rowman & Littlefield, 1997. 210p. $62.50 cloth, $23.95 paper.

Deborah McFarlane, *University of New Mexico*

This book is about child support policy in the United States. Since 1974, the United States has developed and implemented a comprehensive federal child support policy. "What kind of solution is this?" asks Josephson. What are the problems that this system of child support enforcement addresses? "Answering these questions is at the heart of this book" (p. 2). What is dazzling is that the book answers these questions at so many levels: theoretical, empirical, and normative.

The author points out that public policies take place "in a particular historical, political, and institutional context" (p. 2), and they are shaped by ideas. Child support policy lies at the nexus of social welfare policy and family law, both of which are terribly complex. Fortunately for the reader, Josephson is up to the task. She not only provides a coherent explanation of the institutional and historical context of child support policy but also brilliantly grounds that explanation in theory.

The strongest theoretical traditions in the American polity are liberalism and republicanism; for this reason, Josephson refers to the United States as a "liberal republic." She explains how each conceives of the family-state relationship and provides a feminist critique of each conception. For example, neither tradition has historically found a place for women in the polity.

Both liberalism and republicanism have influenced family law and social welfare policy. The notions that clear reasons must be given for state action and that government should be

limited in family life come from the liberal tradition. The republican tradition stresses the cultivation of civic virtue in children, which presumably means that the state has an interest in caring for and educating all children so that they can be good citizens. Josephson provides another lens, the economic traditions and practices of the United States. Liberal republics with market economies have an interest in limiting their financial liabilities (p. 20). Current debates over changes in families and pertinent social policies, such as child support, reveal some of these philosophical incongruities. Given the conflicting state interests with respect to families, it is not surprising that public policies toward families, including child support, have disparate goals.

The second chapter examines the history of child support policy and reviews recent scholarship addressing its implementation. Until 1974, the amount of support required of a noncustodial parent was largely left up to judicial discretion. Unlike other aspects of family law, however, child support has become increasingly regulated and federalized. Because child support policy was created as part of the AFDC program, both its purposes and institutional features are "more in keeping with the U.S. social welfare system than with the tradition of family law" (p. 32). Non-AFDC custodial parents (usually women) may use the child support enforcement bureaucracy, and AFDC mothers *must* use it, although they may keep only $50 in child support above the welfare allotment. Here the goal of state fiscal limitation supercedes child welfare.

Recent scholarship has examined the cost effectiveness of child support policy, enforcement, and child well-being. The federal government no longer saves money through this program because it bears most of the costs, and the states receive the income. One comprehensive analysis of the effect of this policy concludes by arguing for a system of child support insurance (p. 51). What had not been studied is how child support policy affects the lives of individuals and families.

The fourth chapter describes how the system operates at the state level by investigating the implementation of child support law in Maryland and Texas and analyzing the effects of that law on individuals and families. The work is thorough: Josephson examined court documents, surveyed administrators and clients, and even conducted interviews with clients, both male and female. This section could have been improved, however, by the use of an implementation framework (e.g., Mazmanian and Sabatier); such a model could have sharpened the state comparison and provided more insight on the most important factors in implementation.

The findings are original and poignant. Now we can really feel the effect of contradictory policy goals both on administrators and clients. Here is a system that shames and disciplines some women—especially the poor—while tangibly assisting others. The same system compels some men (and sometimes women, when they are noncustodial parents) to provide support for their children when they might not otherwise do so, while enforcing unrealistic expectations on others, particularly very poor men.

But Josephson is not sentimental. The fifth chapter returns to normative questions with special attention to gender. Can we devise policies that can support families and also promote gender equality? To do that, she argues, we have to reformulate some of our most basic ideas about the state and about the public-domestic relationship. The final chapter summarizes the main conclusions of the study and discusses how the child support system might be reformed.

In sum, *Gender, Families, and State* is first-rate scholarship. Using a variety of methods, Josephson has thoroughly analyzed child support policy. The empirical work alone makes a major contribution to the scholarship in this substantive area. But that is only a small piece of what this book offers. In an era of welfare reform, Josephson has pointed us to the right questions. What kind of solution does a particular policy offer? What are the problems that it purportedly addresses? What about the welfare of the children? Do families have to be self-supporting economic units? How can public policy support families and promote gender equality? Are state fiscal interests superceding the well-being of children, particularly low-income children?

Kinship and Politics. By Donn M. Kurtz II. Baton Rouge: Louisiana State University Press, 1997. 249p. $40.00.

S. Sidney Ulmer, *University of Kentucky*

This book is properly placed in the context of social background studies, though, as might be expected, it has its own particular twist. The belief that drives such work is that we are all the product of earlier life experiences, and these earlier experiences hold significant implications for future behavior. In the judicial context, the primary focus of those working in the area has been either to gather and present background information on some set of judges or to use such information to explain, in part, the decisions that judges make. The work of Cortez Ewing and John Schmidhauser exemplifies the first, that of Stuart Nagel and Neal Tate the second.

In *Kinship and Politics*, Kurtz follows the path laid out in 1959 by Schmidhauser, with one important exception. Whereas Schmidhauser identified a large number of potential variables, Kurtz focuses exclusively on the "Political Family" and its various extensions. This political family exists when one or more relatives have held public office, that is, all elected officials as well as all major political appointees at every level of the system. This is a unique definition and, perhaps, a corruption of the word *family*. But whether it will prove to be a useful research concept remains to be seen.

Essentially, the author limits himself to gathering information on the political family backgrounds of 98 judges of the Louisiana Supreme Court (1812–1988) and 104 Supreme Court justices (1789–1988). By explicitly denying any attempt to link background and decisions, Kurtz denies one the opportunity to offer the stock criticisms sometimes associated with such omissions. His more limited aim is to "add to our store of knowledge of the kinship factor in politics by focusing on the public service careers of the [judges'] families . . ." (p. 2). And add he does, with twenty figures or tables in one hundred pages of text, five pages of references, a forty-eight page bibliography, an eighteen-page index, and appendices running seventy-two pages.

He finds, inter alia, that a majority of the judges studied were from a political family, that the political activity of these families was spread over three or more generations, that the marriages of judges and their children extended family political influence, and that kinship connections frequently crossed state boundaries. Thus, he writes that judges' political families continue to have an influence on elite composition.

Kurtz suggests that interest in social background studies and political elites has declined somewhat in recent years. He seems to attribute the decline to a cultural bias against focusing on the unequal distribution of power among office seekers at the political starting gate, an inequality he imputes to differences in family background. While such a bias may

exist, there may be different, and possibly better, reasons for the disinterest he detects.

One reason is that while public officeholders may constitute, in one sense, a kind of political elite, they are not and cannot be viewed as traditional political elites, with all the pejorative implications attributed to such groups. In more classical terms, political elites have possessed greater political power than nonelites. They have been unified, self-conscious, and usually autonomous; self-perpetuating and answering only or primarily to the interests of the group from which they spring. In an open democratic political system, such descriptions are clearly not that germane.

Public officeholders in the United States may very well be able to help us or hurt us, on occasion, as much as the political elites defined above. But the arbitrary and relatively cost-free exercise of power associated with historical elites is not on the top-ten list of things to worry about in our political system. Since we are more likely to study what we fear than what fears us, scholars may choose to limit their concerns about political elites, especially if by that we mean all those who hold public office. Putting politicians in office may not be risk free, but neither are we in the age of the Athenian tyrants. Indeed, the author does nothing to establish that we would be better off without political families, as he defines that term.

A second reason is that most scholars who have authored social background studies of judges (including Schmidhauser) have conceded that background would be most significant if it could be shown to affect public policy. Earlier attempts to establish such a linkage reflect that view. A major problem with these studies, however, is that by and large the nexus has not been parsed for time. Correlations that hold for some periods but not for others suggest that something other than cause-and-effect relationships are involved. At best the "causative" factors are conditioned; at worst, the correlations are spurious. It is not too difficult to suggest factors that might mitigate the effect of social background on the decisions of judges; to mention a few: the law of "anticipated consequences," stare decisis and other legal traditions, the effect of small group settings, and the attitudinal structure of the judges on a particular court.

So, while some may wish to suggest a direct relationship between background and the decisions of judges, bluntly put, that twain has yet to meet. The failure to establish social background as a predictor of judicial decisions may account, in part, for the slackening of interest to which the author refers.

In sum, though, *Kinship and Politics* makes a notable contribution to the literature dealing with the background of judges. Apart from useful information on sources of recruitment, the book presents data, insights, and argument—any or all of which may trigger new studies, new designs for research, or even larger theories which incorporate social background but do not depend on it exclusively to carry the load.

Crisis and Political Beliefs: The Case of the Colt Firearms Strike. By Marc Lendler. New Haven, CT: Yale University Press, 1997. 206p. $30.00.

Laura R. Woliver, *University of South Carolina*

Fortunately for the scholarly community, Marc Lendler was around the Hartford, Connecticut, area during a lengthy labor strike at the Colt Firearms plant, and he had the insights, skills, and wisdom to follow the strike, attend union meetings, and conduct sensitive, enlightening interviews with strikers, their families, and plant officials. *Crisis and Political Beliefs* displays that these workers, under the dire circumstances of a more than four-year strike, gained a sense of self-respect, community, and solidarity along with keen political insights.

When studies of labor unions and other group efforts focus on monetary and material gains alone for the participants, they may mismeasure the effects of the effort and the consequences for our politics. Many workers in the Colt strike felt and acted upon a sense of solidarity and community that went beyond the merely individualized, rationally acting, autonomous calculator (the "rational striker") assumed as the only motivation by some scholars. Lendler found "the conflict led the strikers to develop norms in which immediate individual self-interest played a lesser role than cooperation and solidarity, and in which dignity and self-respect became ends and not means to other ends" (p. 35).

Communal values were given more than lip service, Lendler shows. It is easy to claim to an interviewer that fellow strikers were like "family." The actions of the strikers displayed how they lived out their solidarity through practical cooperative activities, such as repairing one another's cars and houses, to name a few. "The argument is not that the strikers were gushing sentimentalists," he points out, "but that a new form of rationality emerged, based less on finely tuned calculations of individual self-interest than on collective definitions of interest, a shared purpose, willingness to sacrifice for it, and a sense of responsibility to each other" (p. 57). Strikers near retirement when the walk-out call came lived the family vision clearly, participating in a prolonged strike long past time when it made mere financial sense. These workers explained to Lendler that the strike was for the sake of a future labor movement, so future workers might be treated like humans, not just tools. Their commitment to a larger labor cause meant that sometimes they were even "prepared to call in an airstrike on their own position" (p. 58). Thus, solidarity was a norm, not just a tool.

Lendler's study is also a clear and logical model of how to conduct a case study which captures the insights and context of the events and participants over time, and extends and expands important theories. One strength of this study is the author's knowledge of modern labor law, antiunion tactics, and union history. Learning from the participants themselves how to reshape the research design is another illuminating aspect of Lendler's approach. For instance, through interviews with workers Lendler realized that for them the strike began before the official first day. The ten months before, when the workers supposedly "ran the plant backwards," helped set the tone of the conflict and formed the attitudes of the workers while on the picket lines. As Lendler explains:

> "That end came suddenly, on March 31, 1985, when the contract expired. Since this is a study of how people react to the shattering of socially constructed routine, it might seem more appropriate to take the strike itself as the starting point. That is when the employees literally traded one set of daily tasks for another. But the strikers themselves were emphatic that the ten months spent working without a contract were a formative part of the conflict" (pp. 40–1).

A more distant researcher might have missed such an important part of participants' construction of their injustice frames. Indeed, during the prestrike period, while workers toiled without a contract, Lendler documented remarkably high levels of resistance to plant authorities and support for one another.

Throughout the work Lendler notes the possible drawbacks within his design, and he offers alternative interpretations of what his research might mean. One admirable aspect

of this study, among many, is how he gracefully puts himself into the study, too. He relates how he also experienced labor trauma when he and other faculty were "restructured" out of their positions at a college where he worked. His unwillingness to study these strikers as an outside elite observer from on high (the college professor) is an excellent example of the benefits of nonhierarchical consciously nonexploitive fieldwork. Throughout, the workers speak in their own voice, describing their experiences "with a language which located the terms of the conflict in pride, dignity, and social justice" (p. 60). The interview materials are supplemented and supported by deft surveys Lendler also conducted.

Crisis and Political Beliefs shows that people's political consciousness can change in times of crisis. The strike redrew participants' political geography, revealed some hard-edged class hostilities within their "politics without awe" (p. 80), and displayed their political world more clearly "when the gloves are off" (p. 111). "Alienation," Lendler writes, "may stem from a vague distrust of poorly understood processes or from involvement which fails to achieve ends seen as just. In the case of the Colt strikers, it appears to be the latter" (p. 80). Lendler's research and insights make this a very important addition to our literature. Why would beleaguered workers stay on strike for more than four years? "The rational solution to satisfying daily needs—crossing the picket line to go back to work—was available. The utility-maximizing perspective accounts for some of the prose of the strike, but not its poetry" (p. 109). Our understanding increases because Lendler did not leave out the poetry.

The Future of Governing: Four Emerging Models. By B. Guy Peters. Lawrence: University Press of Kansas, 1996. 180p. $35.00 cloth, $14.95 paper.

John D. Donahue, *Harvard University*

Government seems to be in trouble throughout the industrial democracies. Citizens tell pollsters (and affirm at the polls) that they do not trust their government and do not believe it works very well. While depictions of a postwar public-sector golden age and a subsequent fall from grace are often overblown, governments in the United States and Canada, Western Europe, and Japan *have* dropped in perceived performance and public esteem in the 1960s and 1970s, leading to electoral turmoil, austerity campaigns, and a ferment of reform efforts in the 1980s and 1990s. Now what?

B. Guy Peters takes up no less sweeping a question in *The Future of Governing*. What should government do in an age when means dwindle but missions do not? How can politicians and bureaucrats deliver on citizens' expectations despite weakening claims on authority, resources, and popular legitimacy? Why did systems of government that seem to have functioned tolerably well for the industrial democracies in much of the postwar era stumble so badly (and so close to simultaneously)? And how can the surging wave of theoretical work on public-sector reform be harnessed to the service of better practice? Peters has an eagle eye for the right questions.

The Future of Governing is as ambitious as its title suggests. Not only the questions engaged but also the intellectual terrain surveyed are of intimidating scope. This short book features 24 small-print pages of bibliographic references, including works in five languages and covering several disciplines (economics, philosophy, public finance, and business administration in addition to Peters's home turf of political science and public administration.) In less practiced hands such an enterprise could melt into a murky muddle of jargon-laden abstractions, but Peters remains oriented toward concrete problems, and his writing is almost always crisp and coherent. (It is seldom more than that, however; he regrettably reserves for the footnotes a wit that is mostly suppressed in the text, such as the suggestion that the American cliché "bang for the buck" must be "frappe for the franc" among French military reformers; p. 142).

Peters attempts to herd several decades of reformist thought and practice from throughout the developed world (but especially the Anglo-American countries) into four broad models, each with its core diagnosis of what ails government, each with its characteristic approach to defining the public interest, and each offering its preferred alterations in governmental structure, management, and policymaking processes. The "market government" approach sees the public sector as inherently flawed wherever it departs from the private sector's defining virtues of choice and competition. Traditional government grows from the twisted roots of collectivism and coercion, which must be ripped out and replaced with arrangements that mirror or defer to private markets. The "participative government" model sees the problem not as collective action per se, but as hierarchical structures that deter participation. Including elements as seemingly disparate as communitarian philosophy and the "total quality management" notions borrowed from business management, this approach calls for flattening hierarchies and moving authority closer to the citizens to be served. The "flexible government" model identifies the enemy as static governmental structures that cannot but perform badly in a rapidly changing world and endorses a range of reforms meant to overcome inertia and speed public-sector adaptation. And the "deregulated government" model, which laments the profusion of checks, controls, and safeguards that prevent public servants from exercising their judgment and discretion, prescribes a series of measures to liberate bureaucratic creativity.

As grand organizing constructs go this is not bad at all, although (as with any ambitious taxonomy) particular assignments among categories can seem debatable and boundaries rather arbitrary, while superficiality is an ever-present risk. And when Peters departs from his efforts to categorize scores of individual scholarly and practical works into the four models, and attempts to deploy the models as competing, coherent paradigms, there is an inevitable tendency to caricature.

Peters knows both the parallel universes of public-sector reform: the practitioners who maneuver, mostly by instinct, among real-world constraints and the academics who craft their conceptual constructs disciplined by scholarly standards of coherence and rigor. He observes how seldom these universes intersect. Formal analysis only rarely guides practical reforms, while "analysts really do not know what has been happening as governments invest huge amounts of time and energy to change themselves" (p. 113). Peters sympathizes with both camps, although his ultimate loyalties seem to be with the academy, as when he compares practical reformers to "candidates in a New York City election who go through the city eating pizza in an Italian neighborhood, pirogies in a Polish neighborhood, blintzes in a Jewish neighborhood, and enchiladas in a Hispanic neighborhood" (p. 128). Theorists may get heartburn just thinking about it, but New York politicians have their own good reasons for enduring such a diet at election time, and front-line reformers have their own good reasons for an untidily eclectic pragmatism.

"Many of the same ideas for change have been lurking in government for decades, if not for centuries," Peters notes,

"and they keep reappearing, albeit with different names and slightly different twists" (p. 112). Democratic government is a very old problem with a finite number of potential remedies and no possibility of a once-and-for-all fix. This neither minimizes the value of reform nor the potential for scholars to make distinctive and indispensable contributions. But as Peters observes here and elsewhere, there is a powerful tendency to cycle between the poles of centralization and decentralization, discretion and control, formal structure and flexibility as the dark side of the more fashionable extreme gradually becomes apparent while the other extreme's shortcomings recede from memory.

Peters ends up finding "some validity to all the positions" while stressing "the need for a more highly differentiated and contingent view of the problems and prospects for governing" (p. 125). This conclusion is hardly surprising but no less sound for its predictability. Like government itself, on one of its better days, *The Future of Governing* inevitably falls short of its audacious goals but still gets a lot accomplished. Anyone looking for a cogent bird's-eye panorama of what has been said, written, and done about the whole sweeping question of how people in the industrialized countries go about defining and pursuing their common interests will find this book well worth reading.

Ideologies and Institutions: American Conservative and Liberal Governance since 1933. By J. Richard Piper. Lanham, MD: Rowman & Littlefield, 1997. 449p. $69.50 cloth, $26.95 paper.

Nicol C. Rae, *Florida International University*

This interesting volume addresses a neglected but highly significant issue in contemporary American political science, namely, the relationship between political ideologies and political institutions. Examining the period from the New Deal to the present, the author argues that the effect of ideological change within American liberalism and conservatism has not been confined to the realm of ideas but also has had serious implications for America's governing institutions. As political circumstances and electoral coalitions have evolved during this period, liberals and conservatives have, at different times, embraced presidential or congressional supremacy, an expanded federal government, judicial activism and restraint, and preferences for a greater or lesser degree of states' rights vis-à-vis the federal government. In the New Deal era, liberals advocated a strong presidency, an interventionist federal government, a nonactivist judiciary on economic issues, and a strictly limited role for Congress and the state governments. By contrast, conservatives emphasized the powers of Congress, the need for judicial curbs on activist government, a more limited federal bureaucracy, and the constitutional prerogatives of the states. By at the 1980s, however, these positions had almost completely reversed, with the size and power of the federal government and states' rights being the only areas in which liberals and conservatives had remained fairly consistent.

In light of these developments, Piper proposes two hypotheses: (1) Ideological movements in the United States have engendered "institutional prescriptions and governance theories" that are "largely but not fully instrumental" (p. 7) in furthering the movements' goals and political power bases; and (2) these institutional prescriptions have had consequences for political institutions, "other aspects of the political process", and other "components of the ideology" (p. 4). Most of the book consists of a detailed examination of ideologies, institutions, and theories of governance during four periods—1935–45, 1945–64, 1966–81, and 1981–93—distinguished by significant changes in liberal and conservative ideologies and political power bases. For each period the author finds that his two hypotheses are largely corroborated.

Piper provides a very thorough analysis of the essential scholarship and original sources on ideological developments within liberalism and conservatism since the New Deal, and he convincingly relates these to electoral changes and changing ideological attitudes toward governing institutions. In this regard his book will be valuable for students of twentieth-century American liberalism and conservatism, as well as those conducting historical research on changes within and between the various branches of the American political system. Piper also raises a couple of interesting issues for further research. First, in his discussion of the New Deal, he indicates that ideology suddenly became a more significant factor in American politics during the 1930s and has remained so since (p. 25). While this is probably an accurate assessment, it also prompts the reader to speculate about the role of ideologies in American politics prior to the New Deal and whether Piper's hypotheses could be applied to earlier eras in U.S. history, as he implies with reference to the Federalists and the Jeffersonians in his Introduction (p. 2). The second question prompted by Piper's study is whether the evident use of political institutions by ideological movements as means rather than ends in themselves has been a major contributing factor to the widespread public cynicism regarding national governing institutions so prevalent in recent years.

The major problem with this book is Piper's conclusion that liberals and conservatives have regarded political institutions as "largely but not fully instrumental" (p. 7). From the evidence presented here, it is not apparent why the qualification is in order, since in each of the ideological phases examined by Piper the evidence appears overwhelming that political developments, such as the Vietnam war, that placed liberals' and conservatives' current institutional prescriptions and theories of government into question *were* the driving forces in changing liberal and conservative perspectives on political institutions. In short, when institutions have gotten in the way of liberal and conservative political agendas, it has been the institutional prescriptions that have changed, not the ideologies. An additional problem is the role assigned to political parties, interest groups, and party factions, relegated by Piper to the status of "other actors in the political process" (p. 4), but these have arguably been as important as ideological movements in fomenting change in American political institutions since the New Deal. Finally, the author presents several tables showing liberal and conservative support in Congress on selected issues from the 1930s to the present, but in many cases the sources of the data and the basis of Piper's classification of members into liberal and conservative categories are not indicated in the table or the surrounding text.

These misgivings should not detract from the fact that Piper has produced a stimulating piece of scholarship on an arresting topic. He follows in a worthy (and today regrettably undervalued) tradition that combines historical and theoretical approaches to the study of American political institutions, in the style of such distinguished practitioners as A. James Reichley and James L. Sundquist. Piper further echoes the latter in his willingness to tackle major historical and theoretical issues in American politics that are all too often avoided by contemporary political scientists in favor of narrow and more easily quantifiable research topics.

The interaction among ideologies, parties, and governing institutions is one of the most complex, elusive, and yet

fascinating areas of American political science. This rich, well-written, scholarly book reflects that elusiveness and complexity, and it also makes a comprehensive and intellectually engaging effort to unravel it. If this reviewer cannot agree with all the author's conclusions, he strongly believes that Piper should be commended for having the intellectual ambition to tackle a difficult but essential subject.

New Schools for a New Century: The Redesign of Urban Education. Edited by Diane Ravitch and Joseph P. Viteritti. New Haven, CT: Yale University Press, 1997. 320p. $30.00.

Kevin B. Smith, *University of Nebraska-Lincoln*

The list of contributors to this volume of essays reads as a who's who of conservative intellectual firepower supporting the campaign for educational reform. And, as befits a book stuffed with contributions from the likes of Dianne Ravitch, Paul E. Peterson, John Chubb, and Chester E. Finn, this stands as a powerful and persuasive brief for the conservative educational cause. For the most part, the authors tackle the unenviable task of offering reasonable solutions to some of the seemingly intractable problems of urban schools, and in the process they make a series of significant contributions to the ongoing debate on urban school reform.

But while the writing and level of scholarship are uniformly high, this is also a frustrating book. Although the word "new" appears twice in the title, with the exception of some relatively minor data-based contributions, there is little that is fresh in the arguments or their presentation. The well-known conservative script of villains, heroes, and solutions is once again repeatedly pressed into service as the underlying framework in many of the essays. I am not altogether sure we need to be told one more time that teachers' unions and bureaucracy are bad, that decentralization and entrepreneurship are good. Regardless of whether you agree with this perspective, it should be clear by now that teachers' unions are not going to disappear from the political landscape, some form of centralized administration will remain in public education, and reinventing government involves costs as well as benefits. Dealing with these realities is harder than divvying up blame and credit, but the effort would also be more accurately described as "new."

Such minor gripes aside, the essays themselves are a mixed bag and are only loosely connected by the general theme of urban education. They include a fairly damning analysis of the bureaucratic pathologies of the New York City public school system, an upbeat update on the spread of the charter school movement, an incisive case study of school reform in Chicago, a treatise on school-based management, and a lament on the politics of education that ends on a hopeful note for change. The lack of a tight cohesive theme should not be taken as a criticism; the eclecticism of the essays contributed to an informative and easy read, and anyone interested in education reform can profitably "cherry pick" essays of interest.

Some of the individual essays are highly noteworthy. I rate among the best Valerie Lee's contribution on what public schools can learn from Catholic schools. Drawing heavily from a broader research project, Lee concludes there are three basic lessons. (1) The core curriculum restricts academic choice, which stands in contrast to "a smorgasbord" of courses that most public schools offer and then ask "students who make poor choices to suffer a lifetime of responsibility" (p. 160). (2) The focus is on the group rather than the individual. "Catholic schools direct their attention to the social rather than the individual basis of human engagement" (p. 161), which promotes a cohesive sense of community within them. (3) Education is explicitly treated as a cultural and moral enterprise. Some of this has religious undertones, but the general point can be considered in more secular terms: "Catholic schools define education in terms of forming the conscience of all students toward and awareness of what they share in common" (p. 162). Much within these conclusions presents a fairly stark contrast with the modus operandi of the public school and those who defend it as well as with many of the individualistic, market-based arguments scattered through other essays in the book. Lee's essay is among the shortest, but it is one of the more provocative intellectual contributions of the volume.

Other essays are less satisfying but still make a worthwhile read. John Chubb's contribution is an insider's explanation of the trials and tribulations of the Edison Project, the private education company and brainchild of entrepreneur Chris Whittle. Edison arrived with a bang, with much-discussed plans to open a national chain of private schools, revolutionize the delivery of education services, and do it all while making a profit. The actual record has fallen considerably short of this promise. The private school operation was largely abandoned in favor of seeking contracts to run public schools, a move with mixed results. Edison's "redesign" of pedagogy and administration has been dismissed by critics as a lengthy and expensive effort that describes what is already accepted as good practice. And even a charitable capitalist has to concede that Edison has had a hard time making money for its investors. To his credit, Chubb ducks none of this and puts up a spirited and detailed counter. He offers a solid defense of Edison's record and provides a particularly lucid portrayal of the organization's willingness to experiment and take chances. Still, one can be excused for wondering why advocates of market-based reform such as Chubb are not quite as forgiving of setbacks and missteps in public operations as they are in private ones. It would be unfair to characterize Chubb's essay as an apologist's exercise, but it is also accurate to say that a number of the "I-told-you-so" crowd of public school defenders are going to interpret it on exactly those terms.

A handful of the essays have a highly partisan tone that seems to crowd out common ground between opposing sides of the reform debate. An example is the essay on Milwaukee's school choice experiment by Paul E. Peterson and Chad Noyes. The essay is outlined by borrowing Dante's description of Hell's circles and describes a program that succeeded despite being sabotaged by scheming choice opponents from the beginning. Perhaps a supportable perspective on the Milwaukee program but less inflammatory interpretations and less absolutist conclusions would be more defensible. The essay also disappoints because it takes the opportunity to renew an unpleasant exchange between Peterson and John Witte. Witte, of the University of Wisconsin-Madison, was the primary evaluator of the Milwaukee choice program, and he collected and analyzed data in a series of annual reports. Along with a series of colleagues, of whom Noyes is the latest, Peterson in a number of forums has all but charged Witte (unfairly, in my own opinion) of being a stooge of antichoice ideologues, with specific accusations running from bad science to unwarranted embargoing of data. The essay's section on evaluation begins with Dante's eighth circle, where "the practitioners of deceit and fraud" are found, and follows with barbs like "an acrid stench surrounds the manner in which the evaluator was selected" (p. 133). While many in the choice exchange (including myself) have contributed to the heat of this debate, beyond some point hostile semantic

broadsides become counterproductive. The fact that the essay mostly avoids the pointed methodological critiques made of Peterson's reevaluations of Witte's work adds to the general impression that this essay is very close to crossing that point.

The bottom line on this book is that, agree or not, and I suspect most readers will do both depending on the essay, taken as a whole it provides a provocative and stimulating intellectual ride. Ravitch and coeditor Joseph P. Viteritti have put together a volume which, regardless of its audience, will serve to "stir things up." This, I strongly suspect, was their primary motivation and hope. As such, they are to be commended; the book stands as a notable success on these terms and is highly recommended.

Narratives of Justice: Legislators' Beliefs about Distributive Justice. By Grant Reeher. Ann Arbor: University of Michigan Press, 1996. 338p. $21.95.

Paul Schumaker, *University of Kansas*

Drawing on in-depth interviews conducted in 1988 with 35 state senators from Connecticut, Grant Reeher provides an impressive and engaging account of elite belief systems, focusing on attitudes concerning the distribution of material goods. Reeher argues that these senators form three distinct ideological types, and the central chapters of his book are devoted to describing "narratives" containing the beliefs of "Critics," "Supporters," and "Ambivalents."

Critics, who comprised nine of the senators, hold a variety of ideas that depart from American orthodoxy. First, they believe that the lower class is a victim of power plays by the wealthy to maximize profits. Second, Critics embrace the distributional principle "to each according to his (or her) needs." While recognizing the importance of incentives and desert-based principles of justice, they critically evaluate those institutions and practices that fail to provide the disadvantaged with essential economic, psychological, and social resources. Third, Critics seek transformations in human psychology. Believing that unjust distributions are rooted in human greed, fear, and hatred (especially as these are manifested racially), they stress instilling empathy and compassion, especially through integrated education. Fourth, Critics see the "government as hero" primarily because of its capacity to achieve some equalization through progressive taxes.

Supporters, who comprised seven of the senators, hold views that are opposite the main beliefs of the Critics. First, Supporters doubt that the lower class is a victim and believe instead that the path of upward mobility is open to all. Second, downplaying "need" as a basis of just distribution, Supporters endorse the market as a distributor of economic goods because its allocations reflect merit and desert. Third, Supporters seek a transformation in human psychology different from that sought by Critics. Believing that people fail in the capitalist system because of personal vices, they seek to instill motivation, character, and self-reliance. Fourth, Supporters see "government as villain" and believe that its redistributive policies corrupt self-reliance and the efficiency of the market. By calling for major changes in human character and by seeking a minimal state, Supporters are, according to Reeher, as "profoundly reformist" as Critics.

Nineteen of the senators were classified as Ambivalents, whose ideas are shaped by a perception of difficult trade-offs between such things as economic efficiency and equality. Recognizing strengths and weaknesses in both market and government, Ambivalents seek technical and incremental adjustments to the status quo rather than profound reforms. First, they view capitalism as better than all other economic systems, but they recognize that it fails to provide mobility and opportunity for those who are unprepared to participate in it. Second, Ambivalents emphasize that the key to economic fairness is "accessibility and impartiality." Economic growth is seen as vital because it provides the disadvantaged access to economic opportunities. Market-driven inequalities are seen as impartial and thus justified if based on criteria like ability and effort, but unjustified market distributions are perceived. Third, Ambivalents seek lesser transformations in human nature than do Critics and Supporters. Rather than hope that people will lose their selfishness or that they will acquire impeccable characters, Ambivalents are satisfied with providing individuals with an education that increases their efficacy and enhances their skills. Fourth, Ambivalents see government as neither hero nor villain, but as a public agency that can marginally enhance opportunity, especially through strong civil rights laws and extensive early education.

Reeher seeks to make several contributions beyond describing these three worldviews. First, his work suggests that his typology will better contribute to empirical and normative theories of ideologies than do conventional classification schemes involving various forms of conservatism, liberalism, and socialism. This suggestion is plausible if people's concerns about the distribution of material goods dominate their political thinking and action, as justice principles play a more central role in Reeher's typology than they play in most other characterizations of ideologies. But Reeher's useful comparisons among the views of Critics, Supporters, Ambivalents and people holding more conventional ideologies fall short of the kind of analysis that demonstrates the superiority of his classification scheme.

Second, Reeher seeks to establish the relevance of the normative beliefs of policymakers to the legislation they produce. Most paradigms of policymaking ignore or downplay the importance of the justice principles held by decision makers, but Reeher examined three policy issues that arose in Connecticut at the time of his interviews (educational finance, property tax relief, and plant closings) to analyze the effects of legislators' beliefs on their policies. Arguing that beliefs are "filters," he provides an important discussion of how legislators' belief systems enhance or reduce the effect of those factors that are normally studied as the prime influences on policymaking—such things as party and constituency influences and the role of interstate competition for business. While this discussion is provocative, the reader may also wonder when and how legislators go about directly acting on their justice principles. While Reeher provides an interesting analysis of why Critics are deterred from acting directly on their justice principles (they overestimate their political isolation), it would have been equally interesting to examine the opportunities that legislators perceive for pursuing these beliefs.

Third, Reeher seeks to demonstrate the value of "narratives" in conducting and presenting research on political belief systems. The approach solicits stories about specific events and people from respondents and then uses these stories to discern the respondent's abstract beliefs and values. Reeher suggests that the narratives told to him by the senators enable a better understanding of ideologies than do responses to survey questionnaires, the approach that has thus far dominated empirical research on belief systems. Judging from the extracts of the interviews provided, however, Reeher did not collect and analyze narratives, understood as stories. Instead, he asked his respondents to provide answers to an impressive array of abstract questions, and they

usually provided abstract ideas, rather than stories, as their answers. Thus, Reeher's work may show that ideologies are better studied through open-ended interviews than through fixed-response survey instruments, but his study does not demonstrate the value of narratives in collecting data on belief systems.

Narratives can also be understood as expository devices for portraying various belief systems, and it is this use of narrative that Reeher most effectively employs. From the interview data, he has constructed three distinct narratives or stories, each "with a plot and a cast of characters possessing certain qualities . . . [that] both explain and judge perceived outcomes" (p. 249). Reeher believes that these products of his research might facilitate better normative evaluations because humans seem better able to judge the merits of competing narratives than the merits of competing abstract principles of justice. In this regard, he suggests that narratives may be more persuasive than philosophical arguments. Thus, the case for more economic equality—which remains murky given the incommensurability of competing philosophies of justice—may become more apparent if one compares the merits of the Critic's narrative with that of the Supporter. Again, this claim seems plausible. Figuring out a valid method for testing this claim would be an interesting and important future development in Reeher's approach to understanding distributive justice.

Public Spirit in the Thrift Tragedy. By Mark Carl Rom. Pittsburgh, PA: University of Pittsburgh Press, 1996. 326p. $49.95 cloth, $22.95 paper.

Gary Mucciaroni, *Temple University*

The spectacular failure of the savings and loan industry during the 1980s, resulting in losses to taxpayers of approximately $180 billion, is the subject of this volume by Mark Carl Rom. It will undoubtedly become the definitive work in political science on what is believed by many to be the most significant public policy fiasco of recent memory.

Few policy areas are as complicated and full of technicalities as this one. Rom's presentation of the case, guiding us through the arcane worlds of financial regulation, banking, and accounting, is as lucid as can be expected from any scholar. The volume begins with a literature review, a summary of the main argument, and a brief history of the S & L business and policy commitments related to it. The bulk of the book is split between an investigation of the role of regulatory agencies and that of Congress in the saga.

Rom's interpretation of the thrift tragedy is refreshing, plausible, and consistent with the considerable evidence that he musters. His thesis is that the thrift tragedy occurred despite the effort of policymakers to act in the public interest and even because of it. Therefore, self-interest alone is "a woefully inadequate explanation of official behavior and policy choice" (p. 9). The argument is provocative and unconventional given that journalistic and academic accounts of the thrift tragedy typically attribute it to fraud, perverse political incentives, institutional incompetence, or some combination. The volume also adds to a burgeoning literature that brings deliberation back into the study of policymaking and challenges the public choice assumption that political behavior is rooted in narrow self-interest.

Rom traces about half of the thrift losses to policy choices that were made before the deregulation of the 1980s. To help homeowners and thrifts finance housing and create savings, Congress deliberately encouraged the industry to make long-term, fixed-rate home mortgage loans and to accept interest rate ceilings and short-term deposits backed by federal deposit insurance. Eventually these policy commitments left thrifts vulnerable to sharp increases in interest rates in the 1970s, which aggravated the "mismatch" between what the thrifts could earn on their fixed mortgages and what they needed to pay to attract deposits. Under deregulation, the losses increased substantially because S & Ls were empowered to pay whatever they wished for funds and to invest in projects, many of which were highly risky and in which thrifts had no experience. Again, however, Rom argues that "these policies were adopted to benefit the public by making homes affordable, savings safe, and thrifts stable" (p. 211). The fact that the overwhelming preponderance of opinion, including that of independent experts, supported deregulation at the time it was considered adds force to his argument. Critics have argued that the crisis was needlessly exacerbated by policymakers' failures to respond to the crisis more swiftly. Rom counters that policymakers were on the horns of a dilemma. The issues were complex and arcane, often without clear or readily available solutions until it was already too late. "In general," he concludes, policymakers "decided cautiously when no consensus existed and quickly when one did: they deliberated when the solutions were not clear and acted when they were" (p. 211).

Rom cannot be accused of shying away from making startling pronouncements. In objecting to terms like "fiasco" as hyperbolic and contending that the thrift crisis was "hardly disastrous," Rom is bound to raise more than a few eyebrows. The argument that other dubious government programs have cost taxpayers lots of money, too, but have received less attention than the thrift crisis is a weak one that ends up sounding like two wrongs make a right. Suggesting that most people probably find a massive bailout for depositors (whose money was squandered in many instances through incompetence, greed, and fraud) as no worse than the mortgage interest deduction seems improbable and constitutes an odd comparison of apples with oranges.

In offering an alternative interpretation of the thrift crisis, Rom concedes that he is not conducting "unequivocal hypothesis tests between the two approaches [i.e., self-interest versus public spirit]" (p. 20). Indeed, it is one thing to show that policymakers made decisions that were reasonable on the merits, given competing policy goals, limited information, and time constraints. It is another to demonstrate convincingly that those decisions resulted from policymakers being motivated by one goal rather than another. Self-interested goals and the desire to make good policy are not mutually exclusive. Moreover, without being able to read policymakers' minds, it is far from clear in many or most instances which goal is paramount. One can usually find policymakers' choices congruent with some plausible self-interest motive like reelection, power, or prestige. Did Congress refuse to adopt adjustable rate mortgages (a decision that undoubtedly contributed to the thrift tragedy) because it saw it as electorally advantageous to side with realtors, home builders, consumer groups, and the homeowner-voters who preferred fixed rates? Or did legislators do so because they thought that it was more in the public interest to protect homeowners from interest rate risk and save them billions of dollars, rather than the thrifts? Similarly, did Congress adopt asset deregulation in response to industry pressure or because there existed a broad and deep consensus that asset deregulation was a good thing? Or was there a combination of motivations at work in these cases? Rom admits that he cannot pin down motives, and so he develops other criteria for making empirical comparisons between the public choice and public spirit perspectives. These have to do with institu-

tional structure, resources, deliberative effort, and other aspects of the policymaking process. But these are not sufficiently refined and unambiguous to test his contention that self-interested behavior played only a residual role in this case.

If Rom's interpretation is correct, then it confirms the notion that good intentions are not enough to avoid costly, unintended consequences. (One crucial question is how much, if at all, does public spiritedness help to reduce or avoid such consequences compared to self-interest?) Policymakers also need to be smart. Yet, high levels of uncertainty are intrinsic to many policy issues, regardless of policymakers' willingness and capacity for deliberating on the merits. Even when uncertainty can be theoretically reduced, the resources available to overcome uncertainty and reduce error may be insufficient.

Readers may take away from Rom a message that is at the same time reassuring and disconcerting: We have less reason to worry about policymakers' motives and incentives than cynical perspectives on American politics would have us believe, but we have more reason to be concerned about our capacity to gain control over an environment that demands timely answers to complex problems, and whose solutions are uncertain or pit one desirable goal against others.

The American Presidency under Siege. By Gary L. Rose. Albany: State University of New York Press, 1997. 231p. $18.95.

James P. Pfiffner, *George Mason University*

The premise of this book is that the contemporary presidency is under siege from a number of debilitating factors that have developed over the past 25 years and that it is "incapable of creative leadership" (p. xii). Rose presents a well-written, solid analysis of recent presidencies, despite several potential flaws in the overall argument. The first two chapters lay out many of the symptoms of the problem. Presidents are hemmed in by "a monstrous and behemoth-like federal bureaucracy capable of thwarting the implementation of presidential policy, a viperous and tabloid mass media with an insatiable thirst for scandal in the White House and a Congress . . . fully determined to constrain the powers of the presidency" (p. 14).

Chapter 3 presents the heart of Rose's analysis. The deterioration of presidential power has been caused primarily by the decline of political parties in the United States, beginning with the proliferation of primary elections. His historical and developmental analysis is quite good and brings together much of the best political science literature on political parties and their decline over the past several decades. Rose concludes that "the relentless onslaught of anti-party forces throughout the twentieth century" results in an individualized presidential selection process so that presidents in office do not have the necessary support or leverage to enact the policies they want. "It is this condition that explains the failure of recent American presidents" (p. 68).

The core of Rose's solution to the problem of presidential leadership is presented in a chapter on political party reform. He argues that reestablishing the clout of political parties will restore the power and prestige of the presidency. He argues for returning control of presidential nominations to those most committed and able to make wise choices, that is, party regulars and activists, supplemented by rank-and-file party members. He would set aside up to one-third of public campaign financing for the parties, allocate free air time for party advertising, tighten lobbying and revolving door regulations, and further insulate political parties from state control.

While arguing that stronger political parties are most important, Rose says that some legal and constitutional changes are also necessary: cutting the "unaccountable and autonomous" (p. 129) White House staff, giving the president greater reorganization authority, repealing the 22 Amendment, and giving the president the item veto. One of his more controversial reform proposals to strengthen the political parties is to give the president "at least one hundred thousand" (p. 113) political appointments to help him control the executive branch, rather than the current 3,000. Rose begins with the premise (without citing examples) that presidents are continually thwarted by intransigent bureaucracies. But if "the bureaucracy" is so strong and invincible, how was President Reagan able to cut domestic programs so severely in 1981? How was President Bush able to cut defense so sharply in the late 1980s? How was President Clinton able to cut the federal workforce by 300,000 in the mid-1990s?

Transition recruitment of political personnel is a slow-moving administrative nightmare as attested to by professionals from both parties. Increasing by even a small amount the number of people to be recruited, screened, and appointed would overwhelm an already overloaded system, let alone the problem of Senate confirmation of the increased numbers. Introducing 100,000 new political appointees, however competent, who must be trained every four years is a prescription for administrative disaster.

Rose's overall argument is that (1) presidential leadership is crucial, (2) more presidential power is necessary for that leadership, and (3) stronger political parties along with more formal power can restore the presidency. One could grant the first point and admit that parties were an important component of presidential leadership before the 1960s but still reject points two and three.

Rose's analysis of the factors that make current presidents' jobs more frustrating—such as weaker parties, hostile media, declining public support, and a contentious Congress—is convincing and well grounded in the political science literature. But whether these factors are as debilitating as he asserts is arguable. Each of the contemporary presidents he cites won some important policy victories, even if they are not now seen as "near great." But neither was Harry Truman or Dwight Eisenhower in his time.

Even more arguable is whether a return to strong parties would result from his reform proposals, or if parties were to become stronger (as they are in the use of soft money and party voting in Congress), whether it would result in a stronger presidency. Other factors, such as the U.S. role in the world, a hostile press, and a skeptical public, would probably not allow a return to Rose's ideal age of more than three decades ago. But this does not mean that great presidents are no longer possible. Great presidential leadership depends upon an exceptional individual, a certain level of policy consensus, wise policies, and, some would argue, a crisis to confront.

More broadly speaking, whether the problems facing the presidency are primarily the product of the past 25 years, as Rose argues, is open to question. Other scholars, in examining the same presidencies and problems that Rose documents, present different perspectives on presidential leadership. Historian Arthur Schlesinger, rejecting structural and institutional remedies, argues that there is nothing wrong with presidential leadership that better presidents cannot cure. He argues that good leadership will overcome the systemic obstacles to consensus as past great presidents have.

Political scientists Charles O. Jones and David Mayhew

reject a presidentialist perspective and argue that we should be less concerned about how presidents fare and more concerned with how the political *system*, particularly the interaction between president and Congress, deals with national policy problems. In contrast to Rose's claim that the Framers intended a strong presidency, Richard Neustadt based his analysis of presidential power on the premise that the presidency is in an inherently weak position in the U.S. system.

While some of Rose's arguments are open to question, this is nevertheless a very good book. It lays out in detail a careful, reasoned analysis of problems of the contemporary presidency and a coherent program of reform designed to restore presidential vitality. One of the not insignificant contributions is its "executivist" argument for more presidential power that is not based in partisan frustration. It is rather, a reasoned argument based on a philosophical conviction that the U.S. political system has moved away from (what Rose contends was) the Framers' intention for a strong presidency and on an analysis of contemporary public policy problems that Rose argues would be better solved if political parties were restored and the presidency strengthened. One can disagree with both the philosophical underpinnings and prescriptions and still admit that this is a fine book and a well-made argument.

Medicaid and the Limits of State Health Reform. By Michael S. Sparer. Philadelphia, PA: Temple University Press, 1996. 235p. $54.95 cloth, $19.95 paper.

Pauline Vaillancourt Rosenau, *University of Texas*

Today, the federal-state balance leans to the states, and in the absence of national leadership on health policy, the states have gone in different directions. Michael Sparer offers a much needed balance to the one-sided, state-oriented "devolution-revolution" approach to public policy. He argues that, while significant authority over health policy had been given to the states in the past, the results were far from successful. This conclusion is not based on ideology or philosophy so much as on careful consideration of a critical health policy arena—Medicaid. We learn in this volume that state public policies may have serious consequences for residents, that policies vary dramatically across states, and that some states fail their populations altogether.

I would use this book as a text in any public policy course. It is well organized and tightly written. It is a blessedly short book, to the point. The prose is clear and lucid. The text never wanders, and little energy is wasted on what is not pertinent. The author synthesizes enormous amounts of background material for all 50 states in a concise fashion. He examines variations in Medicaid policy across the states, reconciling conflicting findings, drawing in historical material where relevant, and never forgetting that the political culture of each state is different.

A comparative study of Medicaid in California and New York is presented with four in-depth case studies in each state: nursing home policy, home care policy, hospital policy, and managed care policy. The author aptly ties the difference between Medicaid policy in California and New York to the debate about who governs and whether policy is society centered (high interest group input, as in New York) or state centered (state bureaucrats hold power, as in California). The conclusion is that there is no single answer to the question of "who governs?" The response varies from state to state and from one policy arena to another.

The health policy choices that citizens and governments must make are clearly described in this book. They are, in short, summarized by a poster that Tom Bodenheimer observed in the office of an Oregon Health Plan administrator: "Cost, access, quality—pick any two" (cited in "The Oregon Health Plan—Lessons for the Nation," *New England Journal of Medicine*, September 4, 1997, p. 723). From Sparer's analysis it appears that California chose *cost and access* while New York chose *quality and access*. Two states, two different ways of making Medicaid policy; but neither does a very good job. Fraud and abuse coexist with New York's generous programs (pp. 96–7; 121–2); quality compromises plague California's attempt to manage Medicaid. The variation between these states reflects the huge differences that exist across the nation.

Medicaid and the Limits of State Health Reform is likely to remain relevant in the future, perhaps even more than its author and publisher could possibly have anticipated. The 1997 Balanced Budget bill and its Children's Health Insurance Program offer states a great deal of flexibility and responsibility as to how federal requirements are met, how funds are allocated, and how programs are actually administered. Last year the Health Insurance Portability and Accountability Act of 1996 (HIPAA) gave states leeway as to what to do with people who move from a job with health insurance to one with no employer-sponsored coverage. The law does not require that insurance companies provide equivalent coverage. Rather, it permits states to put such individuals into high-cost, limited-benefit risk pools. But is this portability? Each state is a laboratory, but the bottom line is that the states are also testing grounds for private interests who opposed legislation at the federal level and who seek implementation loopholes at the state level.

This book gives pause as to the future, however, for another reason that could not have been predicted. There is a generally positive economic climate today (low inflation and low unemployment). Everyone is hard at work, and that includes many who used to depend on Medicaid. Many states appear to be managing programs better, including Medicaid, when in fact they are merely profiting from events that are beyond their control. States find it easier to balance their budgets because more citizens are employed and therefore paying taxes. Thus, the states have higher than expected revenues and fewer unemployed poor seeking Medicaid eligibility. Those forced off welfare find it easier to find a job in a growing economy. But there is no guarantee as to how long this best-of-all-possible-worlds will last. If the economy turns around, unemployment rates may increase, and there will be greater demand for Medicaid at the same time that the states face lower overall revenues because the unemployed do not pay taxes. A crisis of enormous proportions is likely to ensue, and the states will face hard choices, such as those outlined in this book. They can toughen up eligibility for the Medicaid program, or they can follow the example of California, where state Medicaid administrators offered a cost-control deal that nursing homes could not resist: Accept low reimbursement rates in return for superficial attention to quality oversight by state inspectors (p. 193).

I was left with only one question about Medicaid policy after reading this book. Political parties and their accompanying ideologies are not directly included as explanatory variables. I cannot help but wonder if New York's historically more generous Medicaid benefits ($7,286 per beneficiary) reflect, in part, that state's Democratic Party heritage. Does California's lower Medicaid expenditure ($2,801 per beneficiary) mean that tight-fisted bureaucrats left over from the Reagan years still disproportionately populate officialdom and implement their political point of view thanks to the

considerable bureaucratic autonomy existing in that political culture?

Perhaps the most important conclusion from this book is that if cost containment is the first priority for health policy, this requires "bureaucratic centralization not interest-group politics" (p. 197). This mechanism for controlling costs remains unpopular in the United States today because it acquiesces to federal government power over the states. Instead, we have turned by default to marketplace reform and competition. The market, as much as Congress, makes health policy today. With only four or five years of experience behind us now, it appears that this approach is failing to bring health costs into line with those observed in other industrialized countries. Apparent failure, however, does not seem to dampen enthusiasm for the market—at least not yet.

The Politics of Social Welfare: The Collapse of the Center and the Rise of the Right. By Alex Waddan. Cheltenham, UK, and Brookfields, VT: Edward Elgar, 1997. 204p. $70.00.

Stathis N. Kalyvas, *New York University*

How did the Right win the battle of ideas surrounding the welfare state? How did liberals allow the debate to shift from social justice and poverty to dependency? In other words, how did important parts of New Right ideology permeate the liberal consciousness and become established parts of the conventional wisdom? This book discusses the rise and crisis of the welfare state in the United States and offers an argument explaining the ideological defeat of prowelfare liberals by the Right. It provides a detailed overview of welfare state-building, beginning with President Johnson's War on Poverty (particularly the Economic Opportunity Act), including numerous defeated initiatives, such as President Nixon's NIT (Negative Income Tax) and FAP (Family Assistance Plan), and President Carter's PBJI (Program for Better Jobs and Income). The book's twin focus is on social policy and the ideological context in which social policy was conceived and implemented.

Waddan is particularly keen on explaining "the open and agonizing nature of the collapse of faith in the capacity of social welfare liberalism to provide solutions" (p. 46). The central argument is that the way in which the welfare state was build in the 1960s undermined its future: The model underpinning it was in the long term not a viable one; whatever its merits, it could not last. Overoptimistic and naive liberal reformers defined poverty in a self-defeating way conducive to political and ideological confusion. Overall expectations surrounding the War on Poverty were unrealistic. A philosophical ambiguity and an ad hoc nature seemed to permeate the whole effort. The demise of the welfare state was built in, so to speak, from the very beginning and in its very core. Even when prowelfare ideas enjoyed a political hegemony, Waddan argues, there was little consideration of the long-term implications of welfare policies. While governments engaged in the correction of identifiable social wrongs, there was either an unwillingness or an inability to think through the possible implications of each incremental measure, an absence of reflection about the societal costs imposed by welfare policies, and a reluctance to assume these costs and implement adequate reforms.

An example of how the unrealistic expectations of liberal reformers hurt their long-term goals is seen the way in which they sabotaged the Nixon era reforms, particularly the FAP, thus missing an opportunity to fortify a philosophy committing government to help the poor under virtually any circumstances. Welfare measures slogged along in a haphazard way: No real integrated structure was created, while the continued expansion of welfare rolls was not followed by a rationalization of the system. The fragmented nature of the welfare structure, particularly its residual-type elements, made it an easy target for opponents. As a result, when welfare policies met with the fiscal pressures stemming from the economic dislocations of the 1970s, particularly the recession of 1979–80, they were promptly defeated on the ideological front. It was an easy task for the New Right to connect the issue of taxation with the notion of the undeserving poor, turn poverty into dependency on welfare, and drive home its point that welfare programs undermined both the health of the overall economy and the dignity of the individual.

Although this is an interesting and plausible argument, it is just that: an argument. No competing explanations are offered, no alternative hypotheses examined. Indeed, Waddan readily acknowledges that his book is "essentially a commentary" on the crisis of the welfare state in the United States (p. 9). He does not make any particular effort to convince the reader why his argument is more valid than others that have been (or can be) advanced to explain the same phenomenon. Moreover, even the causal mechanisms underpinning his argument are never specified: They are vague and shifting. A comparative framework could have helped in this respect (in fact, the book's title is misleading: its subject matter is the politics of social welfare *in the United States*). Although Waddan claims that the American case is a valuable guide to the problems encountered by the European welfare states, he takes a stand in favor of U.S. exceptionalism: "Clearly developments in the US cannot be divorced from the country's particularly idiosyncratic historical approach to social welfare issues; and obviously this tradition helps to explain why American liberalism has suffered such a spectacular and painful crisis of confidence" (p. 3). He then points out (p. 172), however, that the "relatively weak collectivist traditions" of the United States do "not fully explain quite why liberal advocates were so badly beaten." This vagueness underlines the absence of a rigorous specification of the central hypothesis. A related but more serious problem is the absence of theory from the analysis. Even though Waddan refers to welfare state theories (such as Offe's and Esping-Andersen's) at the beginning and the end of the book, he makes no systematic attempt to connect his argument to these theories. On a more general level, while he makes several implicit assumptions about the role of ideas in the success or failure of social policies, there is no explicit theoretical discussion of either the role of ideas in policy-making or the interaction between ideas, interests, and structures; Waddan seems to ignore the thriving relevant debates.

The absence of original data, a research design, and a theoretical framework turns this book into a purely descriptive account of the politics of social welfare in the United States. It is certainly possible to argue that descriptive works do not require a full-fledged research design and theoretical framework; however, the presence of many implicit and explicit causal claims in the analysis interferes with the description. Although this book could serve as both a factual reference and a commentary on welfare politics in the United States, it is hardly the only one in the market. After all, the author himself suggests (p. 29) a book which, he claims, provides a lengthier and more comprehensive account of interpretations about the origins and long-term development of the welfare state than his.

Don't Burn It Here: Grassroots Challenges to Trash Incinerators. By Edward J. Walsh, Rex Warland, and D. Clayton Smith. University Park: Pennsylvania State University Press, 1997. 292p. $50 cloth, $17.95 paper.

A. B. Villanueva, *Western Illinois University*

As more social scientists with their sophisticated research methods get involved in the study of the environment, they provide us with fresh insights on the dynamics of citizen groups who play the role of private attorneys general for the environment in the public interest. Out of their research emerges data showing that what was usually taken for granted as valid generalization about the relationship between democratic practices and environmental outcomes turns out to have no empirical support. As the findings of this research have demonstrated, there is no "positive relationship between democratic processes and successful sitings" (p. x) of mass-burning incinerators. Moreover, demographic characteristics of communities in which siting controversies occurred were not as significant in determining the success or failure of siting proposals as previously reported in the literature of municipal solid-waste management. What appears to be a more accurate predictor of siting outcomes is the existence of cleavages among political elites who support the construction of incinerators in the hope of transforming garbage to gold, the involvement of environmental scientists with impeccable credentials supporting the opponents, and the manner in which the issues are framed by both sides of the debate.

Environmental protection bureaucracies have developed a hierarchy of waste-management policies in which volume reduction at the source lies at the top of the totem pole and disposal in landfill facilities at the bottom. The second most preferred technique is recycling and reuse. Combustion with energy recovery and combustion for volume reduction have low priority in the hierarchy. In practice, some county and local governments feel that more can be gained from an economic standpoint by burning trash rather than recycling waste. Environmental quality is evidently not taken into account at all in the decision-making process. The authors of this study correctly point out that a number of forces, such as the privatization policies of the Reagan administration, the energy crises of the 1970s, the flow-control ordinances of local governments (later declared by the Supreme Court as violations of the commerce clause), the increasing shortage of landfills, and the potential profit from this technology by financial lending institutions, set in motion the construction of mass-burning incinerators. Each factor contributed a great deal in the development of policies adopted by some counties and local governments to meet the garbage crisis.

To build or not to build an incinerator is a public policy question. As an issue of public concern, the political arena is replete with actors, including public officials, waste facility developers, consultants, and citizens, who have a negative perception of the effect of the project. These citizens form the core of an anti-incinerator movement aimed at nipping the siting proposal in the bud. The way they behave in the political arena epitomizes direct democracy at work: gathering petitions, attending public hearings, proposing referendums, and threatening to recall elected leaders who turn a deaf ear to their demands. They also give nondecision makers, so popular in the literature on community power structure, a taste of their own medicine: preventing the incineration issue from getting into the policy agenda by bringing pressure to bear on those who control the agenda-setting process and delaying the implementation stage through litigation in the courts. In all the cases reported in this volume, opponents were successful in arousing public awareness of the incineration issue and equally successful in raising the level of its political saliency. Unlike the antienvironmental groups, whose objectives are to stop the government from promulgating regulations restricting their property rights, weaken the power of and influence their local governments, and call for a wise use of the public domain, the anti-incineration groups demand a change in the process and substance of public policy by lobbying local officials to consider other alternatives in the management of municipal solid wastes.

Advising both sides of the incineration debate on how to achieve success in making a case for or against siting proposals is tantamount to a politics of avoidance (pp. 260–2). It is not helpful and useful to policymakers at all in their quest for a politically acceptable and environmentally benign solution to the disposal of trash. There are viable, cost-effective, and environmentally sound alternatives, including recycling and pollution prevention. In their quest for ways to break the siting gridlock, policymakers must keep muddling through to prevent the nation from becoming a wasteland.

The task of crafting a policy of solid-waste management by county and municipal officials will be difficult, because alternatives to incineration are not panaceas to environmental pollution. Recycling cannot eliminate pollution. It can reduce it, but it does little to stop the generation of trash due to the insatiable appetite for consumer goods of a throw-away society. Similarly, pollution prevention is not the magic formula that can bring down pollution to a zero level. Policymakers who are working to make it a goal of environmental policy, desirable and sound as it is, are making promises they cannot deliver.

Environmental science will broaden its base if more research of this caliber is undertaken by social scientists. Additional case studies are needed to examine the relationships between citizens' perception of risks and their degree of opposition to the siting of a new technology, to show the effectiveness of citizen advisory committees in influencing siting outcomes, and to examine the referendum device as a means of resolving the siting conflict. Although this scholarship is the result of only a few case studies and barely scratched the surface of a vast area of environmental study, it is a step in the right direction. Interdisciplinary programs in environmental studies will find this piece of scholarship an invaluable contribution of political sociology to the literature on citizen participation in the environmental policy process.

Comparative Politics

Iranian Intellectuals and the West: The Tormented Triumph of Nativism. By Mehrzad Boroujerdi. Syracuse, NY: Syracuse University Press, 1996. 256p. $45.00 cloth, $16.95 paper.

Gawdat Bahgat, *Indiana University of Pennsylvania*

Unlike many developing countries, Iran has never been under foreign occupation for a long period. But, like many other Muslim states, the Iranian culture has experienced close contact with Western civilization, particularly since the beginning of the twentieth century. In 1908, oil was discovered in commercial quantities and has since accelerated the country's integration into the international system. In the mid-1920s, Reza Khan ended the reign of the Qajar dynasty (1794–1925) and established the Pahlavi regime (1925–79). Like Kemal Ataturk in neighboring Turkey, Khan was eager to adopt Western values, albeit with less success. In September 1941 Khan abdicated in favor of his son Muhammad, who was more enthusiastic in opening Iran's doors to Western technology and values. This uneasy process of interacting between two cultural systems is the subject of Boroujerdi's work.

In order to explain the indigenous response to the perceived external cultural attack, the book seeks to present an integrated analysis of the socioeconomic and cultural transformation of Iranian society in the last five decades. Given the fact that the work of intellectuals represents the public conscience, the author examines the writings of ten scholars: Seyyed Fakhroddin Shadman, Ahmad Fardid, Jalal Al-Ahmad, Ali Shari'ati, Seyyed Hoseyn Nasr, Ehsan Naraqi, Hamid Enayat, Daryush Shayegan, Reza Davari, and Abdolkarim Sorush. They come from different academic disciplines and suggest a variety of approaches for dealing with the West. Two common denominators can be identified in their work, however. First, there is a strong emphasis on authenticity, or what Boroujerdi calls "nativism" (the doctrine that calls for the resurgence, reinstatement, or continuance of native cultural customs). Second, at the heart of this desire to stick to the traditions of Iranian nationalism is Islam and, more accurately, Shi'ism as it has been interpreted and practiced in Iran. Furthermore, there is a recognition of the need to keep Islam alive by introducing theoretical and organizational reform.

In addition to examining the cultural transformation in Iran as it has been portrayed in the work of ten leading intellectuals, the author briefly explores the socioeconomic settings that gave birth to their thought. An important characteristic of the Iranian economy is "rentierism." The concept was originally introduced to the growing literature of international economy almost three decades ago (Hossein Mahdavy, "The Patterns and Problems of Economic Development in Rentier States: The Case of Iran," in *Studies in the Economic History of the Middle East*, ed. M.A. Cook, 1970). It refers to any state that derives a substantial part of its revenue from foreign sources in the form of rent. Iran, like other OPEC members, has suffered from deep dependence on, and vulnerability to, oil as the major source of national income. The stagnated oil prices in the last several years have contributed to the decline of the standard of living for average citizens.

These deteriorated socioeconomic conditions added more challenges to the Iranian intellectuals. The basic question they tried to address was what is wrong with the indigenous system or, put another way, what is right with foreign, particularly Western, systems. In responding to this challenge, many scholars have shown strong appreciation for non-Western civilizations, such as the Japanese, Indian, and Russian. Others have advocated the separation between Western technology and values, that is, import the former and resist the latter. The mainstream Iranian intellectuals, however, have gone through a process of soul searching in which they found comfort in returning to the Iranian heritage. A fundamental question they had to answer was how to identify the "self." Some have found the roots in an Islamic Iran, and others have talked about pre-Islamic Persia. The revolution in 1979 reinforced the Islamic identity of Iran and simultaneously confronted the intellectuals with another major challenge: How does one create an Islamic economic and political model? The domestic and foreign policies of the Islamic Republic can be seen as an attempt by policymakers and intellectuals in Tehran to present an Islamic solution to the problems facing a rapidly modernizing society at the end of the twentieth century.

Boroujerdi's general survey of modern Iranian political thought highlights the significance of understanding the attempt by many developing countries to find a "compromise" between preserving their indigenous cultures and adopting Western values. It is important to point out, however, that the important contributions of the Shi'i Ulama (religious scholars) have not been examined in this work. Equally important, the ten intellectuals covered in this study are men. Women have also contributed to the richness of modern Iranian culture. In addition, the author does not explain his choice of time frame. The challenges of modernization have confronted Iranian society and intellectuals for a longer period than just the last five decades. Finally, there are very few references to similar experiences in other Muslim countries. For example, the work of Pakistani intellectuals (e.g., Abu'l-Ala Al Mawdudi, *How to Establish the Islamic State*, 1969), Egyptian writers (e.g., Taha Hossein, *The Future of Culture in Egypt*, 1938), and Iraqi scholars (e.g., Ayatollah Muhammad Baqir Al Sadr, *Our Economics*, 1982) show some similarities with and differences from the attempt made by their Iranian counterparts to find a common ground between nativism and Western values.

Public Management: The New Zealand Model. By Jonathan Boston, John Martin, June Pallot, and Pat Walsh. New York: Oxford University Press, 1996. 406p. $65.00.

Social Policy in Aotearoa/New Zealand: A Critical Introduction. By Christine Cheyne, Mike O'Brien, and Michael Belgrave. New York: Oxford University Press, 1997. 282p. $49.95.

Michael Mintrom, *Michigan State University*

New Zealand seems so far-flung. As the poet Charles Brasch wrote in the 1940s, "distance looks our way." Yet, since 1984 the country has gone through unprecedented transformations in its politics and public policies, and those old distances have started to collapse. A close resemblance now exists between the problems and issues confronted by New Zealanders and those faced by politicians, policy elites, and citizens in other countries, as changing global forces press upon them. A comparative examination of similarities and differences across 15 countries, including New Zealand, is presented in John Williamson's edited collection, *The Political Economy of Policy Reform* (1993). Here, I review two recent studies of the New Zealand scene written by New Zealand academics.

Jonathan Boston, John Martin, June Pallot, and Pat Walsh provide an up-to-date survey of public sector reforms in New

Zealand since the mid-1980s. These include the establishment of state-owned enterprises, a comprehensive privatization program, sweeping changes to financial management across government entities, and decentralization of much operational decision making. These reforms, while introduced over ten years, all share consistent neoliberal theoretical underpinnings, and they can be said to be mutually reinforcing in the sense that all are designed to make the public sector operate more efficiently. The authors call this set of reforms the "New Zealand Model" and note that "this model has attracted widespread interest, and stimulated no little enthusiasm, among practitioners, scholars, and students of public management in many parts of the world" (p. vii). The intended audience for the book, therefore, appears to be an internationally diverse group of academics and policymakers; those people who are curious about the politics and practice of policy reform and who routinely scan beyond their immediate locale when searching for practical insights and intellectual provocation. For this audience, there is much of interest here, and the authors write with authority. Boston and his colleagues all have established records as chroniclers of the managerial changes that have taken place in New Zealand's public sector. They can legitimately claim to have stood at the epicenter of change, viewing reforms in the making.

In terms of its stated goal of thoroughly documenting the key features of the new public management in New Zealand, I judge the book a success. The chapters cover such topics as institutional design in the public sector, the organization and purchasing of policy advice, accountability and the collective interest, human resource management, and management in local government. My only complaint is that the goal of the book, documenting the New Zealand model, is a little too restrictive. Ironically, the very proximity that has given the authors their expert knowledge of the model also appears to have inhibited the development of a critical perspective. Why have these particular policies been adopted and not others? How sustainable over the longer term are they? What empirically oriented analytical techniques could be employed to evaluate their effectiveness? What features of New Zealand government and politics have rendered these policies unique and, consequently, reduced the potential for successful emulation elsewhere? We may forgive the authors for not addressing some of these questions. Remarkably, they appear reluctant even to raise them. Thus, the authors often come across as apologists for these reforms rather than as independent and intellectually inquisitive analysts. This limits the book's potential both as an enlightening text for students of public management and as a scholarly contribution.

Boston and his colleagues give little attention to the deliberate efforts at agenda setting that prepared the ground for introducing and developing the ideas of public management embodied in the New Zealand model. Moreover, the approaches that were most influential among policymakers as the model emerged (public choice theory, agency theory, transaction cost economics, and the grab-bag of buzzwords and fads going under the heading of "managerialism") are reviewed and critiqued in a surprisingly undigested fashion. Most interested readers will have at least a passing knowledge of the essential features of these approaches. Matters deserving more attention concern why and how these particular ideas were successfully forged into policy prescriptions that rapidly came to dominate (and subsequently transform) the local policy discourse in New Zealand.

A discussion of how the policy community came to fixate on some ideas and disregard others would provide important clues as to whether the mutually reinforcing nature of the components of the model emerged by design or by sheer luck. Knowing about this process would also help us assess the likelihood that this model, or parts of it, could be successfully transplanted to other jurisdictions. But on this matter, the authors equivocate. "Assessing the influence of particular ideas, assumptions, theories, and models on a series of policy changes poses many difficulties. In any given policy setting there are usually numerous forces at work, not to mention a range of political, institutional, and technical constraints. In most situations, therefore, it is impossible to ascertain with any precision the impact of specific ideas, theoretical insights, or schools of thought on the process of policy development and implementation" (pp. 26–7). Sure, understanding the factors shaping social and political choices is difficult. That is why we do social science. The authors obviously have some knowledge about the war of position and the war of maneuver that occurred among policy advisors during the reform period. If they truly believe the conditions under which ideas and models gain influence are opaque, then I doubt that they would have taken the time to delineate so carefully the New Zealand model for readers elsewhere.

The title of *Social Policy in Aotearoa/New Zealand: A Critical Introduction*, may strike readers as unusual. "Aotearoa" is the Maori word for New Zealand (*literal meaning*: land of the long white cloud; *alternate meaning*: land of the wrong white crowd). In this book, the authors go to some length to argue that sound policy analysis starts with an examination of the broader social setting and a recognition that policy design involves trade-offs and rules that inevitably privilege some groups and disadvantage others. Cheyne and her colleagues seek to provide readers with the conceptual and critical tools necessary for understanding the forces that shape policy development. They also seek to promote broader public discussion of policy directions. These intentions alone are quite telling about the nature of the policy debate that has taken place in New Zealand since the mid-1980s. In their quest for reform, policy advisors and politicians have relied most heavily upon stripped-down economic models of human behavior to guide their thinking. Often, questions concerning how reforms will intermesh with those aspects of New Zealand life that are not amenable to inclusion in such models have been ignored. Nonetheless, these aspects—divergent conceptions of property rights, variegated understandings of appropriate behavior, and the importance that different groups place on nonmarket interactions and voluntarism—represent vital strands within the country's broader social fabric and affect significantly the well-being of individual citizens. In chapters on, among other things, feminist and antiracist conceptions of social policy, individualism, collectivism, and recognition of Maori self-determination, Cheyne and her colleagues lay the groundwork for developing a thorough critique of current policy settings in New Zealand.

Advocates of the New Zealand policy reforms have often, quite correctly, argued that trade and fiscal imbalances must be rectified for a sustainable position to obtain. Cheyne and her colleagues call for a rethinking of the various policy reforms that have affected income support, social services, and health care. To their credit, however, the authors acknowledge that the interactions between domestic policy settings and international economic conditions cannot be ignored. Thus, the essence of their argument is that policymakers should become alert to a broader range of conditions and considerations when engaging in policy development. Creative, nuanced thought, rather than slavish adherence to

received ideas or an obsessive focus on fiscal concerns, is what is most needed if policymakers are truly to advance general well-being.

These two books highlight New Zealand as a rich site for investigating many vital, contemporary questions regarding shifting configurations of politics, economics, government, and society. Aside from providing important discussions of distinct facets of public policy in New Zealand, both books offer extensive bibliographies. (The guides to further reading provided by Cheyne and her colleagues are particularly good.) Beyond the information contained in these books, readers interested in learning more about recent developments in New Zealand should consult two further sources: the summer 1997 issue of the *Journal of Policy Analysis and Management*, which is devoted entirely to drawing lessons for other countries from New Zealand's public management reforms; and Lewis Evans, Arthur Grimes, Bryce Wilkinson, and David Teece, "Economic Reform in New Zealand 1984–95: The Pursuit of Efficiency," *Journal of Economic Literature*, December 1996.

New States, New Politics: Building the Post-Soviet Nations. Edited by Ian Bremmer and Ray Taras. Cambridge: Cambridge University Press, 1997. 743p. $74.95 cloth, $29.95 paper.

Paul Kubicek, *Koç University*

New States, New Politics is the successor to the editors' highly acclaimed *Nations and Politics in the Soviet Successor States* (1993). Many of the same distinguished contributors have been invited to share their assessments of the progress and shortcomings of nation- and state-building processes throughout the republics and regions of the former USSR. Their eighteen informative case studies, covering all fifteen states as well as specific regions within the Russian Federation, are bookended by essays by the editors that attempt to provide a framework by which to understand the development, evolution, and potential staying power of nationalism in the region.

While there is an almost inevitable resemblance to the previous work, there is much in *New States, New Politics* that is, in fact, new. All of the case studies have been updated to cover important events through 1994, and newly added chronologies at the end of each provide a handy additional resource for the reader. More significantly, however, the focus has changed with the course of events. Whereas the earlier volume largely chronicled the rise and eventual triumph of national emancipatory movements, this work's aim is to highlight problems of state-building and states' adjustment to independence. A central concern in all states, however, has been the effort to (re)define the nation—both in terms of itself and in relation to others—and to construct identities appropriate for the new states. In other words, the task now in many states is to remold and expand ethnically based conceptions of identity such as *russkii* or *kazak* into something based upon fealty to the state—a *rossiian* or a *kazakistanli*.

This is obviously not an easy task, one made all the more difficult by arduous economic and social conditions in all states. Nonetheless, its importance is easily demonstrated by the conspicuous examples of failure: civil wars in Chechnya, Georgia, Azerbaijan, and Moldova. Elsewhere there has been less violence between different ethnic and national groups, but tensions over questions of national identity and rights remain virtually omnipresent. Understanding the politics of nationalism and ethnicity therefore remains important to the student of post-Soviet politics. Bremmer even suggests that an approach focusing on nationalism and national movements from below—as opposed to the classic state-centric focus—will capture more of the post-Soviet reality. He develops a multilayered typology of national movements and suggests that different groups will pursue policies of integration, domination, competition, or collusion depending upon their relative position.

The primary contribution of this volume, however, is in the case studies. In an average of twenty-five pages, each provides a history of national development, reviews the events leading to national independence, and highlights the main challenges confronting each of the new states or regions. Due to their relative brevity and their clarity, as well as the comprehensive coverage of the entire work, the cases will no doubt be a most useful guide for any student of post-Soviet politics.

As one might expect in an edited volume, however, there is little systematic effort to compare the experiences of different states, but the reader will quickly discover that there has not been one road to independence or one post-Soviet trajectory. Despite certain common traits, it is the differences among the Baltic, Slavic, Caucasian, and Central Asian states that stand out most clearly. This fact poses a real problem for those who want to develop schema or parsimonious theories to explain the dynamics of nationalism in post-Soviet states. Taras's effort to use Boolean analysis shows the difficulty of finding either necessary or sufficient conditions. The answer to the when, where, how, and why of nationalism seems to be, in short, that it depends. Bremmer's typology of interaction among various groups does capture some aspects of competition among groups, but clearly whether they compete or cooperate depends, again, upon a myriad of conditions. In some cases, one even sees that the push of realpolitik has superceded nationalism and now compels many states to enter into closer alliance with the center. Thus, we see Belarus, Kyrgyzstan, and Kazakhstan joining a customs union with Russia and Caucasian states seeking Russian assistance in ending the violence that torments the region.

This points to a larger problem with the work as a whole. The assumption seems to be that national identity and ethnic politics are ascendant and veritable trumps over all other issues. This is clearly not the case. In Lithuania, Ukraine, Moldova, and Georgia, the initial post-Soviet "nationalist" leaderships have been replaced by those adopting a more measured and pragmatic approach. In Central Asia there has been very little ethnic mobilization or "nationalism from below," as mentioned by Bremmer, and as a result the case studies tend to focus on state leaders and their efforts to consolidate their rule. Economics, the primary concern of post-Soviet publics, is disappointingly given spotty coverage in the text. For example, Kravchuk is lauded for his "successful" leadership in Ukraine, despite the fact that he presided over a precipitous economic collapse and by 1994 could run for reelection only a vacuous platform of "*Derzhavnist*" (statehood). In an even more puzzling oversight, the Communists' 1992 return to power in Lithuania is little more than mentioned. Both of these events, as well as numerous others, suggest that nationalist fervor may in fact be fading and that postindependence politics will not be governed exclusively or even primarily by issues of identity.

Is nationalism still ascendant? Should this in fact be the basis of a new paradigm? While no one can deny that nationalism and ethnic politics do matter and will remain a factor in post-Soviet politics, one may well wonder that as independence becomes both a memory and an accepted fact

what "normal politics" in these states will look like. Nationalist slogans may provide little practical guidance on such weighty questions as the role of government, how to resuscitate the economy, and where each state fits in the world community. In the case of Russia, we saw in 1996 that primitive nationalist appeals (as in the case of Zhirinovsky) did not capture the imagination of the electorate. Yes, nationalist discourse was pervasive, but it fared better when combined with calls for greater economic security (Zyuganov) or law and order and cleaner politics (Lebed). In short, issues of national identity do constitute an important political cleavage in post-Soviet states, but others (class, age, region) are also crucial. Future volumes of this genre, therefore, may have to expand their focus if they want to document developments in post-Soviet states.

The Price of Wealth: Economies and Institutions in the Middle East. By Kiren Aziz Chaudhry. Ithaca, NY: Cornell University Press, 1997. 330p. $45.00 cloth, $19.95 paper.

Manochehr Dorraj, *Texas Christian University*

Amid a sea of descriptive and redundant accounts of Middle Eastern politics by instant "experts" and journalists that has appeared in the last two decades, Kiren Aziz Chaudhry's book is a life raft. Using a political economy approach, the author sets out to explain the institutional changes in Saudi Arabia and Yemen in the context of international and regional economies. Having analyzed the utility of international, institutional, and interest-based explanations of change in the comprehension of regional economies, she focuses on the central issues of economic development. Chaudhry delineates the rise of unified markets, the emergence of centralized bureaucracies, the role of petro-dollars and labor, and formal and informal banking in molding government-business relations and the overall development of these two political economies.

The author discerns three distinct formative periods in the economic and political development of Yemen and Saudi Arabia since 1970: the oil boom (1973–83), the recession (1984–1990), and the aftermath of the Gulf War (1991–97). The oil boom brought a huge influx of petro-dollars, which modified the regional economy fundamentally. The boom also functioned to further integrate the region into the global economy. During the boom years, the emergence of ideological politics in both countries was forestalled. The period of recession, when oil prices plummeted and budget constraints reduced state options, forced one to retrench (Saudi Arabia) and the other to grant concessions (Yemen). The Gulf War put an end to the regional economy, a process well underway since the 1980s, and magnified regional economic tensions, such as between the rich and the poor nations. Chaudhry also explains how the institutions built during the boom broke down during the bust.

Chaudhry's contribution is theoretically and empirically rich. It pays a great deal of attention to details, and the author's assertions and analysis are substantiated by abundant data and information. The two case studies are well chosen and enable useful comparisons and contrasts. Saudi Arabia is a rich economy based on petro-dollars, and its fortunes fluctuate with the price of oil. Yemen is a poor economy in which remittances from its out-migrant labor force and foreign aid received largely from the oil-producing states—Saudi Arabia in particular—play a large role in economic development.

The particular structure of the Saudi economy—single-commodity production, heavy reliance on oil, a negligible goods-producing sector, and a small market and labor force—has led policymakers to infuse funds into the economy through expenditures on welfare programs, massive military spending, subsidies to the private sector, and a concentration on infrastructural development. Due to the weaknesses of the private sector and its primary involvement in nonproductive economic activities, government subsidies play a large role in supporting the farming and manufacturing industries, thus keeping the economy afloat. The oil boom of the 1970s and early 1980s followed by the oil bust of the late 1980s (Saudi oil revenues plummeted from $120 billion in 1981 to $17 billion in 1985) forced the government to implement an austerity program and cut back on subsidies for the private sector, policies that the private sector resisted and overturned. But due to its vast foreign reserves, the Saudi government managed to withstand the pressures of lower revenues and continued its high level of military and infrastructure expenditures.

Yemen's economy also was in crisis during the oil bust. Labor remittances dropped 60% in 1986. Foreign aid fell from more than 90% of the current budget to 2%. The government response was heavy taxation, regulation of the private sector, and reform of foreign trade. In Yemen, the weaknesses of central government have strengthened the tribal groups, who use their position of relative autonomy to bargain with the state. The Yemeni regime also initiated political liberalization, whereas the Saudi regime, facing an increasingly militant opposition by the 1990s, has retrenched and suppressed its opponents. The divergent responses to popular demands for participation may be explained in light of the fact that oil revenues had accrued directly to the Saudi government during the boom, releasing the state from reliance on domestic sources of revenue, whereas the privately controlled labor remittances bypassed the Yemeni state. Thus, not possessing the financial assets of the Saudi government, Yemen had to grant concessions and open the political system to wider participation.

While Chaudhry's economic analysis at times can be dry and is not always accompanied by corroborating political observations and conclusions, this volume offers more than enough to compensate for this minor weakness. A major strength of the book is that, by focusing on the nuances of institutional change and their outcomes, it goes beyond the traditional focus on political leaders and their personalities and policies. Chaudhry probes deeper than most, delineating the structural, national, and international factors responsible for changes in regional political economies. While she finds "a modified class analysis" useful in explaining the process of state formation and development of national markets, she is also aware of limitations of class analysis in understanding the internatinalization of the two economies during the boom years. Her analysis is also sensitive to the specific realities of Saudi Arabia and Yemen, which are linked to their respective historical, cultural, and social development. This attention to specificity, however, does not detract from the breadth of generalizations and theorizing in this volume. In fact, in many ways, the author's analysis and observations go beyond her two case studies and provide insights that are helpful in understanding the complexities of Middle Eastern as well as Third World political economies. This leaves the reader wondering why, with the wealth of information and data presented in this volume, the author did not expand her research into a comparative study encompassing more countries. It is hoped that this will be a future project. Given the high quality of the present contribution, such an undertaking would be welcomed by students and scholars of the Middle East and Third World development alike.

Libya's Qaddafi: The Politics of Contradiction. By Mansour O. El-Kikhia. Gainesville: University Press of Florida, 1997. 213p. $49.95.

Dirk Vandewalle, *Dartmouth College*

During the last decade, the literature on the rentier—or allocative or distributive—state has produced a growing number of works investigating the effect of commodity booms on the political and economic fortunes of numerous developing countries. Because of the extraordinary size and effect of hydrocarbons in several of those countries, oil exporters in particular have been the subject of intense—albeit often somewhat inconsistent and largely descriptive—studies. Two recent works (Terry Lynn Karl, *The Paradox of Plenty*, 1997, and Kiren Aziz Chaudhry, *The Price of Wealth*, 1997) synthesize many of those earlier findings in an interpretive and heuristic fashion that insightfully reveals how a combination of history, rapid inflows of revenues, and institutional development (or lack thereof) produces uniquely structured "petro-states."

Arguably, no single country in the Middle East and North Africa has suffered more under the deleterious effect of that combination of factors than Libya. The legacy of a brutal colonial experience and the coincidence of state-building and sudden influx of exogenous capital saddled the country since its independence in 1951 with two rulers (Idris al-Sanusi and Muammar al-Qadhafi) whose often-voiced suspicions of the modern state, with its vertically integrated bureaucratic structures, exacerbated the tendency prevalent in most rentier states to use revenues in ways that are politically expedient but, most often, economically ruinous.

In *Libya's Qaddafi*, Mansour El-Kikhia aims to explain how precisely Libya's current ruler has accomplished the latter through what he defines as "the politics of contradiction" (p. 5): the creation of political turbulence and continual chaos that allows the regime to ensure its survival while effectively shielding itself from the actual ongoing turmoil (and, although less explicitly stated, by engaging in large-scale regime-sponsored patronage that relegates most Libyans to the political and economic sidelines). Although a short introductory chapter summarizes the basic argument, the book unfortunately contains no abstract framework that clearly sets forth what the author hopes to investigate or achieve beyond chronicling the turbulence of Libyan politics, or one that would allow the reader to compare implicitly its alleged uniqueness against examples of political and economic development in other rentier states. For readers interested in such questions, Lisa Anderson's now decade-old account (*The State and Social Transformation in Tunisia and Libya 1830–1980*, 1986) remains the most insightful one available.

The strength of El-Kikhia's account is that it provides a thorough, albeit descriptive and at times even speculative, rendering of Libya's recent and current political and economic history. Such is the paucity of serious works on Libya that this achievement alone should not be underestimated. But for social scientists who have closely observed Libya, this book will raise a number of serious questions regarding its evidence and interpretation thereof. In light of the fact that research in Libya is virtually impossible, it is perhaps hard to fault the author for relying so extensively on second-hand sources, even though some of those have become quite suspect and outdated by now, and even though first-hand written documentation is more abundant and available than the author claims.

Evidence, however, informs interpretation, and in this particular case it leads to a number of assertions this reviewer would dispute. Perhaps one example suffices to make the point. El-Kikhia argues that Qadhafi has constructed a powerful neopatrimonial society (my words) in Libya. In language that recalls a by now traditional interpretation in Middle East studies, he asserts that the Libyan leader has done so by increasingly gathering around himself tribal support in order to stay in power in a divide-and-rule fashion. A good part of the book, and its appendix entirely, is taken up therefore in examining and documenting the growing role of the Qadhadhfa tribe in Libyan politics as evidence of this neopatrimonial society. This, however, represents an incomplete explanation that obfuscates simultaneously the process, structure, and evolution of Libyan politics and society since 1969.

In reality, and although Qadhadhfa members have become more prominent, the country's informal power structure, which El-Kikhia correctly identifies as the locus of real power (pp. 5–6), is permeated by scores of non-Qadhadhfa citizens whom the Libyan leader has deliberately cultivated as a means of offsetting the potential power of any institutionalized group, including his own tribe. In addition, El-Kikhia's analysis ignores a crucial development that has been heightened since 1969: the way in which Libyan society, due in part to policies fostered by Qadhafi but also because of the intense individual rent-seeking that has taken place, has become much more differentiated than a purely tribal scenario accounts or allows for. The notion that Libyan society is increasingly reverting back to tribal politics (p. 103), and the apocalyptic vision that pits tribes against one another in a struggle for survival in a post-Qadhafi situation, is therefore highly problematic. It ignores the profound reality that tribes in what eventually became Libya have never, not even during the disastrous colonial encounter, served as a sustained and coherent focus for opposition, and they show no real signs of doing so now.

Undoubtedly, this book will appeal to many readers who, for a number of reasons, share the author's interpretations and his conviction that Libya resembles nothing less than a predatory state that is unfortunately (mis)guided by a ruler whose actions often have been, both nationally and internationally, "beyond the realm of the rational" (p. xiv). While *Libya's Qaddafi* is a welcome addition to the literature, primarily for some of the insights it contains regarding the minutiae of politics in the country under Qadhafi, it should be read with a good deal of caution. It is a courageous work by an individual whose family has suffered considerably at the hands of the Qadhafi regime, but it is not the "completely evenhanded" account its dustcover touts. It also does not, despite the valuable information it contains, in the end provide the kind of comprehensive and more structural analysis that would allow an academic audience to better understand the author's central theses about the continuity in Libyan politics, or how Qadhafi's "politics of contradition" flow logically from and reinforce that continuity.

The Rise of Christian Democracy in Europe. By Stathis N. Kalyvas. Ithaca and London: Cornell University Press, 1996. 300p. $45.00 cloth, $19.95 paper.

Kees van Kersbergen, *University of Nijmegen*

Christian democracy has been one of Western Europe's most successful political movements, yet it has long been neglected in comparative politics. This book is a welcome and innovative contribution to the field. It analyzes the origins of confessional parties in Belgium, the Netherlands, Austria, Germany, and Italy and explains why such a party was not successful in France. It not only provides a genuinely com-

parative account of the conditions under which these parties arose but also offers a theory of party formation that is potentially valuable in other contexts. A major strength of the book is its rare combination of an analytical framework rooted in rational choice theory and a detailed historical and comparative account of critical cases.

The study starts with an explanation of a peculiar fact that has puzzled the (scarce) literature, namely, that both the church and conservatives opposed the political mobilization of lay Catholics, but confessional parties nevertheless emerged. Kalyvas solves this puzzle in an elegant manner. He identifies the crucial actors, maps out their preferences, and develops a strategic model of party formation that explains the puzzling result. His thesis is that confessional party formation was the contingent and unintended outcome of the strategic moves made by the Catholic Church and conservative politicians in response to the liberal anticlericalism of the late nineteenth century. The author convincingly shows that the rational calculus of costs and benefits by the church and the conservatives in this "game" led them both to oppose the formation of confessional parties.

In the light of the liberal assault, the church preferred to follow a strategy of compromise because the creation of mass organizations of lay Catholics would produce prohibitively large negative externalities. The church traditionally controlled its members through hierarchy and centralization. But mass mobilization would break this unity and subvert hierarchy by allowing the lower clergy and lay Catholics to become independent of the episcopate. The problem, of course, was that the liberals were unwilling to compromise and instead intensified their attack on the church's power in society. Given the ineffectiveness of the numerous attempts at a compromise, the church was eventually forced to choose the organizational strategy for its defense and to participate in the political process. But even political participation of lay Catholics involved substantial costs, such as a further weakening of the bond between the church and its members, a loss of control, and a possible backlash against Catholicism as an effect of politicizing religion.

The conservatives' preference was to continue their participation in the political game and to find a way to call a halt to the liberal advance. The conservatives, however, lacked the resources and the salient issues necessary for mass mobilization. Moreover, mobilization also appeared too costly for them. The liberal attack against the church itself created the opportunity for avoiding these costs, however. Religion potentially was an electoral asset, and the organizational powers of the church could be used as a political resource. Neither the church nor the conservatives, however, favored the formation of Catholic parties. The conservatives feared that the permanent politicization of religion and the association with the church would restrict their autonomy. The church feared the confessional party because it shifted the source of power of activists from the church to the voters. Moreover, it diluted the hierarchical relation between church and activists and therefore threatened to reduce the influence of the church in society.

The author shows how the interacting strategies of the two main actors opposed to the formation of confessional parties paradoxically produced the unwanted outcome. First, the church took centralized control over the existing yet loosely organized associations of lay Catholics and thus created the Catholic social movement. Next, provoked by increasing anticlericalism, the movement rapidly politicized, especially when the church decided to use its organizational capacity in support of those conservative politicians who agreed to defend the church's interests. This participation strategy unintendedly produced two new actors (lay Catholic activists and politically active priests) as well as a new identity (political Catholicism). Finally, the prochurch coalitions unexpectedly turned out to be electorally successful. As a result, political Catholicism gained a momentum that went beyond the control of the church. Electoral success provided the mechanism of transition to the formation of confessional parties. The author shows that these sequences constituted the general pattern in five nations but not in France. Contrary to conventional explanations that stress the decline of religiosity, Kalyvas argues that it was a series of electoral defeats of the prochurch coalitions that prevented the formation of a confessional party in this country.

A second major argument of this study is that the formation of confessional parties reinforced a distinctive Catholic political identity which not only reinterpreted Catholicism in much less doctrinal terms but also started to challenge the religious primacy of the church in political matters. As a result, political Catholicism became separated from the church and religion. "Thus, in a paradoxical way, the politicization of religion contributed to the secularization of politics" (p. 245).

The analyses are generally convincing, and there is only one major quibble. The author does not explain why only the church and the conservatives are included in the model as actors with preferences and strategies and why the lower clergy and the lay Catholic activists are treated as merely by-products of the strategic actions of the main actors. One reason the church was and remained so vehemently opposed to the formation of confessional parties is, in my view, that lay Catholics and priests were already mobilizing politically long before the church decided to follow the participation strategy. The conservatives and the church reacted not only to the liberals but also to the grassroot activism of Catholics themselves. This activism not only was prompted by anticlericalism but also was a response to the effects of industrialization, a topic largely neglected in this study. Social Catholicism now wrongly appears to have been created by the church, while in reality the presence of social Catholics changed the very calculus of the church and the conservatives. This otherwise fascinating study tends to shed insufficient light on the crucial effect of a third actor. The model seems therefore incomplete because it ironically excludes the only actor in favor of the formation of a confessional party.

The Radical Right in Western Europe: A Comparative Analysis. By Herbert Kitschelt. Ann Arbor: University of Michigan Press, 1995. 332p. $49.50.

Ekkart Zimmermann, *Dresden University of Technology*

Kitschelt deals with three variants of the radical right in seven countries: France with the National Front as the "prototype of the New Radical Right"; Austria and Italy ("blending new right appeals into a broad populist antiestablishment strategy"); Germany, Norway, and Denmark, with a "milder version of the new radical right"; and Britain, with the National Front as a failing case. The theoretical framework is contained in two theoretical chapters dealing with alternative explanations as well, and it is taken up again in two final chapters, with the country studies in between. Dealing with so few cases in so many variations and (almost by necessity) following a sampling procedure on the dependent variable may easily create problems of design, theoretical explanation, and empirical inference. These do not fade away when taking Kitschelt's earlier book (*The Transformation of European Social Democracy*, 1994) as a companion volume that uses the

same theoretical framework with a different dependent variable. If Kitschelt were to succeed, then the case could be made for powerful and parsimonious theorizing. Does he?

Kitschelt argues that the rise and the success of new radical right parties mostly after 1965 cannot be understood by a demand perspective alone (a shift in voter preferences) but must be complemented by the more important supply aspect (logic of party competition, internal organization patterns, and ideological traditions). Kitschelt senses a shift of voter preferences from the traditional Left-Right axis to an ideological cleavage dividing left-libertarians from right-authoritarians. How do the established parties react to this?

This shift in political issue preferences has occurred only in advanced postindustrial capitalist societies, the first condition for a new radical right party to emerge. The gate-keeping function is provided by the second condition, a convergence of the moderate leftist and rightist parties in political issues and more so in a government coalition. Just as on the left, new oppositional space will then be opened at the right end of the political continuum. Whether a new radical right party will form and have electoral success *further* depends on organizational skills, leadership resources, media attention, and successful competition in "secondary elections." The third major condition in the political opportunity structure emerges in the presence of a patronage-based party system and political economy. Here the populist antistatist radical right scores its biggest successes with a broad coalition of protest voters (Northern League in Italy and the Austrian Freedom Party). Yet, if the third and second mobilizing condition break down, then these rightist protest parties are doomed. This is not so, Kitschelt argues, when a *rightist authoritarian* party (the master case of the new radical right) "combines economic promarket and political authoritarian and xenophobic messages" (p. 42), a mixture that may be less contested by the major parties. Those parties have an ideological niche but also fewer adherents than populist-antistatist parties.

In the third case, that of welfare chauvinist parties, emphasizing the issue of ethnic homogeneity in the defense of the welfare state, a similar logic applies. Furthermore, there are contradictory effects of widespread higher education in advanced European welfare states, making voters more resistant to simple formulas of both, cutting massively into the welfare state, or putting the blame on foreigners as to undermining the welfare state (p. 23). The negative effect on support for welfare chauvinist parties is to predominate among the public at large.

There is a fourth variant of the radical right, the neofascist type, with which Kitschelt deals in passing. Due to historical burdens and changed politicoeconomic circumstances, parties like the NPD in Germany and the MSI in Italy end up in failure. He adds a brief assessment of the conditions of fascism and national socialism in the interwar period (pp. 28–42), which oversimplifies a more complicated set of conditions and focuses on structural conditions only.

Kitschelt largely relies on survey data (mostly the *World Values Survey*, 1990) that sometimes are a bit dated. The number of respondents with sympathies for radical parties often is limited in those data. Even more problematic is the extensive reliance on factor analytic methods (including confirmatory factor analysis). The amount of data trimming (in the hands of the collaborator Anthony J. McGann) meets with reservation when, for example, reading that the varimax rotation "does *not* allow for a simple two-dimensional representation of the results" (p. 88, emphasis added). Yet, the trimming goes on to meet the theoretically outlined dimensions in the data structure. Also, in many instances (p. 65 on the economic growth ranking for the French National Front or p. 74 with respect to populist antistatism) the distinctions between the variants of the new radical right do not show up as precisely and consistently as one would wish.

Chapter 8 links the comprehensiveness of the welfare state with left-libertarian versus right-authoritarian opinion cleavages in the electorate and the convergence of the established moderate Left and Right parties, as well as the political salience of race and immigration, to predict either stability of the party system or change toward left-libertarian or right-authoritarian parties. Noteworthy is the discussion of under which conditions the welfare state was founded and extended and how this may change through ethnic pluralization. Arguing against public bads, namely, having more crime and xenophobia, Kitschelt defends the welfare state and ethnic pluralization against the new right. Some arguments (referred to) here are naive, for example, concerning the relationships between highly developed and less-developed states under global economic conditions (e.g., as to limiting the mobility of people between highly developed states, p. 273).

The latter point brings up the issue of a classical misnomer, that of "left-libertarianism." People subsumed under this syndrome share beliefs in participatory and individualist cultural values and a privileged existence in the welfare state. Yet, how about a liberal economy? Without becoming polemical here, you may call them left-libertarians if you want, but it should be added for clarity that these so-called left-libertarians may be antilibertarians when it comes to the *economy*.

This leads back to the (assumed or proven?) rectangularity between the two basic dimensions of socialist versus capitalist economics and libertarian versus authoritarian politics. Kitschelt would have little trouble in comparing East Asian states with Western Europe along those dimensions. Within Western Europe, however, such a fundamental two-dimensional space and the shifts therein need much further corroboration.

Furthermore, Kitschelt argues that the winning formula of the new radical right consists in the mixture of "economic free market appeals that are combined with authoritarian and ethnocentric and even racist messages" (p. viii), and he lists among the groups *over*represented here blue-collar workers and small business, such as craftspeople, shopkeepers, and farmers. Otherwise, social background is said to have little effect on voting for new radical right parties, after controlling for political attitudes. A brief look at earlier waves of the radical right in the 1960s, 1950s, and the interwar period would have revealed that the antistatist ideology may only be a first step, followed by protection for one's own turf, in particular under the conditions of increased (global) competition (on p. 19 this argument seems to be shared). Whatever party platforms may say, the relevancy of such arguments cannot be totally denied. Moreover, theoretical explanations such as status politics, status frustrations, and status inconsistency, to name only a few in a series of previous efforts (by, e.g., Lipset, Hofstadter, Bell, Parsons, Geiger, Ranulf) to come to terms with the radical right, are missing in the perspective of Kitschelt, which is dominated by the supply-side logic.

In terms of design, Kitschelt's approach calls for a full party-to-party comparison *everywhere* along the political spectrum to get at the dynamics of changes in issue preferences and protest voting as well as to avoid the sampling on the dependent variable. The quality of data needed for a truly causal comparative assessment must go much beyond that presently available.

The electoral landslide of Blair in Britain may again be a deviating (British) case. Yet, to the Continent it may signify a different future evolution of the party system than envisioned by Kitschelt. The competition of major parties for the center continues, perhaps with less entrenching in old ideological patterns, but in successfully copying elements from other parties that work well while simultaneously avoiding their and one's own (past) mistakes in electoral strategy. Such a strategy may leave the end positions of the Right (and Left) open or may drive it into marginality with an increasing overrepresentation of unskilled workers.

Currently we lack an independent test case of the theory presented. It remains to be seen whether Kitschelt's theory is more than a momentary depiction. One also would like to know to what degree interactive, additive, or substitutive relationships prevail among the essentially three critical variables—convergence of the dominant parties, political patronage, and economic crisis. Either in perception or in reality, the latter variable underlies any of the demand shifts to the radical right. The basic theoretical elements Kitschelt uses may be convincing, less so the labeling of the political space and other issues of methodology.

This study is a milestone. It is guided by theory, with an emphasis on macro phenomena, and has potential applicability to a broad range of phenomena. With repeated evidence, Kitschelt dismisses other more simple rival hypotheses, making claims for, for example, single-issue protests (taxes and immigration), disaffection with elite democracy, right-libertarianism and neofascist countermobilization, as well as for a counterreaction against left-libertarianism and effects of electoral laws. The *Radical Right in Western Europe* is not the definite word on either the new radical right or the emergence of party systems. Combining knowledge about the various countries with broad analytical goals, it remains an intellectual achievement. Even where wrong (or possibly wrong in the future), it is a source of learning for all—scholars, interested general readers, and politicians alike.

Japan's Postwar Party Politics. By Masaru Kohno. Princeton, NJ: Princeton University Press, 1997. 172p. $55.00 cloth, $17.95 paper.

The Japanese Election System: Three Analytical Perspectives. By Junichiro Wada. New York: Routledge, 1996. 112p. $60.00.

William W. Grimes, *Boston University*

In 1993, the Liberal Democratic Party (LDP) lost its parliamentary majority in Japan and entered the opposition for the first time since its formation in 1955; the following year, a non-LDP coalition government passed a major reform of the Lower House election system. At the core of these momentous events was a belief that the existing electoral system had led to a variety of unfortunate effects in Japanese politics, which a new system might reduce or eliminate. The present review considers two new books that seek to advance understanding of just what those effects were and why they appeared.

The Japanese Election System addresses the 1994 electoral reform from a rational choice perspective. This focused, extremely short book (only 54 pages of text) began its life as the author's economics Ph.D. dissertation, and it appears not to have undergone serious revision in its journey into print. The "three analytical perspectives" of the title are a game-theoretic analysis and application of Duverger's Law, a rational coalition analysis of the internal politics of the LDP, and a model of the economic effects of pork-barrel politics. These three substantive chapters are introduced by a concise history of the Japanese electoral system from the Meiji period (1868–1911) until 1994.

Wada seeks to demonstrate that (1) the old system of multimember electoral districts with a single, nontransferable vote was superior to the new system (a 3:2 mixture of single-member districts and regional proportional representation) and (2) the consistently unbalanced apportionment of Lower House seats in favor of rural districts since the 1950s has been a major drawback economically as well as politically. (Some Japanese writers see unequal apportionment as offering the benefit of automatic redistribution to the economically declining rural areas.)

The book offers some interesting insights. The analysis of Duverger's Law focuses on candidates' choices of whether to run or to exit rather than on citizens' decisions on how to vote. Based on a three-party model with varying voter preferences in a single-member district, Wada demonstrates that a game of Chicken is likely to develop between opposition parties, and thus their candidates will run even when it would be rational for one to drop out in favor of an ideologically similar opponent. Unfortunately, while the analysis does have some explanatory power in a static situation, it ignores some relevant longer term considerations, such as the desire to position oneself as the main challenger in the next election.

The strongest analysis is that of the LDP as a coalition government. Wada addresses an interesting puzzle in Japanese politics—why factional representation in LDP cabinets went from a "mainstream" versus "anti-mainstream" spoils system to one of proportionality to Lower House strength. His model shows that, as opposition parties moved ideologically closer to the LDP, antimainstream LDP factions had a more credible threat of breaking away from the party to form a government with them. In order to maintain the loyalty of factions that lost out in the contest for leadership, proportional representation in cabinets came to be a recognized norm within the LDP. Wada's analysis includes simulations of bargaining sets and cabinet representation that match well with actual post-1983 LDP cabinets.

While it brings mainstream American public choice theory to the study of Japanese electoral politics, *The Japanese Election System* is unlikely to have a strong influence on either field. From the standpoint of the study of Japanese politics, none of Wada's results appear very exceptional. In terms of theory, his models may be of some interest but appear to offer more in the way of modification than innovation.

Japan's Postwar Party Politics, in contrast, is likely to become required reading in the field of Japanese politics. I suspect that it will also earn a respected place in comparative politics and comparative electoral studies. The book is a concise and well-written rational-choice analysis of several key events and evolutions in Japan's electoral and party systems. These include early postwar electoral reforms, pre-1955 coalition building, the creation of the LDP in 1955, the evolution of LDP factions and party norms from 1955 to the 1980s, the evolution of the overall party system in the same period, and the political upheaval of 1993. What is most notable is the way in which Kohno applies theory to detailed historical accounts; unlike many game-theoretic treatments, this book cannot be accused of being "ahistorical."

Kohno criticizes traditional explanations (which he categorizes as "political-cultural," "historical," and "socio-ideological") on both logical and empirical grounds, making a strong case in each of his chapters that they have been overextended in both directions. In particular, he is bothered by their lack

of attention to independent political actors: "These approaches all entail overly deterministic explanations for political outcomes: to the extent that Japanese political actors are the 'prisoners of the underlying structure,' no matter what power, incentives, and strategies each relevant political actor has, the Japanese political systems and processes would have evolved the way they actually did" (p. 29).

The book is at its best in analyzing coalition building in the early postwar years, including electoral reforms in 1945 and 1947, the series of coalition governments before 1955, and the LDP merger itself. In each case, a methodical application of bargaining theory (with an assumption of a unidimensional policy space) gives order to tangled historical processes and provides answers to niggling questions and contradictions that have long been ignored.

To be more concrete, let us consider Kohno's analysis of the 1947 formation of the Katayama cabinet (a Socialist-led coalition of Socialists, Democrats, and several smaller parties). Most studies point either to the existence of a "New Deal consensus" or to the Socialists' plurality in the Lower House as central factors. Kohno refutes the former notion by pointing out that in early negotiations for a grand coalition the Socialists acquiesced to many key demands of the conservative Liberal Party. As for the latter, he points out that no established norm gave the largest party automatic leadership of the government; moreover, the Socialists' plurality over both the Liberals and Democrats was quite small. Also, neither explanation accounts for such apparent anomalies as the length of negotiations, the Socialists' persistent courting of the Liberals, or the Liberals' willingness to support the election of Katayama for prime minister before a cabinet was formed but only two days after having opposed the Socialists' and Democrats' joint candidates for speaker and vice-speaker of the Lower House. In Kohno's interpretation, the Socialists courted the right-wing Liberals because they rightly feared a Liberal-Democratic coalition and thus preferred a grand coalition to the possibility of being left out of government. That fear also led them to propose voting for the prime minister before bargaining was completed, in order to ensure that they would remain in the game. The Democrats and the Liberals accepted that proposal because the former were unable to come up with an alternative candidate, while the latter could only retain the hope of breaking apart the Socialists and Democrats if they remained in the bargaining process. The resulting game might well have been indeterminate but for the *deus ex machina* of General MacArthur's public endorsement of Katayama's selection. That made Katayama the focal point for subsequent coalition bargaining, at which point the principle of maximizing power by minimizing surplus majority took over, and the Liberals were dropped from contention.

The chapters on the evolution of intra-LDP politics (originally published as "Rational Foundations for the Organization of the Liberal Democratic Party in Japan," *World Politics*, 44[1992]:369–97) and of the multiparty system are also solid, but they provide fewer counterintuitive insights. Both use Duverger's Law and the concept of "expected party [or faction] differential" to explain the numbers of actors (parties or factions) and the style of their interactions.

The final substantive chapter, on the realignment of 1993, is less authoritative (ironically, since it is presumably easier to determine the actual incentives and preferences of actors in recent rather than less recent cases). Kohno focuses on the breakup of the LDP and the formation of the non-LDP coalition. The microanalytical approach is consonant with the fact that most of the Diet members who left the LDP to form the Shinseito and Sakigake parties were relatively junior members of the Lower House, and thus they were the most politically vulnerable if accused of trying to block reform. Kohno also provides a credible explanation for the coalition negotiations. It would be interesting, however, to see what he would make of the LDP-Socialist-Sakigake coalition formed in the summer of 1994 (one doubts that Kohno predicted this strange union any better than any other observer), the results of the October 1996 elections under the new system, or the present tug-of-war within the LDP over whether to continue the existing quasi-coalition or to try to form a more formal conservative coalition.

Readers will find two main weaknesses in Kohno's work. The first is a tendency to be dismissive of more macroscopic approaches. In some cases that attitude is warranted, but often such approaches simply address different questions than Kohno's. The second is that much of his analysis remains speculative. In order to avoid the pitfalls of hindsight, Kohno's approach requires him to construct hypotheses about actors' contemporaneous perceptions and preferences that may or may not be accurate. Also, given the one-shot nature of the events analyzed, it is often difficult to distinguish those elements which are systematic from those which are merely coincidental. To me, his judgments generally appear sound, but there is surely room for other scholars to dispute them. One hopes that this book will become a focal point for a debate that matches clear questions with logical, historically supported answers as well as Kohno has.

The End of the Peasantry: The Rural Labor Movement in Northeast Brazil, 1961–1988. By Anthony W. Pereira. Pittsburgh, PA: University of Pittsburgh Press, 1997. 280p. $45.00 cloth, $19.95 paper.

Kurt Weyland, *Vanderbilt University*

Recent analyses of Latin American politics—even studies of popular sectors—have focused almost exclusively on urban areas. Anthony Pereira's fine book helps fill this gap through an in-depth case study of the workers' movement in the sugar zone of Pernambuco. This section of Brazil's poor Northeast is of particular interest because it has been the site of one of the most active labor movements in the country.

Pereira sketches the book's theoretical themes in the introduction and first chapter. Like many others, he criticizes the literature on democratic transitions for its "elitism and lack of historical perspective" (p. 8). The author combines state- and society-centered approaches (pp. xviii–xix) to explain the striking transformation of rural mobilization in Pernambuco: Whereas peasant leagues pressing for land redistribution predominated in the early 1960s, rural labor unions demanding wage raises—a much less controversial demand—became ever more active during Brazil's democratic transition. This change stemmed in part from agricultural modernization, which shrank the peasantry and gave rise to rural wage labor. Furthermore, the Brazilian state promoted rural labor unions, which were less radical than the earlier peasant leagues. These unions gained increasing independence from state control and turned more "militant" (p. 4) after 1979.

Chapter 2 reviews the emergence of militant peasant leagues and more moderate rural labor unions during the government of João Goulart (1961–64). Chapter 3 analyzes the agricultural modernization stimulated by the development policies of Brazil's military regime (1964–85). It documents the displacement of many peasants and the growing social differentiation in the countryside, especially the rise of wage workers and small farmers. Chapter 4 shows how the

corporatist strategies of the authoritarian regime provided significant resources to rural labor unions, which came to distribute welfare services and in this way attracted numerous members. The unions shook off state control during democratization, mobilizing growing support among rural workers and attaining considerable gains through strikes and bargaining with plantation and mill owners.

Complementing this analysis of macrostructural developments, the following chapters, which are based on intensive field research in the sugar zone during the late 1980s, investigate the activities of rural labor unions at the local and regional level. After examining unions' limited capacity for collective action, chapter 5 stresses the disproportionate attention that their leaders devote to the provision of welfare services and to wage issues. By contrast, leaders deemphasize land redistribution, which is difficult to achieve and benefits few constituents. Chapter 6 examines landlord violence and the state's failure to protect union leaders and rural workers. The resulting climate of fear constrains union activities. Chapter 7 documents rural unions' disappointment with Brazil's political parties, most of which rely on clientelism and lack a programmatic orientation. As a result, unions are unable to use the large number of their constituents for effectively advancing their interests in the electoral arena. Due to unions' other priorities and the multiple constraints they face, plans for land redistribution made little headway under the new democracy, as chapter 8 shows. After summarizing the book's main findings, chapter 9 stresses the challenges confronting rural unions in recent years, when their moderate attitude and the failure of land reform triggered the emergence of more militant competitors, such as the landless workers' movement.

Contrary to the heroic image painted in many analyses of trade unions and social movements, Pereira draws a refreshingly realistic picture of the opportunities and numerous constraints faced by Pernambuco's rural unions. Given that he studied a "best case," that is, one of the relatively successful instances of lower-class mobilization in Brazil, this general finding is especially noteworthy. Displaying a fine sensitivity to the multiple dilemmas confronting union leaders and members, chapters 4–8 are the strongest sections of the book. The author's insights on union culture, which are based on intensive participant observation and a survey of 50 union leaders, are particularly interesting.

Pereira also does an excellent job of analyzing the interface of local- and national-level structures and processes. In this way, he thoughtfully explores the effect of "big processes," such as democratization, on the lives of common people. Chapter 7, for instance, provides a new perspective on the much-discussed weaknesses of most Brazilian parties by thoroughly examining the difficulties that union leaders and members face in using elections for making successful demands.

Another important contribution of the book is the emphasis on increasing social differentiation in the countryside. Peasants turned into wage workers, impoverished field hands, better-paid technicians, or small farmers, or they migrated to the cities. Pereira therefore correctly eschews dichotomous class categories (pp. 13–5, 44–8, 152, 167).

While the book's greatest strength lies in the author's rich observations, its contribution to political science theory is somewhat more limited. Pereira's criticism of the democratization literature (pp. 6–10, 154–5) echoes earlier arguments. Combining state- and society-centered theories (pp. xviii–xix, 154) as well as macrostructural and microanalytical perspectives (p. 80), the author does not frontally address important "paradigmatic" debates. He also could have discussed alternative arguments on his topic, for instance, by criticizing the more optimistic image painted in much of the social movements and labor literature.

Furthermore, the integration of different approaches leaves some questions unresolved. For instance, if the rise of rural labor unions stemmed from "the end of the peasantry," then these unions appear as faithfully representing their constituents. But if this change was due to the inclusionary corporatism of the military regime, then unions appear more as "filters" that pursue the interests of some constituents while neglecting others. Other arguments also are slightly ambiguous. For instance, after boldly proclaiming "the end of the peasantry," Pereira stresses the survival of a "peasantariat"—a combination of peasants and workers (pp. 60–1)—and the "durability of a peasant identity" (p. 65). Similar ambiguities appear in his characterization of unions' stance ("militant" on pp. 4, 75, 156 versus "pragmatic" or "incremental" on pp. 136, 161) as well as in his assessments of unions' degree of success (e.g., p. 156 versus p. 160) and of the effectiveness of state efforts to tame and coopt them (pp. 58–9, 75 versus pp. 80, 92–4, 100-101, 136, 162).

Despite these quibbles, Pereira's rich and sensitive analysis of union activities and the dilemmas their leaders and members face makes a fine contribution to the literature. The book is recommended to scholars interested in contemporary Brazilian politics and to specialists on labor unions and social movements in Latin America.

The Counter-Insurgent State: Guerrilla Warfare and State Building in the Twentieth Century. Edited by Paul B. Rich and Richard Stubbs. New York: St. Martin's Press, 1997. 235p. $59.95.

Marc V. Simon, *Bowling Green State University*

The central issue explored in this edited volume is the relationship between counterinsurgency wars and the evolution of the institutional state in developing countries during the latter half of the twentieth century. The editors understand that this relationship is only one of many factors important for explaining state development; it is not the sole determinant. The theoretical foundation for this volume comes from the structuralist literature, beginning with works like *Bringing the State Back In* (Peter Evans, Dietrich Rueschemeyer, and Theda Skocpol eds., 1985), but the insights which emerge are applicable to a wide range of theoretical work and models of revolution and government versus opposition conflicts. By omitting a concluding chapter, the editors missed a chance to expand on these linkages and clarify an appropriate agenda for future research. The cases selected show a variety of effects of internal wars on the state. Though the authors make few references to discussions in other chapters, most of the contributors focus on the central issues and propositions raised in the Introduction. Overall, as edited volumes go, this one is quite coherent and thought provoking.

Within the nexus of state evolution and insurgency, some of the relationships investigated are of particular interest to formal modelers and other theorists of revolution. Two examples are the effects of the type of counterinsurgency strategy pursued by the state and the effect of internal war on the relative power of the military within the state. The chapters on the Philippines (by Patricio Abinales), the Malayan Emergency (by Richard Stubbs), and Pèru (by Philip Mauceri) do the best job of investigating these issues.

The Malayan case provides an excellent illustration of the effectiveness of changing from a coercive, hawkish strategy of

counterinsurgency to a more dovish, "political" strategy aimed at addressing rebel grievances and building legitimacy. The hawkish strategy appeared to weaken an already weak state by putting a strain on resources and stimulating rather than repressing the growth of the insurgency. Changing to the political strategy, combined with a windfall of revenues from the Korean War, reversed this cycle. The state began to centralize and strengthen itself, increasing its extractive capability; addressing rebel grievances built legitimacy and diminished rebel strength. This not only defeated the insurgency but also provided the state with capacities that led to more successful economic development. In contrast, the Marcos regime in the Philippines pursued a more "predatory" mix of strategies against the NPA insurgency (the Moro rebellion is omitted). This allowed Marcos to centralize the Philippine state and freed him from the power of the *caciques* (the landed elite), but it failed to defeat the insurgency, as much because of the rebels' own self-reliant and decentralized strategy as because of the state's hawkish strategy. In the end, Marcos's failure as well as the NPA seem to be determined by poor strategic choices, which, with some U.S. influence, brought the *caciques* back to power.

In Peru, we see further evidence that the more hawkish strategy may tend to be less successful in the context of a weak state. In the fight against Sendero Luminoso, the poor administrative capacity and resource base of the state prevented the efficient implementation of a hawkish strategy. Furthermore, by giving the military more power in the state, the hawkish strategy reduced state legitimacy. Peru was unsuccessful against the guerrillas until the Fujimori regime first developed more efficient state administration and a stronger economy, which then were used to carry out an effective counterinsurgency strategy.

A more dovish, "political" mix of tactics does not always fare well either. The Algerian case (by Paul Rich) shows the FLN trying to coopt the Islamic FIS opposition by campaigns of Arabization and mosque building, but these fed the growth of the FIS and revealed the degree to which the government is cut off from the values of the opposition and much of society. Other weak state cases are important as examples of how insurgency can lead to state breakdown. In Mozambique (by Tom Young), Afghanistan (by Stephen Chan and Dominic Powell), and Sri Lanka (by Bruce Matthews), counterinsurgency has failed or nearly failed. Again, a concluding chapter might have yielded better generalizations about the reasons for these failures. Weak states do not have the capacity to pursue vigorous military strategies in the absence of intervention, and they are often ineffective in administering political strategies as well. In these cases the militarization of the conflict did strengthen the role of the military in government but did not result in the type of centralization, efficiency gains, and increased extractive capability that strengthened the Philippine state in the Marcos years.

Other issues which might have been considered in this volume include the role of interveners as well as the role of ideas and learning in each of these conflicts. Intervention was an important factor in the Afghanistan, Mozambique, and Northern Ireland cases, and the lack of intervention on one side or the other was important in Peru and the Philippines. Another theme which emerged from reading these chapters was that the success of counterinsurgency often depended on the application of lessons learned in other cases. This is true of the Northern Ireland chapter (by Paul Dixon), in which we see Britain applying several "lessons of Vietnam." In many of the failure cases, lessons from other insurgencies were not known or applied, though they obviously could have helped (for example, in Mozambique or Peru). The capacity to learn is an important element of the strength of the institutional state; reasons for the success or failure to learn should be more developed. Also, the effects of ideas, culture, and political identity issues (emphasized in the "fourth generation" of revolution theories, following the structuralist, "third generation" represented by Skocpol and others) appear important in some of the cases, particularly Algeria and Mozambique. These insights, however, are left undeveloped.

This book provides readers familiar with theories of revolution, social movements, and conflict processes ample evidence to provoke new insights. It will benefit readers who know much of the historical background of these cases and can thus understand the subtleties and uniqueness of some of the arguments made. Formal modelers will benefit from this collection as well; it provides a beginning empirical evaluation of some important assumptions and processes used in many models, including my own. In particular, the relationship among state and rebel strategies, state building, and the outcome of counterinsurgency and insurgency is shown to be complex but amenable to theory building.

Restrained Trade: Cartels in Japan's Basic Materials Industries. By Mark Tilton. Ithaca, NY: Cornell University Press, 1996. 225p. $29.95.

Troubled Industries: Confronting Economic Change in Japan. By Robert M. Uriu. Ithaca, NY: Cornell University Press, 1996. 290p. $45.00 cloth, $17.95 paper.

Japan under Construction: Corruption, Politics, and Public Works. By Brian Woodall. Berkeley: University of California Press, 1996. 227p. $28.00.

H. Richard Friman, *Marquette University*

These three books challenge the increasing polarization of the literature on Japanese politics between developmental state and rational choice approaches. The authors do so through seeking a more nuanced integration of bureaucratic autonomy and political pressure explanations of Japanese industrial policy. To explore this middle ground, the authors turn to substantive work on two underexplored areas of industrial policy: the domestic construction market and adjustment polices for Japan's declining industries.

Brian Woodall argues that Japan is best understood as a dual political economy in which high-growth sectors and a developmental state lie in a "symbiotic balance" with, but are "largely insulated" from, extensive clientelist networks (p. 135). He contends that Japan's dual political economy helps explain governmental resistance to U.S. efforts at market access, continued economic growth despite clientelism, and patterns of collusion between state and societal actors more extensive than those found in other advanced industrial democracies. In exploring these issues, Woodall concentrates on Japan's domestic construction industry as a "case study of a crucial policy domain within Japan's clientelist state" (p. 22).

Beginning in the mid-1980s, U.S. efforts to gain access to Japan's domestic construction market publicized the broader patterns of price fixing and bid rigging in Japanese public works projects. Why did the Japanese government resist pressures for increased market access? Woodall argues that resistance reflected the threat that liberalization posed to the extensive network of clientelism in public works, a network linking the interests of elite public works bureaucrats, career politicians, and construction contractors. Woodall's detailed work on the players and politics of collusion in Japan's domestic construction market, especially the sections on the

construction industry and Ministry of Construction, is a strong addition to the literature.

Nevertheless, Woodall's linkage between clientelist networks and Japanese resistance to U.S. pressure on public works risks overstating the former's effect. He ignores, for example, the broader context of bilateral trade disputes over market access distinguished by U.S. threats of retaliation under Section 301. This context suggests a wider array of bureaucratic players and the potential for interministerial pressures to disrupt the influence of clientelist networks. More important, Woodall overstates the relationship among the construction industry, the Liberal Democratic Party (LDP), and its policy tribes. Support for the finding that the construction industry is a primary financial source for LDP candidates, for example, is anecdotal and lacks comparative data on other industry and agricultural contributions. Faced with an inconclusive multivariate regression analysis on the determinants on public works spending, Woodall also overstates the importance of campaign contributions by firms as a possible explanation of tribal funding choices (compare pp. 118, 123, 161).

Woodall is less convincing in his secondary discussions of the relationship between clientelism and economic growth and the cross-national variation in the persistence of collusive practices. He notes the "much honored" and "supposedly iron-clad" hypothesis that clientelism inhibits economic growth in developing countries and underdeveloped regions, and he then cites Japan's experience over the past forty years as challenging this hypothesis (pp. 16, 134–7). Since Japan is not easily classified as a developing country during this period, the extent of the Japanese anomaly is unclear. Similarly, the question of cross-national variation in public works collusion requires additional work. Woodall offers only brief comparisons with Italy and passing references to the United States and France, which lend limited support to the conclusion that differences in institutions matter.

Robert Uriu turns to a broader focus on Japan's declining industries. He argues that they challenge the traditional views of Japan's success in structural adjustment and bureaucratic autonomy in industrial policymaking. Rather than rely on market forces to direct adjustment, bureaucrats have shielded declining industries, especially through the use of cartels. Similar to Woodall, Uriu takes issue with bureaucratic autonomy arguments. He hypothesizes that policy choices reflect the preferences and influence of industry actors as well as pressure from the industry's political supporters and an industrial policy environment of domestic (primarily intrastate) and international constraints.

Uriu accurately notes the potential for a stronger test of bureaucratic autonomy arguments in cases of declining industries in which public and private interests are more likely to diverge. Rather than assuming industry interests, Uriu also explores their sources. He hypothesizes that industry structure shapes the preferences of industry actors for political and/or economic adjustment strategies. Uriu tests his arguments on industry preferences and influence against eight detailed case studies of troubled industries in Japan, with a primary focus on cotton spinning, steel mini-mills, synthetic fibers, and shipbuilding.

Uriu's conceptual framework and case studies, however, contain inconsistences and omissions that overshadow the book's potential significance. In presenting the first portion of the framework, Uriu argues that "the main actors within an industry—individual firms and their labor forces—will be the ones most likely to influence policy outcomes" (p. 10). In contrast, Uriu's case studies reveal that industry associations often played a greater role than individual firms and labor, especially in less concentrated industries, such as cotton spinning. The case studies also reveal organized labor as less consequential than either individual firms or the industry associations. Labor's relative influence is also difficult to isolate since the author offers few cases in which the demands of labor, individual firms, and industry associations diverge. Although Uriu later acknowledges the mitigating effect of Japan's enterprise unions on labor's influence, the conceptual framework is weakened by its initial omission.

Uriu makes a better case for linking the broad preferences of individual firms and labor for economic and political adjustment strategies to "the structure of the industry," specifically concentration and size of labor force (p. 10). The case studies, divided along the lines of this operationalization, largely support the argument. But what other indicators matter? The work of scholars such as Peter Gourevitch and Helen Milner on the effect of international factors on industry preferences is part of a broader literature that Uriu needs to address, if only to rule out alternative explanations. Similarly, to what extent does a demonstration effect take place, whereby firms and labor look to the earlier experiences of their counterparts in other industries in following economic and political adjustment strategies? Uriu all too briefly acknowledges the limitations of a two-variable explanation at the close of chapter 2, and then adds a nine-sentence footnote to explore industry homogeneity as an additional variable.

The second portion of the book's conceptual framework, as presented in chapter 3, explores the policymaking environment, which Uriu defines as "the political and bureaucratic actors and institutions, both domestic and international, that are involved in or influence the policy process" (p. 23). Uriu focuses on the Ministry of International Trade and Industry (MITI) as "the relevant ministry" in industrial policy (p. 23). Other Japanese ministries and the literature on them are not addressed in the conceptual framework. This creates an anomaly in Uriu's case study of the shipbuilding industry, since the Ministry of Transportation dominates the analysis, and in the book's conclusion the ministry is ignored.

Uriu argues that industries can influence bureaucrats directly and work indirectly to obtain influence through the support of politicians. Each of the case studies reveals industry influence over the bureaucracy's policy choices, suggesting a challenge to bureaucratic autonomy arguments. Yet, the more important questions are why such influence occurs and what explains the extent of interindustry variation. The book contains an array of possible causal factors but does not offer or test explicit hypotheses.

Uriu writes, for example, that an industry's direct influence is shaped by control over information, the ability to withhold cooperation, and MITI's desire to avoid open confrontation. Indirect influence is shaped by industry assets in dealing with politicians—financial contributions, voting strength, the presence of contending constituents, and regional concentration (a variable added in the case studies)—as well as the sensitivity of bureaucrats to politicians and contending pressures from other industries. When variables stemming from the legal trust and international environments are added into the framework, the reader faces a double-digit array of explanatory factors chasing four primary and four secondary cases. Uriu acknowledges that parsimony suffers when integrating levels of analysis. Such integration efforts are commendable, but the proliferation of variables noted above has more to do with an underspecified conceptual framework. Thus, while Uriu's findings challenge "extreme versions of the 'bureaucratic dominance' view" (p. 254), the book offers less insight

into the variations in industry influence over the policymaking process.

Mark Tilton's work on basic material industries offers a more nuanced argument on the interaction between private and public actors in Japanese industrial policy. Tilton seeks to challenge traditional economic and political science debates over the sources of Japan's success in phasing out sunset industries. He argues that the Japanese bureaucracy neither has been successful in responding to market signals in basic materials industries nor has always sought to do so. Unlike Uriu, Tilton explicitly focuses on trade associations in his conceptual framework. He also broadens the scholarly debate over declining industries to include the understudied area of basic materials industries, explicitly focusing on petrochemicals, cement, primary aluminum, and ordinary grade steel.

Tilton contends that Japanese trade associations turned to long-term interindustry relational contracts to mitigate the effect of market signals on basic materials industries. Driven by the desire to retain access to strategic materials, MITI officials supported these efforts at informal governance by turning to capacity and price cartels, joint-sales companies, and limited subsidies. In some cases, these steps effectively protected domestic production from import competition. Tilton notes that trade associations and their LDP supporters also pressured MITI into backing more extensive subsidies, often over the opposition of the Ministry of Finance.

Relational contracting—long-term ties between firms—lies at the heart of Tilton's analysis of informal governance. He extends the concept to include the efforts of trade associations to build support networks between industries. Tilton argues that the success of "interindustry relational contracting" depends on three necessary variables derived from "a large number of cases" of major declining industries reviewed in the book's final chapter (p. 13). Specifically, the upstream industry must be concentrated and must have some type of "technological hook," or the downstream industry must be cartelized or receiving import protection. The author notes that two additional variables are helpful, and three additional systemic variables work to facilitate informal governance. Tilton finds that these variables, especially the three necessary factors, help explain differences in successful informal governance among Japan's basic materials industries.

Aspects of the conceptual framework and the author's findings, however, are troubling. As with Uriu's analysis, the explanatory factors outnumber the cases being tested, which increases the difficulty of determining patterns of causality. Greater problems lie with the determination and testing of necessary variables. In chapter 7, Tilton briefly reviews twenty cases of declining industries from which he derives his three necessary variables. These declining industries include the four cases against which Tilton seeks to test his conceptual framework, raising questions of selection bias and the extent of the framework's explanatory power.

The potential for selection bias lies in the characteristics of the four cases versus the broader set of declining industries. Of the twenty cases of declining industries, only the basic materials industries offer examples of "successful, long-term informal protection" (p. 191). They account for eight of the nine examples of necessary upstream concentration, the only two examples of technological hooks (petrochemicals and steel), and the only four examples of downstream cartels/protection (cement, fertilizers, soda ash, glass). Tilton's primary cases deal with four concentrated, basic materials industries. Two of these have technological hooks, one (cement) has the requisite downstream connections, and one (aluminum) lacks both factors. His finding that the cases fit the predictions of the model is, as a result of their influence on the larger sample from which the model is drawn, not surprising.

In conclusion, and critiques aside, all three works take the important step of moving the Japanese politics literature beyond its growing polarization. Insights lie in exploring the interplay among firms, organized labor, industry associations, bureaucrats, and politicians within the broader context of international relations. The research efforts in these books strengthen the literature on underexplored aspects of Japanese industrial policy through a detailed integration of extensive field research and primary source materials with the secondary literature.

How Russia Votes. By Stephen White, Richard Rose, and Ian McAllister. Chatham, NJ: Chatham House. 1997. $24.95.

Public Opinion in Postcommunist Russia. By Matthew Wyman. New York: St. Martin's Press. 1997. 256p. $65.00.

John Anderson, *University of Nebraska at Kearney*

These books read together represent a complementary effort to explain democratization in post-Soviet Russia. Together, the studies do much to enlighten us about the nature of Russian politics vis-à-vis the nascent public and the beginning steps of its development. As such, the books discuss social and political phenomena that should be of great interest to students of politics in general. Taken separately, the book by White, Rose, and McAllister offers an insightful narrative about the nature of Russian elections, especially as they have related to broader political changes in the country. In particular, *How Russia Votes* should be carefully read by students of comparative politics, development theory, electoral behavior, and public opinion. The book by Matthew Wyman explores public opinion in post-Soviet Russia, and its appeal will most likely be limited to political sociologists interested in Russian public opinion.

Wyman's book primarily recounts the findings of a host of public opinion polls taken in Russia. Unfortunately, Wyman's scope of inquiry was limited to polls taken between the late 1980s and 1993. This is unfortunate because the limited findings exclude many significant events of the postcommunist era in Russia. The election of the current Duma and Yeltsin's grab for presidential power both unfolded after the polls Wyman used had been administered and analyzed. Nonetheless, Wyman's monograph begins to explore interesting topics relating to the development and nature of public opinion in Russia.

How Russia Votes does not suffer from the malady of being too narrow in scope. In fact, this book has been praised by James Gibson (one of the publisher's reviewers) for being comparable to Campbell et al.'s landmark study, *The American Voter* (1960). He means to tell us that the book has a very broad reach that encompasses, in a meaningful manner, a complex of political and social phenomena. Gibson is also right in another regard: *How Russia Votes* makes an important contribution to our discipline because of its unique style. Much like *The American Voter*, the book deserve credit for its multifaceted nature. Mostly, White, Rose, and McAllister worked to integrate qualitative and quantitative research into a theoretically sophisticated analysis, that difficult task which always deserves praise.

On the quantitative side, White, Rose, and McAllister chiefly employed analyses of public opinion and electoral results. For data from survey research, they relied mostly upon the results of their own work on the New Russia Barometer (NRB) to analyze public thinking. The NRB was

instituted as an annual survey of social and political attitudes throughout the Russian Federation, and it has been labeled a "barometer" because it repeats questions to enable some measurement of changing attitudes (seeking continuity and discontinuity in individual political thinking). The authors also incorporated work by other pollsters when appropriate, such as the VTsIOM (a Russian public polling institution). To the authors' credit, they incorporated sophisticated statistical techniques into their overall discussion of Russian elections in a nearly seamless fashion. Far too often in the past, social scientists have used jargon-laden language when reporting important findings, but White, Rose, and McAllister avoided that difficulty by carefully wording their explanations.

It is also praiseworthy that the authors incorporated empirical findings directly from election results to strengthen their arguments about Russian electoral behavior. In electoral matters, their analyses were directed at several elections, including legislative and executive elections from 1989 through 1995 as well as the important presidential election in 1996. The authors' analyses of recent Russian electoral behavior were augmented by their addition of electoral results during Communist rule—dating back to 1922. This perspective of Russian elections allows them to explore issues relating to popular governance within the context of twentieth-century Russian history. This perspective helps readers understand Russian politics in a manner that is less sensitive to the narrow interests of recent times. In addition, it allows readers to better understand how debates in earlier times were open to issues relating to democracy and equality.

The authors notably incorporated qualitative information into their narrative as well. This is primarily taken from news sources, speeches, and other publicly available material. While the exclusive use of these limited forms of qualitative methods indicates the work lacks insight into some interpersonal relationships—like the kind of techniques which provide greater understanding of identity formation and acculturation—the inclusion of archival information does allow the authors to do more than speculate about their quantitative findings. They can build cases for the causes of institutional change and begin to explain the role taken by culturally based meanings in the development of modern, liberal democracies.

By weaving their sources into a narrative, White, Rose, and McAllister were further able to describe voting behavior and political struggles in Russia in some detail. Whereas other scholars often place themselves in narrow disciplinary structures, these authors were able to weave an insightful, provocative, and interesting analysis that attempted to detail many facets of social and political life in Russia. This is particularly true because the authors used sources of qualitative information to analyze institutional dimensions of political matters as they developed in Russia. Readers will be especially impressed by the way political culture, weak institutional norms, and unusual electoral behavior were combined to form a politics characterized by outright power struggles, often ignoring democratic norms of behavior we expect. Such insights should warn us about the nature of problems other nations may face when people begin to believe that elections and the rule of law do not matter.

Many other findings in *How Russia Votes* are worthy of mention. For instance, the authors note that democratization is a lengthy process because it requires changes in a country which shake its foundations. Those foundations are obviously institutional and less obviously cultural (including ways of thinking and acting) in their nature. Often the tendency is to approach these (and other) foundational characteristics as though they were mutually exclusive. White, Rose, and McAllister refrain from this much easier and less accurate form of analysis, instead explicating the characteristics of Russian political development within the more complex warp and woof of human existence.

To suggest that any book can be characterized by broad brushstrokes, lacking either flaws or worthwhile characteristics, is inappropriate. For instance, White, Rose, and McAllister ineptly apply rational choice theory to suggest that Russian political parties behaved irrationally in the 1995 Duma campaign (pp. 198–201). While I may share some of the authors' apparent disdain for rational choice theory, that does not justify the inappropriate usage of the perspective. Each theory of politics deserves to be applied in a manner consistent with its basic tenets. Unfortunately, the authors begin their analysis of rational electoral behavior by prescribing the rational outcome; in essence, they were trying to show that Russian electoral behavior was not rational and that the model did not explain behavior. The authors seemingly employed this approach to argument without considering how actions that did not fit their prescribed model could be seen as strategic actions within the rational choice paradigm (as suggested by the prisoner's dilemma). In addition, the authors might have considered how game theorists would argue that electoral behavior is best modeled as a reiterative game.

Turning back to Wyman's book, we find few similarities to the strength's exhibited in the White, Rose, and McAllister volume. Wyman's work is narrow in scope, method, and writing style. Nonetheless, the book complements *How Russia Votes* because it presents analyses of public opinion in far greater depth. For instance, Wyman examined ethnicity in Russia and its associated influences on public opinion. Similarly, Wyman's work leads readers to think more carefully about the important effect of rural/urban differences in Russia. It is the explication of detailed sociological phenomena that makes Wyman's book important enough that students of Russian politics and public opinion will want to read it. Others will find that *Public Opinion in Postcommunist Russia* adds support for the understandings built in *How Russia Votes*.

In summary, I would suggest reading both books because they complement each other. If time is a problem, however, then read *How Russia Votes*, since it is a far richer text and may one day be known as a classic in political science. *Public Opinion in Postcommunist Russia* will probably interest only a small circle of students and scholars because its frame and scope are limited. If time allows, readers should find that reading both books will add to their knowledge base, especially as related to public opinion and the influence of the social setting on the development of the Russian public and its attendant politics.

International Relations

Presidents and Foreign Policy: Countdown to Ten Controversial Decisions. By Edward R. Drachman and Alan Shank. Albany: State University of New York Press, 1997. 390p. $19.95 paper.

Deborah Welch Larson, *University of California, Los Angeles*

How can we evaluate past foreign policy judgments of presidents? Too often scholars are swayed by their own political biases for or against a particular foreign policy. Retrospective judgments are also subject to hindsight. Analysts criticize a leader for failing to use information that was not available to him at the time, or for failing to predict events that could very well have turned out differently. Drachman and Shank present more objective criteria by which to evaluate past presidential foreign policy decisions. They sketch the foreign policy problem facing the president at the time, describe the options available, and explain why he chose one alternative rather than another. Their goal is to enable students to empathize with the president's position and to work through the problem as it must have appeared at the time. They then illustrate their evaluative framework in ten case studies of "controversial" foreign policy decisions.

At the outset, the authors state that they will evaluate the president in terms of how well he understood the historical context of the issue and applied that knowledge in making a specific decision. A president's options are necessarily limited by what his predecessors have done. The sequence of state actions in a region enables one to see whether the current problem is a significant break from past events. Continuities in behavior provide clues about the state's goals and intentions. The president should also consider the potential reaction of allies and whether the decision is consistent with his world view and national security strategy. Finally, in order for the policy to be implemented effectively and consistently, the president must have the support of Congress and the American people. Drachman and Shank assess the president's effectiveness in persuading relevant audiences of the merits of the decision. Finally, they consider whether a decision achieved the president's goals, both in the long and short term. The authors have provided a checklist of these factors for students to use in evaluating these and other presidential decisions.

Drachman and Shank assert that several decisions show evidence of the groupthink syndrome, in which the desire for unanimity overrides critical thinking and reality testing: the Bay of Pigs fiasco, Johnson's failure to end the Vietnam War after the Tet offensive, Nixon's decision to invade Cambodia, and Ford's approval of covert intervention in the Angolan civil war. The authors agree with Irving Janis's thesis that Kennedy approved the disastrous Bay of Pigs invasion because his advisers did not want to disturb the comfortable atmosphere of consensus by raising questions and doubts. But one wonders whether groupthink is essential to explaining Kennedy's decision to implement the plan, which was developed in the Eisenhower administration. Eisenhower did not give it his final approval because the CIA had not been able to find a Cuban exile leader who could organize the covert forces and lead a new government. Despite the absence of a popular exile leader, Kennedy did not object to the covert operation when he was briefed after the November 1960 election, before he held meetings with his advisers. By 1961 the CIA plan had developed bureaucratic momentum; calling off the operation would have led to politically damaging leaks and protests from the Cuban exiles. Thus, Kennedy might have approved the Cuban invasion even if his advisers had pointed out potential problems.

The authors also do not show that Johnson was unduly influenced by apparent consensus among his advisers when he rejected the "Wise Men's" advice to end U.S. involvement in the Vietnam War. Rather, Drachman and Shank suggest that Johnson was following his customary decision style of choosing the middle course when he decided on a partial rather than a complete bombing halt in 1968 and pursued negotiations with the North Vietnamese without making concessions necessary for agreement. The authors indicate that Nixon, after discussion with his advisers, shifted to a more risky plan using U.S. forces as well as South Vietnamese troops to invade Cambodia. They are less persuasive in arguing that Ford approved covert U.S. intervention in the Angolan civil war in 1975 because of groupthink among his advisers; Kissinger took the lead in pressing Ford to approve aid to two factions.

While the evaluative scheme seems plausible and reasonable on the face, it does not always work well when applied to specific cases. In evaluating the consequences of a decision, one should consider whether feasible alternatives would have yielded a better outcome. For example, Drachman and Shank criticize Bush for failing to take advantage of the U.S. victory to destroy the Republican Guard or depose Saddam Hussein. But could U.S. forces have installed a replacement government without incurring unacceptable costs? Many of the authors' criteria conflict, forcing the president to make painful tradeoffs, for example, between pursuing his national security objectives and maintaining public support. But the authors do not prescribe how such tradeoffs should be made. When consequences are far removed in time and place from an initial event, many intervening steps could alter the final outcome, making it difficult for the analyst to draw a causal connection.

The book needs a concluding chapter, to sum up what we have learned about the factors affecting the quality of presidential judgments and to suggest areas of further research. The authors do not draw any generalizations about the conditions which promote good or bad decisions.

Presidents and Foreign Policy will stimulate the interest of undergraduates and provoke them to think in greater depth about problems of foreign policy decision making. The book would be useful to researchers on decision making as well, because the case studies are well researched, concise, and provocative.

We Now Know: Rethinking Cold War History. By John Lewis Gaddis. New York: Oxford University Press, 1997. 425p. $30.00.

Miroslav Nincic, *University of California, Davis*

Not only is John Lewis Gaddis one of the most influential historians of the Cold War, but also he is a frequent critic of our own discipline. Almost as often correctly as not, he takes political scientists to task for failing to match methodological and theoretical pretense to relevant explanatory performance. While claiming no final word on the subject, his most recent book assesses the Soviet-American rivalry from the perspective of the post–Cold War era. In this regard, he successfully accomplishes what, thirty years previously, Louis Halle's *The Cold War as History* (1967) prematurely sought to do. A major advantage of writing several years after the Cold War's end is the possibility of comparing that chapter of history with other epochs, including a post–Cold War period.

In Gaddis's words, it stands to "place the Cold War within the stream of time; it will not confuse the Cold War with the stream" (p. 282). It also allows a proper degree of detachment, and it makes it easier to regard that period from the point of view of participants other than the United States (mainly the Soviet Union and China).

Benefiting, inter alia, from new material drawn from bulletins and working papers of the Cold War International History Project at the Woodrow Wilson Center in Washington, DC, the book deals principally with the superpower rivalry in Europe (Germany in particular), the Third World (especially Asia), and the nuclear arms race. It is a pleasure to read and, as far as a nonhistorian can judge, it is very competent historiography. The question here, however, is how valuable this volume is likely to be from the perspective of a political scientist.

Is there, to begin with, much of which those of us who study international rivalry have been unaware and which might make us revisit our assumption and theories? A little, but not much. Perhaps it is still too early to expect vast new quantities of exciting information, and, expectedly, the additions to factual knowledge Gaddis provides are somewhat at its margins. Still, many will find his coverage of the reasoning of participants in key Cold War crises useful. The chapter on the Cuban missile crisis does an excellent job of presenting the range of options Kennedy was really considering, debunking the belief that a nuclear exchange was very close at hand, and analyzing the inverted perceptions that Castro and Khrushchev had of what each was doing for the other. I was also intrigued to learn of the role that Beria had sought to play in resolving the German question after Stalin's death, and of Stalin's initial indifference to the Middle East, its geopolitical position notwithstanding. Virtually every chapter also presents an interesting exercise in counterfactual speculation, extending the range of contingencies many political scientists would have considered.

The second issue is whether the book contributes much of an explanatory nature—causal propositions that, applied to the Cold War, place its occurrence and trajectory in a new light. The author, as do most diplomatic historians, accounts for both minor and major events almost exclusively in terms of human agency—things happen because of what important people wanted and how they acted. There is very little appreciation of the need to account for the patterns of interest and power in which motivations are rooted and which constrain the scope of policymakers' ability to shape history. Where propositions of this sort are advanced, many political scientists may be tempted to sigh. There is, for example, frequent recourse to the explanatory power of "power vacuums," or to such things as the "hydraulic theory of geopolitics," according to which "dams built or dikes bolstered in some locations diverted the Soviet expansionist impulse to others." (p. 167). At the same time, Gaddis does a good job of analyzing the role of ideological values in major foreign policy strategies and how the two superpowers resolved conflicts between their values and interests.

Of course, these are the criticisms of a political scientist, while the book was written by a historian for an audience of which we are not necessarily the dominant members. In any case, our own discipline will continue to benefit both from the willingness of historians like Gaddis to probe our shortcomings and from the ever-improving store of factual information with which they provide us.

The North American Trajectory: Cultural, Economic, and Political Ties among the United States, Canada, and Mexico. By Ronald F. Inglehart, Neil Nevitte, and Miguel Basañez. New York: Aldine de Gruyter Press, 1996. 198p. $40.95 cloth, $19.95 paper.

Risking Free Trade: The Politics of Trade in Britain, Canada, Mexico and the United States. By Michael Lusztig. Pittsburgh, PA: University of Pittsburgh Press, 1996. 180p. $49.95 cloth, $22.95 paper.

Christopher Sands and Sidney Weintraub, *Center for Strategic and International Studies*

Why is it that governments choose to pursue a course of trade liberalization? Scholars have offered a variety of answers ever since Adam Smith's *Wealth of Nations* (1776) and David Ricardo's *Principles of Political Economy and Taxation* (1817) made the case that international trade is good for nations, and therefore its promotion is wise policy. In these two books, the authors approach this theme from different perspectives and argue that trade liberalization, particularly in North America, may also be wise politically.

Lusztig examines various models of the decision-making process and argues that support for liberalizing trade is a second-order objective adopted by political actors as a by-product of the first-order objective of effecting a favorable political realignment of domestic interests. In any society some interests benefit from trade liberalization while others are harmed by the reduced scope for rent seeking. These groups may align themselves in support of common political coalitions within parties and within the polity. The economic costs and benefits of liberalizing trade are significant enough to redistribute this support, and so political actors who stand to benefit from this realignment adopt corresponding positions on trade. When in power, these political actors rationally pursue international agreements that bind the country to the consequences of liberalized trade and thereby attempt to confirm the new constellation of political support that benefits them.

Having laid out this model, Lusztig applies it to four historic cases, each of which is considered in a separate chapter. The cases are: the repeal of the Corn Laws in Great Britain by the government of Robert Peel; the adoption of the Reciprocal Trade Agreements Act in the United States with the strong support of President Franklin D. Roosevelt; the successful pursuit of the Canada–U.S. Free Trade Agreement (CUFTA) by the Canadian government of Prime Minister Brian Mulroney; and the decision to negotiate the North American Free Trade Agreement (NAFTA) by the administration of President Carlos Salinas in Mexico.

As one might expect when an author employs cases involving complex motivations, specialists in the politics of the four countries at the critical moments will likely disagree at several points with the interpretation given by Lusztig to the motives behind particular decisions. For example, in the case of Canada, Lusztig links the decision to pursue CUFTA, popular in Quebec and western Canada, with the Mulroney government's constitutional reform strategy designed to placate Quebec nationalists unhappy with the 1982 constitutional reform adopted by the government of Pierre Trudeau. The tenuous linkage Lusztig asserts between these two initiatives, never made publicly by the Mulroney government, raises a puzzling question: If the two initiatives were linked as parts of a strategy to build a lasting electoral coalition, then why did Mulroney chose to seek a mandate to adopt CUFTA in the 1988 federal election without attempting to secure as well a mandate for the swift ratification of the Meech Lake constitutional reform package by holdout provincial govern-

ments in that campaign (a move that arguably could have strengthened the government's efforts in this area)? In the Mexican case, Lusztig implies that Salinas pursued NAFTA as a by-product of the domestic Solidarity program for which Salinas sought the support of the masses. The argument for this reasoning is not overwhelming.

Lusztig is more successful in arguing the British and U.S. cases, perhaps because in these he is considering powerful countries that altered their policies toward weaker states, and so the primacy of domestic considerations is credible. In the Canadian and Mexican cases, the role of the predominant trading partner is not directly addressed, leaving unexplored the question of how changing the structure of bilateral relations beyond the economic sphere would influence domestic interests and the pattern of interest realignment in support of or opposition to trade liberalization.

Nevertheless, Lusztig's model of political entrepreneurship is valuable as at least a partial explanation for the decision by governments to pursue a strategy of liberalizing trade. For this reason the book will be of interest to readers seeking a conceptual framework for policymaking in the area of international trade.

Inglehart, Nevitte, and Basañez offer a model that also places trade liberalization behind other considerations for governments in North America. Yet, their argument is about more than the decisions that led to NAFTA; it is about three societies undergoing profound transformation driven by a process of economic integration that has intensified their interaction and introduced new tendencies toward convergence in the values and expectations of citizens in Canada, Mexico, and the United States.

In making such a daunting case, these authors deploy an impressive amount of data on individual values and attitudes drawn largely from the annual World Values Survey, itself based on the model of the Euro-barometer series. An appendix on the survey method includes some of the questions asked but is disappointingly brief in its description of the study design, instead referring the reader elsewhere for a more detailed presentation.

The authors argue that as values change over time, so do perceptions of national interests, and it is possible to see in the survey responses a pattern of movement with clear generational striations from materialist to postmaterialist values in all three North American societies. Looking at data from the early 1980s, the authors demonstrate that in many ways Canada, Mexico, and the United States were becoming more alike. Gaps in the three societies' attitudes toward sexual permissiveness/restrictiveness, respect for authority/individual autonomy, family duty/independence, religiosity/secularity, and civil permissiveness/order narrowed in the observed period. Of particular interest to the authors is the apparent decline of deference to authority and attendant distrust of political leadership in all three countries. This is linked directly to a decline in the ability of these governments to tell their citizens what to do, and a corresponding assertiveness on the part of the three publics in demanding things from their politicians. All three countries, the authors argue, are undergoing an epochal rebalancing of the roles of the state and the economy and of individuals and their society. Significantly, it is in this context that NAFTA was conceived and realized.

The authors contend that the data can be used to predict the future direction of the shift in values within and among the societies, and they make a compelling case by showing that the data from the 1980s foreshadowed the direction that attitudes would take into the 1990s. The next logical step is to look ahead, and to their credit the authors attempt to do just that in addressing the explosive issue of the political integration of the three countries. Carefully considering how attitudes toward the possible advantages and disadvantages of political union might translate into support for the amalgamation of the three societies, the authors conclude that the citizens of Canada, Mexico, and the United States may yet warm to this concept, and the idea cannot be easily dismissed.

The North American Trajectory raises intriguing questions about what is actually happening in contemporary North America and what the consequences of deepening economic integration in an era of high-velocity information flows might be. Regardless of one's attitudes toward continental integration, this book is a good read. Not since Louis Hartz (*The Founding of New Societies*, 1964) and Seymour Martin Lipset (*Continental Divide*, 1990) has such a wide-ranging and fascinating analysis been offered of the character and aspirations of the people of these three countries in a comparative context. This book is highly recommended for students and policymakers considering the direction that North American integration may take.

Tamed Power: Germany in Europe. Edited by Peter J. Katzenstein. Ithaca, NY: Cornell University Press, 1997. 328p. $49.95 cloth, $19.95 paper.

The German Predicament: Memory and Power in the New Europe. By Andrei S. Markovits and Simon Reich. Ithaca, NY: Cornell University Press, 1997. 248p. $29.95.

Beverly Crawford, *University of California at Berkeley*

Since the Berlin Wall's collapse, the "German Question" has dominated debate about the future of Europe. Few need to be reminded that the growth of German power provoked two bloody wars in this century. The current question is framed this way: Will accelerated European integration subjugate German power to a united Europe? Or does it provide a veil behind which Germany exercises self-interested dominance, both in Europe and on the international stage?

These books will reshape the terms of that debate. Indeed, both point to the limitations of the question itself. Since 1949, Germany has accepted constraints on its sovereignty and military capability in ways unique in world history for an important European power. And contrary to many expectations, there is little evidence of pernicious German nationalism in current foreign policy. Indeed, Germany appears to have broken radically with the traditional practices of national sovereignty so common in large European democracies. Thus, both books address the more interesting puzzle: Why has the growth of German power *not* been accompanied by a return to the "normal" practices of great powers and sovereign states? What explains the "taming" of German power? They provide answers whose significance reaches far beyond these questions themselves.

In rejecting both realist claims, which suggest that Germany will maximize its power in Europe, and institutionalist arguments, which claim that Germany is constrained only by its commitment to multilateral institutions, these books offer new approaches to the study of foreign policy and international relations. They argue that culture and identity are primary forces driving the current policies of Germany as a rising power, offering pioneering perspectives that can greatly advance our understanding of the missing analytic link between international relations theory and foreign policy analysis.

Markovits and Reich stress cultural factors *within* Germany that limited the exercise of power, even as Germany's economic might has grown. Before unification, they argue, three forces constrained Germany's growing power: neigh-

bors who feared resurgent German aggression; Germany's inability to export its culture abroad; and Germany's unwillingness to use power, a reticence driven by an "ideology of reluctance." After unification the first two constraints were partially lifted. Others' acceptance of German unification suggests that old fears are buried. And Germany's ability to export its culture still remains limited. The decisive factor explaining German reluctance to maximize power is the ideology of reticence.

That ideology is explained by "collective memory," a potent ingredient in any nation's political culture and foreign policy tradition. While Germany's memories are multiple and clustered—memories of Weimar and the Bundesrepublik also mark public debate—the defining memory remains that of the Nazis' fatal exercise of power, the memory of Auschwitz. That memory now shapes a fear of foreign engagement. And as long as it defines political discourse, Germany, despite its predominance of regional power, will assume neither a position of malevolent dominance nor hegemonic leadership in Europe. The book's careful discussion of German reluctance to commit troops abroad despite vast military potential clearly supports the argument.

But although the memory of Auschwitz currently guides a reticent policy, Markovits and Reich argue, the future is uncertain. Identity rooted in collective memory can change as postwar elites die out and as competing memories rise to dominance. And lurking alternative memories open the door to a new German nationalism. Indeed, in the current period, they warn, "Germans are trading a European identity for a German one (p. 205)." We cannot be sanguine about the future of "tamed" German power.

Katzenstein and his collaborators disagree. More important than domestic collective memory, they argue, are external institutional forces shaping German behavior. Participation in European institutions defines German identity as explicitly international and particularly *European*. And that identity leads Germany to consistent pursuit of policies that support European integration even if they reduce German power or undermine its narrow interests. Because Germany's European identity is taking root in stable external institutions, its foreign policy preferences are increasingly multilateral. This process of identity and interest transformation tames German power now and will continue to do so.

For Katzenstein and his collaborators, deepening European institutionalization ensures that future. Multilateral institutions do more than constrain state behavior. They mold the identities of their participants. And identities do not change easily. This does not mean that all is rosy: A European identity includes heavy doses of xenophobia and exclusion. Recall that Germany buttressed its controversial push for Croatia's recognition with claims that Croatia was committed to "European" values; Serbs, according to the influential German press, were "hardly European at all." Katzenstein's point, however, is not that Germany's European identity will ensure regional peace but that a "nationalistic" identity is unlikely to reemerge in Germany as long as European integration continues.

This view contrasts with the uncertain future that Markovits and Reich envision. And that contrast goes to the heart of the debate between these two volumes. *Tamed Power* sees a constitutive role for institutions in the transformation of German identity and behavior. *The German Predicament* argues that institutions fine-tune the values and memories of dominant classes, but those values and memories lie at the root of ideologies shaping identity and policy.

This debate marks other disagreements, such as the issue of regional hegemony. The exact threshold of material capability necessary to achieve a hegemonic position—even a regional one—remains unspecified in the literature; how much power it takes to be a hegemon is now a matter of judgment. *Tamed Power* argues that because of its European identity, Germany sacrifices self-interest for multilateral cooperation in the *absence* of sufficient hegemonic capability. And because European institutions stabilize the region, hegemonic leadership is unnecessary. Markovits and Reich argue that Germany's material capabilities suffice for it to become a European hegemon, but due to its ideology of reluctance, it cannot make sacrifices required of a hegemonic leader. And Europe *does* need a leader.

A second disagreement concerns the international reaction to postunification Germany and the role of institutions in shaping that reaction. Markovits and Reich measure the neighborhood climate by examining responses to German unification. Others' initial reactions indicate long-term perceptions of German power and provide a basis for evaluating the claim that external forces—neighbors' fears—could trigger external constraints that explain Germany's reticence.

The authors found widespread acceptance of German unification, suggesting little apprehension of a resurgent Germany. These findings should silence those who argue that Germany's neighbors feared a new *Sonderweg* and therefore chained Germany more tightly to European institutions. Ironically, however, the fears others *did* express were defined less by memory of past German atrocities than by present realities of German economic power, their own economic dependence on Germany, and even German neglect of its old partners as it now looks eastward for economic opportunities.

Using different indicators, Katzenstein's collaborators found more varied responses to postunification Germany. Ironically, too, they found a greater role for the memory of Nazi atrocities in defining others' fear of Germany than did Markovits and Reich. But they found that those memories were activated *only under particular institutional conditions*. The extent of participation in EU institutions and domestic institutional compatibility with Germany greatly influence others' perceptions of German power. Without these institutional buffers, German relations with smaller European states are strained and World War II memories remain alive. Where buffers exist, relations are cordial, and institutional constraints are experienced as "soft." These constraints are also "soft" because German participation in European institutions defines German interests and identity. These claims are buttressed by exhaustive case studies that present new and important empirical material, particularly on Europe's smaller states.

How can we assess the relative strength of these competing arguments? Markovits and Reich present a pluralistic palate of competing German memories which may all vie for dominance when a new political generation takes the helm. We have no way of telling *which* collective memory will come to dominate or why a new political elite may pursue more nationalistic policies. And because almost any future policy trend can be traced to a particular memory, the argument is difficult to falsify.

Similarly, although the argument of *Tamed Power* rests on comparative case studies, permitting some factors to remain constant while others are examined, the institution/identity explanation is marred by the supposition that the European identity of France and Britain is weaker than that of Germany. No explanation for this variance is offered that is consistent with the argument. Why does institutional participation shape Germany's identity more than that of other large European powers? Although Katzenstein hints that institutional compatibility explains the variation (but does

not explain why Germany's institutions are more compatible with EU institutions than those of France or Britain), he ultimately relies on realist notions of self-interest and power balancing: France calculates that it can relinquish regional control from Paris to gain control in Brussels, and Britain's geographical location leads it to continue the role of power balancer in Europe, undercutting the power of European institutions to shape British identity. What prevents Germany from falling prey to similar forces? Perhaps it is because current German identity and European institutions were mutually constitutive. France and Britain helped reconstruct German identity through those institutions, while their own national identities remained intact; thus their more self-interested behavior. Postwar legacies, therefore, may explain more about identity formation than institutional participation.

Neither book claims that it will scientifically "test" its argument. Each admits that its evidence is illustrative, not conclusive. The strength of each is the construction of an analytic framework to explain the puzzle of German power. A complete test of the argument is neither book's aim and is rightly left to future researchers. Nonetheless, the empirical material presented suggests that Germany's behavior is subject to equally valid alternative interpretations.

Some of that material even supports neorealist or neoliberal institutionalist claims. Markovits and Reich caution that Germany exercises its preponderant power inadvertently, occasionally triggering crises like the 1992–93 ERM breakdown or the bitter dispute over Croatia's recognition: Germany seemed to behave like a "normal" power, even exercising dominance. This behavior, though now the exception, is subject to realist interpretations. Jeffrey Anderson argues in *Tamed Power* that Germany's attention to relative gains since unification is increasingly evident, a finding that could support some realist predictions.

Katzenstein argues, however, that what escapes realists is that Germany's "self-interest" is no longer a national self-interest. The "self" has become the idea and the region of Europe, and much of Germany's behavior is consistent with this broader view of identity and interest. But it is also consistent with a liberal institutionalist version of hegemonic stability theory, which suggests that dominant liberal states identify their self-interest with the stability of the system in which they operate; they sacrifice narrow self-interest to achieve it. If Germany's European identity is stronger than that of other powerful EU members, then why is Germany's structural position of European dominance not the key factor shaping its European behavior?

Neither approach specifies the conditions under which Germany acts in narrow self-interested ways and the conditions under which it is reluctant to do so. And the conditions that *are* specified are not consistent with the arguments. Concurring with other analysts, Katzenstein agrees that narrow self-interest is expressed more frequently when EU institutions are weaker and less frequently when institutions are stronger. But if European identity is to have independent explanatory status, it must be a more powerful force than the particular institutions that give birth to it. Otherwise the argument comes dangerously close to liberal institutionalism. Similarly, Markovits and Reich's explanation of Germany's "bull in the china shop" behavior is not consistent with the collective memory argument. Why the memory lapse?

These issues, however, do not weaken the significance of these important books. Both contribute major analytical statements and compelling challenges to the conventional wisdom. As Markovits and Reich note, "the debate about German power will be lively and unfettered" (p. 204). These two intriguing arguments provide the opening salvos and set the terms for that debate. They will long be crucial sources for any attempt to comprehend Germany's European role in the twenty-first century.

Economic Incentives and Bilateral Cooperation. By William J. Long. Ann Arbor: University of Michigan Press, 1996. 151p. $39.50.

Gerald L. Sorokin, *University of Iowa*

Almost every introductory textbook in international relations begins by discussing two things: how the agenda of international politics has grown in recent years as new problems arise and new actors enter the political arena, and how technological developments both increase the cost of conflict and create opportunities for cooperation. But simply recognizing the changes in the agenda and the role of technology is not the same as developing and testing theories to account for these things. Nor is it sufficient as a guide for policymakers. What we need is systematic research into problems and policies—some new, some familiar—that have been largely ignored in the scholarly literature. Long's book is a laudable attempt to do just that: He tackles a problem (how to bring about cooperation) and a policy (the provision of economic incentives) that are not well understood. Although Long's short book is far from the final word on this subject, it provides a useful starting point.

The book is organized around a theoretical chapter and three case studies. Long's principal theoretical argument is that a state offering economic incentives, such as preferential trade or technology transfer, in exchange for political compliance may be better able to exact concessions than if it applies economic sanctions. According to Long, a key difference between the incentive and sanction strategies is that domestic constituencies in both the sending and receiving state are more likely to support incentives. In the sending state, for example, "exporters and investors take a growing interest in the gains from trade and technology associated with new or expanded commercial relations" (p. 25), which leads them to support the policy. Moreover, unlike sanctions, incentives "are not, per se, an affront to recipient state sovereignty, [which makes them] more likely to penetrate the state and shape the process of preference formation within the recipient state's society" (p. 28).

Long's theoretical story is plausible, though the hypotheses to which he repeatedly refers are never explicitly presented. This omission makes it difficult to contrast his argument with one based on a structure of international payoffs. That is, it is difficult to know whether and when his predictions are consistent with those of an international-based argument and, therefore, whether the empirical evidence confirms or disconfirms his expectations. Furthermore, Long claims that his theory helps overcome some of the shortcomings of a game theoretic approach to questions of cooperation. I am unconvinced: Even if the criticisms of game theory that he cites are correct—and they are, at best, weakly so—his theory suffers from some of the same problems. For example, he accepts Robert Jervis's argument "Realism, Game Theory, and Cooperation," *World Politics*, April 1988, pp. 317–49) that a weakness of game theory is that it takes players' preferences as predetermined. While Long's attempt to look inside the proverbial black box is a step in the direction of understanding motivations, he can easily be criticized for taking substate preferences as given. I am more likely to be persuaded by a game theoretic model in which state leaders react to both internal and external interests than an informal

argument in which relationships among actors and variables are not laid out explicitly.

Long illustrates the theory with case studies of U.S. attempts to exercise influence through economic incentives. His three cases—relations with Sweden concerning nuclear power beginning in the 1950s; relations with China between Deng's 1978 visit to the United States and the first high-level military exchanges in 1986; and relations with Czechoslovakia after the fall of the Communist regime—are similar, in that they all involve the application by the United States of economic incentives, with mostly successful results. The problem with the case studies is not that they are poorly researched or written—they are neither—but that there is no way to generalize from them. Long makes several references to U.S. policy toward Iraq in 1990, a case in which economic incentives, to the extent they were implemented, failed to produce changes in the target state's behavior. A chapter on that case, or another failure, would have helped make the inferences he draws in his conclusion more convincing.

Similarly, since Long is interested in international politics in general and not just U.S. foreign policy, it would have been helpful to read about a case in which the sender was a country other than the United States. Long realizes that U.S. interests, capabilities, and behavior are not typical. But without sufficient explanation he extrapolates from his case studies, saying that "incentives may be equally or more important policy instruments for other countries" (p. 81). Perhaps, but incentives may also be less important to other countries, especially when they are too weak economically to offer concrete rewards for compliance.

As Long points out in his conclusion, a large-N study would be a useful complement to this book. I do not agree with him, though, that "the relationship between the sender's market power and the success of attempted incentives might be operationalized without great difficulty" (p. 94). It is the very complexity of the political and economic environment that would make such a relationship difficult to measure. Indeed, if Long's book teaches researchers one thing, it is that adding new variables and actors to our models makes it harder, not easier, to draw convincing conclusions. For policymakers, Long's book provides some useful examples and, by implication, suggestions for making better use of the range of policy instruments at their disposal. I hope both communities take advantage of Long's work and that the study of incentives continues to develop.

Internal Factors in Russian Foreign Policy. By Neil Malcolm, Alex Pravda, Roy Allison, and Margot Light. New York: Oxford University Press, 1996. 356p. $72.00.

Russian Foreign Policy: From Empire to Nation-State. By Nicolai N. Petro and Alvin Z. Rubinstein. New York: Addison-Wesley Longman, 1997. 347p. $31.95.

From Empire to Anarchy: Postcommunist Foreign Policy and International Relations. By Mette Skak. New York: St. Martin's Press, 1996. 340p. $55.00 cloth, $18.95 paper.

James M. Goldgeier, *George Washington University*

During the Soviet period, the classic approach to studying Moscow's foreign policy was embodied in the title of Adam Ulam's *Expansion and Coexistence* (1968, 1974). Scholars typically wrote about a tension between those factors, such as political culture or ideology, that they believed drove an expansionist foreign policy and those factors, such as the constraints imposed by the international system, that were seen as restraining any desire to build a larger empire or to promote world revolution.

With the collapse of the Soviet Union at the end of 1991, authors have continued to address these age-old questions: Is there something about Russian culture or geography that will lead to continuity in foreign policy, or will change occur due to a new international environment and a new domestic structure? Most important, how will a postimperial Russia define and pursue its national interests?

In this vein, two key questions are asked by these three works. First, will Russia become "normal"? By that analysts typically mean will Russia forgo imperial claims on the other New Independent States (NIS) and interact with them on the basis of respect for their sovereignty? If the answer is yes, then Russia would respond to opportunities and threats as a traditional territorial state, trading with its neighbors and allying with some of them in order to increase its security and prosperity, but it would not attempt to reintegrate these states either economically or militarily as occurred during the tsarist and Soviet periods. If one believes that the answer is no, however, because certain internal impulses lead naturally to Russian expansion, then the current period is merely an interlude between imperial systems, as was the case between 1917 and 1922.

The other key question asked in these books is: Who makes Russian foreign policy? We want to know how much power the president has to set foreign policy priorities, what role the Duma and Federation Council play, whether regional governments conduct their own foreign policies, when different ministries inject themselves into the process, and how the bankers and industrialists shape Russian interests. Over time, we should see more use of comparative approaches to give greater analytic clarity in the study of this particular state, and the volume by Malcolm and others will be a valuable resource for anyone engaged in a comparative effort to understand the influence of domestic structures, political parties, public opinion, and civil-military relations on a state's foreign and security policy.

The most interesting questions raised by each of these works concern Russian attitudes and policies toward the other NIS. The radical shift in foreign policy toward the West had begun, of course, with Mikhail Gorbachev's redefinition of Soviet interests, and thus there was less of an abrupt transition on those foreign policy issues after the Soviet Union's collapse. But dealing with the former imperial possessions has been much more difficult psychologically and materially, and this issue is at the heart of the definition of national interest. Formulating foreign policy toward former constituent republics is much more difficult for the elite and the public than any other external affair.

For thinking about these and other issues of Russian foreign policy, any graduate seminar would benefit from the inclusion of the volume by Malcolm and others, and one can only hope that Oxford will issue the book in paperback so that students can afford to purchase it. While not providing any overarching social science analytical framework for why certain outcomes emerge or for how to weigh the role of different internal factors, this volume is extremely well researched and informative. The basic problem for the Russian elite is defining what Russia is and why, and then what to do about it. This book provides insight into the debates that have taken place within the elite, among the masses, and between civilians and the military on issues such as policy toward the West and toward the other NIS.

The book argues that within Russia there have been three basic groups, which the authors label the Liberal Westernizers, the Pragmatic Nationalists, and the Fundamentalist Nationalists, and that over time elite consensus has emerged around the centrist view. Malcolm and colleagues believe

that the Liberal Westernizers, such as Foreign Minister Andrei Kozyrev, were discredited over time for promoting Western-oriented policies that became viewed as exploitative of Russia, and President Boris Yeltsin, who held this view in 1992–93, moved to a more "pragmatic" position during 1994. The Fundamentalist Nationalists, whose most extreme proponent is Vladimir Zhirinovsky, have proposed the reintegration of the old empire and virulent anti-Western policies that are appealing to only a small segment of the elite and population. Thus, Malcolm and colleagues believe that a consensus around a more "realist" Russian foreign policy—good relations with the West but also some distance and influence but not control over the other NIS—is emerging as the driving force behind Moscow's approach.

After Margot Light traces the evolution of these three views and their manifestation in certain key Foreign Ministry documents and in the commentaries of Russian political observers, the succeeding chapters by Malcolm, Pravda, and Allison look at how these views are expressed by the key institutions in Russia, by political parties and public opinion, and by the military. There are excellent sections on the evolution of successive drafts of the Foreign Ministry's key concept paper (pp. 61ff), on the perspectives of the different political parties (pp. 172ff), on the role of the military (pp. 251, 264, 274–5), and on the problems of identity (e.g., p. 289). The chapters are uniformly well written and well researched.

One can certainly take issue with the definition of the three camps and with the authors' conclusions on how foreign policy has evolved. Since only the Fundamentalist Nationalists have an ideology, as Light herself notes (p. 43), "it is easier to detect an entire world-view in their discussions of foreign policy." One could also place the emphasis not on a general convergence to some centrist position but rather on a continuing Western orientation that has existed since 1987 despite a failure by the Group of 7 to deliver much of the economic assistance promised and despite policies such as NATO's intended expansion. Pravda's chapter on public opinion points to the relatively large consensus in the electorate against reintegration of the NIS and on good relations with the West. It is that continuing tendency and the public support for such policies that one might wish to emphasize and seek to understand in analyzing the post-1991 period.

The book shows convincingly that the elite and the public have not been able to come up with a coherent agenda for interacting with the former imperial possessions given tremendous uncertainty about what would best serve Russia's interest. Some individuals favor greater integration for security or economic reasons or for reasons of identity, but others oppose integration for the same reasons, and in particular due to the costs that would accrue. There has also been tremendous debate about the legitimate use of force, especially after the Chechnya debacle, although action in some areas, such as Tajikistan, has been viewed as necessary for Russian security.

The lack of attention to the internal divisions on this issue and to the importance of these divisions for identity formation and for the coherent pursuit of interests is the major shortcoming of the Petro and Rubinstein volume. This book, which is otherwise valuable as an undergraduate textbook, tries to get students thinking about the issue of continuity and change in Russian foreign policy. But as a textbook, it has two fundamental flaws that one hopes could be addressed in any succeeding editions and that are highlighted by the sections on Russian foreign policy toward the other NIS. One is ascribing to the Russian government a single foreign policy impulse toward the region, a myth which is effectively destroyed by the other two books reviewed here. Petro and Rubinstein argue, for example (p. 116): "By integrating all the CIS states into a customs union, Russia hopes to bind them closely to the Russian economy and to strengthen the strategic partnership between these states." Such statements fail to acknowledge both the lack of coherence in Russia's policy and the lack of implementation of CIS agreements.

The second problem is the use of terms better left to Russian imperialist tracts than to Western texts: "near abroad" and "the Ukraine." It is true that it is hard to find a good term for this part of the world. Not all countries are in the Commonwealth of Independent States (CIS). After a certain period, these New Independent States will no longer be new. Former Soviet Union applies to their origin but not necessarily to their trajectory. The problem with the term "near abroad" is that it is used by the Russian elite in ways that suggest a sphere of influence. Malcolm and others decided to use "near abroad" with the quotation marks throughout their volume, and that is preferable to simple use of the Russian terminology. Regarding "Ukraine," there is no excuse for the term in a book published in 1997; it is an independent state, no longer the frontier of the empire, and it should not be labeled "the Ukraine" in discussions of events after December 1991.

Otherwise, Petro and Rubinstein have a nicely organized volume. They present a concise account of the Soviet period, giving an overview of the revolutionary ideals, the nature of Stalin's foreign policy, and the contours of the Khrushchev and Brezhnev periods. In discussing each, they stick fairly close to the conventional wisdom on topics such as the nature of Stalinism and Soviet policy toward Eastern Europe under Khrushchev and Brezhnev, and this approach works well in a textbook. (Although in adopting the typical dichotomy between ideology and national interest, they forget that ideology helps define national interest.) Gorbachev is treated in a cursory fashion in the introduction, but he is discussed further in the more thematic chapters, which include policies toward Europe, Asia, the Middle East, the CIS, security and arms control issues, and international organizations.

Rather than cover foreign policy thematically across regions or issue areas, Skak takes the interesting tack of focusing on foreign policy in a postimperial space. Her discussion of the Soviet past concentrates on empire building, not general foreign policy, and her three post–Cold War foreign policy cases are Russia (the center), Lithuania (part of the inner periphery), and Hungary (part of the outer periphery). Skak knows her cases extremely well.

Thinking about the effects of empire on the foreign policies of the postimperial center and periphery could have made for an analytically interesting volume. But Skak tries to cover every potentially relevant concept, model, and theory in social science. In the first chapter she discusses anarchy, empire, praetorianism, corporatism, and consociational democracy, and she follows that with a chapter covering every model ever used to study the Soviet system. Having done so, she declares what has become obvious: "As the reader may have noticed I have tried to link myself to nearly all of the explanations mentioned here, not just totalitarianism. From the point of view of analytical clarity this sounds like a very frustrating outcome—a case of overdetermination, not to say explanatory overkill" (p. 134). Well said. Books should have a clear direction, with concepts, models, and theories serving to illuminate, not obscure. And the lack of a clear framework shows when Skak abandons a discussion of postimperialism in her treatment of Hungary. Having not developed a clear model at the outset, she has nothing to apply to that case. But

the reader who concentrates on her empirical material will be rewarded.

Malcolm and others and Skak provide rich material for area specialists and comparativists. With the development of political parties and movements, tensions between president and parliament, and the issues of postimperial development and the construction of national identity, Russia and the other NIS are interesting cases for thinking about general problems of state building and foreign policymaking. After all, how these states are even building the necessary institutions to manage complex economic and security foreign policy problems is an important subject for scholars of comparative politics and international relations.

Eisenhower and the Missile Gap. By Peter J. Roman. Ithaca, NY: Cornell University Press, 1995. 264p. $35.00.

Eisenhower, Kennedy, and the United States of Europe. By Pascaline Winand. New York: St. Martin's Press, 1996. 432p. $17.95.

Meena Bose, *Hofstra University*

Research on the Eisenhower and Kennedy presidencies has benefited greatly from the growing declassified records on both administrations in the past fifteen years. Based on these records, scholars have reevaluated initial appraisals of the two presidents' decision-making processes as well as their actual policies. Roman and Winand solidly contribute to this literature by reinforcing and at times raising new perspectives on previous analyses.

Roman's study of the missile gap focuses on the final years of the Eisenhower administration, from the launch of the Soviet satellite Sputnik in October 1957 to Kennedy's inauguration in 1961. The book examines the development of Eisenhower's strategic nuclear policies during this period, focusing both on their content and on the policymaking processes behind them. In so doing, it builds upon the literature on foreign policy decision making and bureaucratic politics as well as scholarship on the Eisenhower presidency.

As a whole, this book is well researched and will be of interest to students of Eisenhower and of American foreign policy. Roman focuses on four aspects of nuclear policy that became top priorities after Sputnik: intelligence assessments, national strategy (basic national security policy) and nuclear strategy (force posture and operational plans), defense budgets, and nuclear weapons choices. In examining these subjects, Roman presents a thoughtful and considered reappraisal of earlier studies. For example, scholars typically maintain that Eisenhower resisted pressures to increase defense expenditures after Sputnik because classified U-2 information clearly contradicted Soviet claims of nuclear superiority. Roman holds that the evidence actually was not so definitive, and while Eisenhower did limit defense spending, he also initiated the strategic buildup that typically is seen as originating in the Kennedy administration. These arguments provide a fuller picture than we have seen of Eisenhower's efforts in this period.

Roman's arguments about Eisenhower's leadership skills raise important points as well but cover familiar ground. One central claim is that while Eisenhower successfully kept defense budgets from escalating during his final years in office, despite strong disagreements within his administration, he did not effectively communicate his rationale to Congress and the public: "Eisenhower's success in the administration was not matched outside the administration" (p. 207). But other scholars have made this argument as well, notably McGeorge Bundy (*Danger and Survival: Choices About the Bomb in the First Fifty Years*, 1988, pp. 334–50), Greenstein (Preface, *The Hidden-Hand Presidency*, 1994), and Divine (*The Sputnik Challenge*, 1993). It is particularly surprising that Roman does not cite the latter work, though this is difficult to confirm because his book lacks a bibliography. The Divine and Greenstein sources may have been published after Roman's book went to press, but it is nevertheless evident that critiques of Eisenhower's political skills during the missile gap debate date back at least to the late 1980s. Roman's analysis explains the point well through a detailed study of post-Sputnik defense budget deliberations, but it does not present a new perspective.

In concluding that Eisenhower's nuclear policies leave a "mixed legacy" (p. 197), Roman continues a long-running debate. He finds that Eisenhower initiated efforts to develop second-generation strategic missiles and thus holds Eisenhower responsible for the "large, secure, and redundant strategic nuclear triad" (p. 193) of bombers, ICBMs, and SLBMs. But the massive buildup of strategic nuclear forces took place during the Kennedy administration, as Ball's *Politics and Force Levels* (1980) explains. Furthermore, Roman suggests that Eisenhower initiated these developments "just as fears about the existence of a missile gap were beginning to subside" (p. 200). Yet, the missile gap debate played a major part in the 1960 presidential campaign. If one accepts Roman's position that U-2 information in the late 1950s was inconclusive, then it becomes even more evident why Eisenhower decided to build up strategic forces. Given the political pressures he faced, it is indeed surprising that he limited the buildup as much as he did. Roman's criticisms of Eisenhower's policies raise the question of what the president might have done differently.

A few minor points about the organization of the book should be noted. Charts outlining the changes in defense budgets and nuclear weapons between 1957 and 1961 would have helped illustrate the detailed analyses of each. The introductory chapter should give some indication of the author's thesis or discuss more explicitly the questions that guide the analysis. Instead, it contains a lengthy review of the literature on presidential power, bureaucratic politics, and Eisenhower, much of which could have been condensed. The author's own interpretations of nuclear policy and decision making during Eisenhower's final years in office are of most interest, and those views merit a higher profile.

Winand's book examines European integration efforts during the Eisenhower and Kennedy administrations. It uses both archival research and elite interviews to produce an exhaustive study of such subjects as the European Defense Community, Euratom, the Common Market, and the Multilateral Force. Material from European archives along with the author's personal interviews especially provide new and authoritative information on European-American relations in this period.

Given the ambitious scope of the project and its length, it is surprising that the study does not contain more of an analytic structure. Winand's purpose is to portray "the American administration's attitudes towards the unification of Europe, and the continuous dialogue between American supporters and opponents of European integration, and their European counterparts" (p. ix). This subject merits attention, Winand explains, because it "will help contribute to a better understanding of how patient and determined individuals bring about such changes" (p. ix). Yet, the book provides no conclusions about which individuals were most important in this period and why. Indeed, it lacks a real concluding chapter, as the epilogue discusses how the Johnson administration addressed efforts begun under its predecessor.

Winand acknowledges from the outset that the study may contain "an unnecessary wealth of detail" (p. xii) but maintains that this information is needed "to provide a more lively and accurate account of events" (p. xii). Her point is well taken: The difficulty is not the amount of detail but the lack of a framework to review and assess it. At the very least, the number of European organizations and initiatives examined merits a timeline to help the reader keep track of when each proposal is presented. Individual chapters could use introductory and concluding sections that explain what each will examine, why the subject is important, and the author's conclusions about the initiative and U.S. and European actions. In addition, the introduction needs to provide a road map for the book as a whole.

Perhaps the greatest strength of the book is its careful illustrations of the policymakers involved in European integration efforts. Winand devotes an entire chapter to explaining the views of Kennedy administration officials, including George Ball, Walter Rostow, and McGeorge Bundy, and her meticulous descriptions provide a useful basis for understanding the administration's actions. A parallel chapter on Eisenhower administration officials would have served the book well. Winand also examines the views of such European leaders as General Charles De Gaulle and Jean Monnet, but these studies appear within the substantive chapters on specific initiatives rather than separately. A chapter on each U.S. administration's top officials and their European counterparts might have provided more easily accessible references. Of the specific initiatives examined, the chapter on the early efforts of the Roosevelt and Truman administrations is especially instructive, providing useful background material for the chapters that follow.

This study does not attempt to provide a theoretical argument for understanding European integration efforts, perhaps because Winand aims to address not only students and researchers but also "a larger public outside of academe" (p. x). But readers who are not experts on the subject will want to see more forest in this work for all the trees. For example, the book is particularly timely because of current initiatives for European monetary and political integration. Winand herself notes that "Washington has dusted off the idea of a partnership between the United States and a tightly integrated European Community, which Kennedy advocated some thirty years ago" (p. xvi). Yet, she does not provide a fuller explanation of how efforts today are rooted in initiatives from the 1950s and 1960s. It is unfortunate that the book does not include such broader analysis, for it is a fine study that contains much fascinating detail.

Both of these books will serve as valuable reference tools for scholars studying or teaching about these subjects. As such, they provide important contributions to the burgeoning literature on American foreign policy during the Eisenhower and Kennedy administrations.

The Once and Future Security Council. Edited by Bruce Russett. New York: St. Martin's Press, 1997. 179p. $39.95.

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The end of the Cold War has renewed serious attention to the issue of the reform of the United Nations Security Council (and indeed the United Nations in general). Various proposals have been advanced to change the decision-making powers or structure of the Security Council, although the greatest attention has been directed to changing the composition of its membership or altering the status of the five permanent members. Proposals include increasing the council from its current 15 to various membership sizes of 20–30 as well as initiatives to give permanent membership (and veto power) to Japan, Germany, and/or some Third World state. This slender volume from the United Nations Studies program at Yale University brings together eight chapters of essays and empirical analyses that deal with various aspects of reform proposals for the Security Council. Although four chapters have appeared in earlier versions in leading journals in the profession, the collection does make a value-added contribution by the additional four chapters and the coherence of reading the chapters together and in sequence.

The opening chapter by James Sutterlin provides a historical overview of politics and values underlying the creation of the Security Council in the 1940s (and beyond). There is little new here for scholars of international organization, but this chapter provides a vital baseline for understanding the current parameters of the Security Council. The second chapter, by Bruce Russett, provides another kind of baseline, offering ten "balances" on which to weigh various UN reform proposals; for example, the balance between enforcement and neutrality is especially critical as the UN has seemingly lurched from one extreme (neutrality) in its early peacekeeping efforts to the other (enforcement) in contemporary manifestations of peacekeeping. The only problem with this chapter is that several of the balancing criteria seem more applicable to UN reforms other than those involving the Security Council.

The edited collection also strays a bit from its central theme in the third chapter, an empirical analysis of voting patterns in the UN General Assembly by Soo Yeon Kim and Bruce Russett. Except for a brief discussion of the Security Council at the end of the article, it is not always clear why and how this chapter is relevant for reform of the Security Council. Nevertheless, this chapter and its successor, by Barry O'Neill, are representative of the revival of a solid tradition of UN voting studies that had become almost moribund over the last twenty years in the scholarly community. Kim and Russett's results provide new knowledge to the field about the relevant clusters (mainly a strong North-South cleavage) that characterize post–Cold War voting patterns at the United Nations; previous analyses of General Assembly voting patterns are all now obsolete. O'Neill's is a superb analysis of the implications of various voting schemes for the Security Council. He looks not at raw voting power but at power over outcomes and satisfaction with outcomes. There are a number of counterintuitive conclusions (i.e., expanding the number of nonpermanent members is not likely to make any difference in the council) that illustrate the synergy which can occur between good theoretical and policy-relevant research.

The fifth chapter, by Nigel Thalakada, looks at China's voting patterns in the Security Council from 1990–95. Although China has not exercised its veto power since 1989, it has an unusually large number of abstentions since then, and even its support of UN resolutions has often been qualified. Thalakada's analysis helps us understand these behaviors, although China's narrow views of sovereignty and quest for Western aid and investment may not account for all its actions or necessarily give us clues about its future behavior. Masayuki Tadokoro provides a Japanese view of Security Council restructuring, but this is more a reflection of the author's heritage than a viewpoint necessarily representative of the government or people of Japan. He proposes four changes to the Security Council (such as new veto members and broader focus) that are an amalgamation of many of the suggestions offered in various political circles and therefore not necessarily new or innovative. Ian Hurd's following

chapter is a welcome move away from an almost exclusive focus on voting structure and patterns. Hurd emphasizes the "informal" aspects of Security Council decision making. Among other points, he argues that there has been a blurring of the distinction between members and nonmembers of the council and that extensive consultations among a range of states occur; this further suggests that merely expanding the number of seats on the council may not change the form or outcomes of council deliberations.

The concluding chapter, by Russett, Sutterlin, and O'Neill, lists six suggestions for changing the Security Council. Read on their own, these ideas seem remarkably limited in ambition and scope. Yet, in the context of the preceding chapters, they seem eminently reasonable and realistic, and they may find their way into practice in the not too distant future.

For one-stop shopping on Security Council reform, this is a handy little book. Unlike most edited collections, it is more valuable if one reads all the chapters rather than one or two highlight chapters. The staying power and importance of the collection is likely to be limited, however, especially if the Security Council reform advocated by the authors actually occurs and thus renders moot the issues and discussion.

The Endangered Atmosphere: Preserving a Global Commons. By Marvin S. Soroos. Columbia: University of South Carolina Press, 1997. 339p. $39.95 cloth, $19.95 paper.

Marian A. L. Miller, *University of Akron*

The Endangered Atmosphere is an intelligent contribution to the discussion of the regulation of the global commons. Marvin Soroos places his examination of the atmosphere within the context of the commons, and he analyses it through the lens of security policy. His study underscores the crucial role of international regulation in addressing atmospheric degradation, and it also highlights the complex and untidy nature of the regime process, complicated as it is by considerations of economics, efficiency, and equity.

The book is organized around an examination of the regimes for atmospheric testing of nuclear weapons, long-range transboundary air pollution, the depletion of the ozone layer, and global climate change. These cases are preceded by a primer on atmosphere and pollution and a discussion of scientific knowledge. With its information on the characteristics of the atmosphere and the nature of the pollution threats, the primer is intended to help readers understand the related policy issues. The discussion of scientific knowledge includes an examination of the development of scientific cooperation and the role played by international scientific programs in each of the four issue areas.

The examination of the four cases is followed by an exploration of the commons issue and a treatment of environmental security. The commons chapter discusses the atmosphere as a global commons and assesses the extent to which the atmosphere matches the commons described by Garret Hardin ("The Tragedy of the Commons," *Science* 162 [December 13, 1968]:1243–8). The succeeding chapter places pollution concerns in a security context and assesses how this context may affect the strategies people use to address atmospheric degradation.

Soroos points out that although the atmosphere cannot be properly classified as common property, it is a common resource. His examination of the four cases within the larger integrative framework of the atmosphere provides us with an enhanced understanding of the factors driving the degradation and preservation of the global commons. His organization of the cases allows for examination of the regimes along the following general lines: historical context, emergence as global or public issue, creation and evolution of the regime, compliance, and effect. The organization facilitates useful comparisons of the various environmental problems and public policy responses. It also enhances observation of the science and policy interrelationships among the cases. In addition, the chronological treatment of the cases offers the reader a sense of the learning curve in the development of atmosphere-related regimes.

It would have been useful to see some small additions to the treatment of the climate change and the transboundary air pollution regimes. While the evolution of the climate regime is clearly presented, the examination of the role of economic actors is limited. Since global warming is substantially driven by industrial development, corporate actors have a major stake in the evolution of that regime. It would have been illuminating to have an assessment of the extent to which they are likely to shape the regime's evolution. Some corporate actors, like insurance companies and petroleum corporations, have particular stakes in this issue. As an atmospheric issue, transboundary air pollution is properly included with the other cases. All of these cases fall within the overall commons framework of the atmosphere, but they are not all commons problems. The analysis would have been deepened by an assessment of the effect of the transboundary characteristics of air pollution on the evolution of that regime.

As Soroos considers the characteristics of the global commons, he focuses on two tragedy-averting strategies: voluntary restraint and regulatory regimes. He examines state actions that seem to contradict Hardin's position questioning the viability of voluntary restraint. But he suggests two explanations for these unilateral commitments: public pressure from citizenry and nongovernmental organizations and attempts to influence other states to act in a similar fashion. He emphasizes that voluntary restraint is not a dependable path for addressing transboundary or global environmental problems. International regulation, adopting features such as prohibitions, limits, operational rules, and economic disincentives, is the more effective option.

Over the last decade, the term "environmental security" has become an important part of environmental discourse. Soroos adds to this by providing a lucid discussion of the choices available to states as they seek to reduce environmental threats or vulnerability. A focus on threat leads to a preventive strategy, while a focus on vulnerability is likely to result in a defensive posture. States also have the option of acting unilaterally or collectively. This security scenario results in four strategic options: self-prevention, collective prevention, self-defense, collective defense. He uses the prisoner's dilemma to provide some insight into the strategic choices states make, applying it to each of the four cases. He warns of the danger inherent in states addressing environmental security in a fashion similar to the way they have addressed military security. The resulting strategy of self-defense would, indeed, lead to a tragedy of the commons.

The concluding chapter includes an intriguing discussion of the prospects for a comprehensive Law of the Atmosphere. Currently, there is no overarching law addressing issues such as jurisdiction over the atmosphere and users' rights. Unlike the oceans and outer space, the United Nations has not attempted to address the atmosphere in a comprehensive manner. Soroos explores the reasons for this and concludes that the current flexible, problem-specific approach has served the global community well. He argues that it was more important to focus on the development of regimes dealing with specific problems than to spend time developing a comprehensive legal regime for the atmosphere.

Soroos has provided a useful exploration of the issues relevant to the preservation of the atmosphere. This book is recommended reading for at least two reasons: The integrative examination of atmospheric degradation allows us a clearer understanding of the relevant challenges and policy responses, and the discussion of states' strategic options illuminates some of the factors that drive state responses to global environmental threats.

Political Realism in International Theory. By Roger D. Spegele. Cambridge: Cambridge University Press, 1996. 284p. $59.95 cloth, $19.95 paper.

Robert M. A. Crawford, *University of British Columbia*

It has become almost customary to apologize for approaching the problem of theory in international relations from a philosophical perspective. First among its many merits, Roger Spegele's *Political Realism* avoids this annoying and gratuitous practice. Spegele admits up front that his book is a second-order inquiry—an examination of "the examiners of the world rather than the world itself" (p. xiii). If this is enough to make some readers give it a pass, then the usefulness of Spegele's book will already have been confirmed, since an impatience for the "real business" of world politics discloses the preclusive but erroneous assumption that we already know how, and why, to study it.

In the genre of disciplinary thinkpieces to which Spegele's book stands refreshingly opposed, international relations (IR) is conceived as a monistic science for which there can be only "one correct methodological approach" (p. 88). That this quest for methodological and moral consensus has foundered says less about the actual state of IR than the failure of so many of its practitioners to recognize the patent absurdity of founding so manifestly a political subject on any sort of predefined consensus. As Spegele suggests, the surest way for a science to fail is to set standards it cannot possibly meet. But his rejection of the continued application of monistic scientific principles is also based on his view that they constitute only the most prominent of three inconclusive conceptions of IR. Spegele labels these "positivist-empiricism," "critical/emancipatory," and "evaluative political realism." What he is able to offer, with considerable persuasion, is reasonable grounds to reject the adequacy of the first two approaches and reasonable grounds to accept a revised conception of "commonsense" realism as the only viable and appealing version of IR on offer. It is this defense of evaluative political realism that makes Spegele's book much more than just another antipositivist tract.

The book is divided into two parts, the first of which offers a "first cut" at evaluative political realism. It is focused mainly on identifying key elements of the positivist-empiricist and emancipatory approaches, with a view to exposing their inability to form any conception of international theory that we can use. Positivist-empiricism, Spegele argues, is just formalistic "speculation," rather than a theory suited to our comprehension of "the life that we share with others in our human groupings" (p. 98). Critical/emancipatory theory, meanwhile, fails to recognize that all idealized versions of humanity are ultimately assimilationist, since the world is composed of human beings, societies, and nation-states, and these are always situated. It is in the latter half of the book that Spegele brings his argument for a revised version of traditional realism to fruition.

Spegele's enterprise reads like a bit of a theoretical reclamation project to the extent that he seeks to liberate the realism of E. H. Carr, Reinhold Niebuhr, Hans Morgenthau, Martin Wight, Raymond Aron, and Herbert Butterfield (among inestimably many others) from the damaging clutches of neorealism or—as Spegele prefers to call it—"concessional realism." By concessional Spegele means a version of realism that shares with "commonsense" realism its concern with sovereign states interacting as potential or actual adversaries, but concedes "the validity of the principal methodological claims of positivist-empiricism" (p. 15). The primary and paradoxical defect of concessional realism is that the very act of cementing its scientific credentials makes realism less real, in the sense that it precludes its ability to evaluate the human activities and practices that traditionally are its stock-in-trade. Spegele thus reaffirms the traditional realist idea of practice as ethical knowledge and the traditional understanding of theories as "conceptual capacities we can do things with" (p. 93). But in an important and perhaps understated sense, Spegele also needs to save commonsense realism from itself. Much of the seeming irrelevance of realism can be attributed to its aversion to any form of philosophy of science, a natural offshoot of its commitment to the irredeemable pluralism of interstate relations. This disinclination to philosophical self-analysis helps create the erroneous view that dramatic changes in world politics, and the seemingly waning fortunes of the state, must portend the diminishment of realism as well. Spegele's self-described objective, then, is to remind realists that theirs is a critical-reflective enterprise that understands the relationship of practice to theory not in terms of universal human interests and aspirations, but in terms that are always local. If this appears to point us toward relativism, it is only because we have failed to understand that theory can and must be a meaningful, critical device to evaluate and sort ends into better or worse, but it cannot do so in any way that is acceptable to all.

There is much to recommend this book, more perhaps than Spegele is disposed to presume. While he can hardly be blamed for not promising to make sense of the widening array of paradigms, positions, and discourses that sometimes pass for debate in the increasingly fragmented and tentative discipline of IR, the process of identifying rivals to his own favored conception of the subject does much to help us find our way about. To be sure, his discussion of many of the subtypes of positivist-empiricism and emancipatory theory are far from comprehensive, but this is not part of his overall purpose. What matters is that Spegele's ordering principles are useful, and persuasive, and his treatment of every perspective appropriately critical. Because Spegele has his own unambiguously favored conception of IR on offer, his balanced treatment of rival perspectives is all the more impressive, though he cannot resist the occasional acerbic dig. In one of the more interesting and original contributions in the book, Spegele is also able to find convincing support for evaluative political realism in the unlikely domain of Lyotardian postmodernism. Such is but one example of Spegele's admirable ability to make sense of international relations while resisting a truncated conception of where we can legitimately locate the subject, and to make a forcefully partisan argument for realism without collapsing into narrow dogma. Nevertheless, Spegele pulls no punches in his searching and telling critique of positivist-empiricism and emancipatory theory, and he seems to relish the prospect that his book will generate controversy.

On the whole this erudite and well-crafted study lives up to its advance billing as a coherent, viable, and attractive defense of realism. It also offers a timely middle way between the mutually unappealing extremes of rank rationalism and open-ended relativism that now appear to define the dis-

course. There are, however, problems with the book as well, the most notable of which pertains to how (or if) we are meant to situate it in the broader realist revival presently under way. Though Spegele's prime objective is explicitly revisionist, the seeming novelty of his position as an evaluative political realist threatens at times to obscure the sense of continuity and tradition that remains essential to his argument. As a result, Spegele leaves himself vulnerable to misunderstanding and invites us to wonder if he is trying to put a kinder gentler face on realist theory while shying away from its less palatable associations with realpolitik. It is worth noting, at least, that Spegele takes the moral high ground on more than one occasion in his condemnation of barabarities in the name of emancipation. He does admit, however, that—whatever its aspirations to achieve moral ends in particular contexts—evaluative political realism acknowledges the "possibility of moral and political tyranny" (p. 101). In the 1980s an English pop group called The Smiths titled one of its albums "Barbarism Begins at Home." Is it possible that Spegele has something similar in mind? Despite an otherwise superbly crafted argument, he could do more to help us answer this question, or at least make sure that it does not arise.

Recasting the European Order: Security Architectures and Economic Cooperation. By James Sperling and Emil Kirchner. New York: St. Martin's Press, 1997. 287p. $69.95 cloth, $22.95 paper.

Carolyn Rhodes, *Utah State University*

Anyone wanting to understand the character of political-economic and security structures in post–Cold War Europe should read this book. It is a richly detailed, carefully written examination of existing cooperative arrangements in military security, trade, finance, and environmental affairs. Newcomers to the subject will find the treatment somewhat dense since the book goes into considerable detail about past and current experiences and assumes a certain level of previous understanding. Still, I would recommend it to students because of the strong historical foundation that is laid for each issue. Experts will find the book's approach very interesting and well considered, encountering a number of useful insights about the current state and future direction of European security. What the book does not do is attempt to advance any kind of overarching theoretical model, even though the authors embed their analysis in prominent existing literature.

This book is predicated on the assumption that in the post–Cold War period the evolution of the concept of security has outpaced the ability of existing architectures to adapt to its wider definition. That definition, the authors compellingly argue, must take into account economic security, which is shaped by trade, investment, and environmental relations and which requires very different policy approaches compared with narrower definitions of military security. The book's rationale seems to have been driven by the recognition that post–World War II structures—from NATO to the IMF—must be rationalized in both purpose and function to meet the demands of post–Cold War political-economic security concerns. At the heart of those demands is the effort to realize the peaceful transformation of the countries of Central and Eastern Europe (CEE). Therefore, underlying the authors' larger premise that European security architectures must be reconsidered is the view that "the stability of the post–Cold War European security order depends upon the successful societal transition of the former states of the Warsaw Pact, namely the transition to the market economy and competitive multi-party democracies." (pp. 202–3)

This emphasis upon the role of the CEE in the future of European security reveals how pivotal the region has become in defining where Western European interests lie and where Western European, U. S., and Russian interests overlap and diverge. Keeping this emphasis in mind, it would be misleading, however, to characterize the book as being driven solely by consideration of CEE futures. In fact, the book consists of a broader view of European security concerns that looks critically at the major institutional arrangements that currently exist in both the military realm of security and in the economic/environmental realm of security. Still, the use of the CEE situation to highlight interdependent concerns and institutional capabilities brings into relief how relevant the book's thesis is regarding broader definitions of security in the post–Cold War era.

The authors observe that post–Cold War circumstances have "diminished the force of anarchy . . . [and] enhanced the 'interaction capacity' of the European state system" (p. 16), which has created demands for new approaches to security and new opportunities for cooperation. As they systematically review and assess the major institutions designed to cope with these demands, however, it becomes clear that the myriad arrangements involved, including the major ones such as NATO, OSCE, EU, WEU, IMF, G-7, GATT/WTO, EBRD, and IAEA, all have limitations that have historical, constitutional, and budgetary origins. Furthermore, the authors make it clear that despite some success in international cooperation under these arrangements, individual states have tended to place domestic interests ahead of international cooperation. This is well illustrated in the macroeconomic policy arena, in which Germany and the United States often pursued monetary policies that worked at cross-purposes. It is evident as well in the military sphere; despite considerable lip service about cooperation, the United States wants burden sharing in NATO without having to forfeit decision-making supremacy, and EU member states profoundly disagree about the specific role that the WEU should have both within the EU and as the EU's defense pillar within NATO. While the authors recognize that the potential for future coordination is there, they observe that the desire for cooperation and unity of purpose remains thwarted by concerns over national sovereignty.

This may be especially problematic if the countries of Central and Eastern Europe are not successfully transformed into democratic market economies and integrated into Western cooperative structures. Making the point that the peaceful achievement of these goals is of vital concern to the economic, environmental, and military security of the West, the authors argue that such an outcome is not a foregone conclusion. Differences among the major players in the European Union, differences between the United States and European states, and reactions by Russia could all serve to thwart this goal. While the authors did not have the benefit of knowing about recent decisions regarding NATO enlargement or about the EU's Amsterdam Treaty, their assessment is still valid, and their words of caution remain relevant.

The Retreat of the State: The Diffusion of Power in the World Economy. By Susan Strange. Cambridge: Cambridge University Press, 1996. 218p. $49.95 cloth, $16.95 paper.

Stephen J. Kobrin, *University of Pennsylvania*

Susan Strange ends this provocative and thoughtful book with a reference to "Pinocchio's problem." Not his nose

growing when he lied—a metaphor better left alone—but the lack of strings to guide him once he turned into a real boy. The diffusion of state power, and a more general systemic *entropy* of authority, has left us with "a ramshackle assembly of conflicting sources of authority" (p. 199). We all share Pinocchio's problem: The strings that bound us unambiguously to state and nation have snapped, and we are left adrift in a world of multiple authorities, allegiances, loyalties, and identities.

Strange's central argument is that the authority of all states is diminished as a result of changes in technology and finance and of the integration of national economies into a global world economy. In part, authority has shifted to non-state actors, most notably transnational corporations (TNCs). Yet, some authority in the system has been "lost" in the sense that it has shifted from states to impersonal markets; some of the fundamental responsibilities of the state are not being exercised by anyone. "The diffusion of authority away from national governments has left a yawning hole of nonauthority, ungovernance it might be called" (p. 14).

The state is not disappearing; Strange does not argue for the "end" of anything. She does argue forcibly (no surprise here!) that the state is in the midst of significant change—a "metamorphosis"—brought on by structural change in the world economy. It can no longer make exceptional claims; it is now one source of authority among several.

The Retreat of the State follows logically from *Rival States, Rival Firms* (1991), in which Strange and John Stopford argued that competition for global market shares has replaced competition for territory. Here, the shift in authority from states to markets is seen as "probably the biggest change in the international political economy to take place the last half of the twentieth century" (p. 43). At this point, "there is not much left of the territorial basis of authority" (p. 45). Territoriality is neither the sole source of political power and authority nor a critical factor determining the prosperity of national society.

In a sense, Strange is "taking the state back out." While she only alludes to premodern political structures, she clearly looks both back ("once more and as in the past," p. 73) to times when multiple authorities (e.g., church, emperor, prince, and lord) coexisted and when multiple political loyalties were the norm. She believes that international relations and other social sciences must transcend state centric theory and analysis: the "colonization" of the study of the study of politics by the state (p. 32).

Thus, power is "the ability of a person or group of persons to so affect outcomes that their preferences take precedence over the preferences of others" (p. 17). Power should be seen in terms of relationships—as a social process affecting outcomes—rather than in terms of resources or capabilities. One can then ask how and by whom values are allocated and decisions taken to affect outcomes. And Strange's answer is—in large part—economic actors and impersonal markets.

TNCs, for example, are not "powers behind the throne" but "political institutions having political relations with civil society" (p. 44). As Strange sums the three major premises of the book, politics is no longer confined to politicians and officials; power is impersonally exercised by markets; and authority is legitimately exercised by authorities other than states. After developing her argument, Strange looks at five examples of "authority beyond the state": telecoms, organized crime, insurance, the Big Six accounting firms, cartels, and international organizations. While the case studies are interesting, if somewhat superficial, they leave me with the old Scotch verdict of "not proven."

The idea that the growth of markets can compromise state authority is certainly not new; Charles Lindbloom (*Politics and Markets*, 1977) made the point in considerable detail twenty years ago. The question here is whether recent developments have transformed changes in degree into changes in kind: whether the shift of authority from states to markets, from governments to private actors, is marginal or systemic. Strange clearly believes the latter. What is new this time around is that all states are undergoing change in the same relatively short period. She notes that the last time this happened was the medieval to modern transition from feudal agriculture to capitalist industry. While I happen to agree, the question is whether the cases support the argument. Of that, I am not so sure.

There is no question that the global spread of enterprise, changes in technology, and American deregulation have resulted in major changes in the telecommunications industry: in pressure for privatization of PTTs, the opening of markets to competition, and the emergence of international groups of alliances. States have lost power over the sector.

There is no question that two functions critical to a capitalist economy are largely in private hands: insuring against risks and auditing accounts. I am not convinced, however, that the evidence presented here adds up to strong support for Strange's arguments. These cases are not the best examples of structural change in the power and authority of states versus other actors in the system; of states being rendered as one among many alternative sources of authority.

Two minor quibbles with these cases may be noted. Political risk insurance did evolve from Lloyds coverage of ships at sea against hazards such as piracy. What happened in the 1930s, however, is that after Fascist bombing of Spain, Lloyds realized that land-based war risk could be serious, and it decided that neither the company nor anyone else would take them on. While the Big Six global accounting firms may have considerable power as a group, auditing has become a commodity as a result of digitalization, and there is fierce competition among them; their profits, "which might otherwise go to the shareholders" (p. 143), increasingly come from other sources.

Strange is on much more solid ground when she considers cartels and asks why this phenomenon has been virtually ignored by academics in the last quarter century. (While Paul Samuelson may have ignored cartels, I do suspect that he would be surprised to be placed on the political right.) The strongest potential argument here is not well developed, however. At this point, "cartels" are implicit rather than explicit; they reflect dense oligopolisitic concentration resulting from dramatic increases in the cost, risk, and complexity of technology rather than an ex ante attempt to collude. The best example is commercial airframe manufacture, the world market for which will only support two huge players: Boeing and Airbus. Semiconductors and telecommunications are other sectors in which the number of viable players is limited by the size of a competitive R&D budget relative to the size of the world market.

The fact that in many of these sectors the scale of technology is mandating cross-national alliances among the largest players (e.g., IBM, Siemens, and Toshiba in semiconductors) exacerbates the problem. If size and concentration result from structural change, and the capabilities reside in transnational alliances, then power may really be shifting to actors beyond the reach of the territorial state.

One glaring omission is the absence of any discussion of the literature on transnational actors which has developed similar arguments—albeit in a very different context. Neither early efforts (i.e., Robert Keohane and Joseph S. Nye, Jr.,

eds., *Transnational Relations and World Politics*, 1971) nor more recent work on the emergence of global civil society are even cited.

I have one last qualm. While it may well reflect my own national bias—globalization only goes so far even for us believers—I think that Susan Strange has more than made the point by now that arguments about hegemonic decline are a mask for the incompetent use of American power and that international organizations tend to serve American interests. How about a truce?

The critical question is how the world economy—and the international political system—will cope with the diffusion of state authority; the spread of power to other actors; and more important, the vacuum that has arisen as the sum of authority gained by other actors is less than that lost by states. "How much disorder can the world market economy tolerate before it starts to unravel, wind down, fall apart" (p. 191)?

As Strange notes, international political economists are split between the "nothing has changed" and the "maybe a lot has changed" views of globalization. Given even a slight possibility that globalization entails a major systemic change in economic and political governance, however, it is irresponsible to ignore both the phenomenon and its consequences for our societies.

In this uneven but important book Strange raises a number of critical questions intelligently and provocatively. Reflecting her earlier life as a journalist, it is very well written and intellectually coherent. *The Retreat of the State* is rich in detailed argument and discussion and covers many more topics and subtopics than I can mention here. This book deserves a very wide readership among those interested in where the modern territorial state and the modern state system are headed as we enter the twenty-first century.

Democracy, Equality, and *Eidê*: A Radical View from Book 8 of Plato's *Republic*

ARLENE W. SAXONHOUSE *University of Michigan*

A Plato opposed to democracy fills the literature, and while some scholars question whether Plato adequately captures Socrates' possibly favorable views of democracy, Plato himself remains a paragon of elitism. I argue that Plato's response to democracy is far more theoretically interesting than simple disdain for the unenlightened masses. Rather, in Book 8 of the Republic *he explores the fundamental tensions of a regime identified with freedom and equality, which he presents as characterized by formlessness, and the epistemological and theoretical problems posed by the absence of forms* (eidê). Eidê *give structure and identity to regimes and to their citizens; they are necessary for intellection and philosophy, but they are also the grounds for compulsion. Plato's analysis of democracy thus becomes a more serious challenge for democratic theorists than previously recognized.*

An elitist Plato, opposed to democracy and hostile to the masses, fills the literature. In the midst of an extensive philological and grammatical commentary on Plato's *Republic*, James Adam (1902, 2.24, *ad loc.* 494a) includes the following brief but telling observation: "The theory of Ideas is not a democratic philosophy." He writes this in response to an interchange between Socrates and Adeimantus concerning the access of the many to the idea or form of the Good, during which Socrates claims: "It is impossible for the multitude to be philosophic." Only a few will have access to the forms (*eidê*).[1] A profound inequality of rule and authority seems to follow from that unequal access. I could begin with Adam's assertion that the theory of ideas is not a democratic philosophy, but the basis for my argument derives from a very different perspective, an epistemological one that has nothing to do with the capabilities, or lack thereof, of the many to attain a vision of the Good. Rather, I focus on the theory of the forms as a mechanism for categorization, opposing epistemologically, politically, and psychologically the openness of democracy. While Adam and numerous others see elitism in the Platonic theory of the forms because the many cannot ascend to a philosophic vision,[2] I attend to the opposition between democracy and that theory to illustrate how Socrates' discussion in Book 8 of the *Republic* points to democracy's dependence on a "formlessness" that challenges claims of equality and of identity within democratic regimes.

The epistemological critique of democracy that derives from the theory of the forms points to very different challenges than those that motivated the judgment by Adam and others that the theory of the forms is not a democratic philosophy. For them, a hierarchy of intellectual skills justifies a hierarchy of political rule, and since the highest level of intellectual skill is required for an apprehension of the forms, a regime that distributes power equally to those who can ascend and those who cannot must fail. The parable of the boat from the beginning of Book 6, for example, captures this argument vividly. There, the somewhat deaf shipowner of limited vision who knows little about seafaring allows himself to be drugged by the quarrelsome sailors. Though they never learned the skill of navigation, they are eager to control the ship. Meanwhile, the true pilot is scorned as a useless "gazer at the heavens" (488a–489e). The discussion of democracy in Book 8 does not address that issue at all. Rather, it presents democracy as a regime that in its insistence on freedom and equality is a regime of formlessness, one that lacks *eidê*. The theory of forms insofar as it is explicated in Book 6 is in part the basis for our capacity to categorize—to recognize similarities and differences so that we can distinguish one person or object from another and recognize as well what unites them. On this rests the foundation of mathematics, our capacity to count, to add, to subtract, and our capacity to discriminate, to separate the good from the bad, the noble from the base, the citizen from the noncitizen.

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The original version of this paper was written while I was a Fellow at the Center for Advanced Study in the Behavioral Science. I acknowledge with gratitude the support of the center's staff and of the Andrew W. Mellon Foundation, which helped fund my tenure there. Peter Euben, Jill Frank, Stephen Salkever, Jacqueline Stevens, and Judith Swanson offered helpful comments along the way. Thanks are due as well to the anonymous reviewers for this journal.

[1] *Eidê* (sing. *eidos*) in the Homeric epics and later Greek literature often means simply that which is seen, the shape or form of something; it came also to mean "class" or "kind" of object. In the *Republic* and elsewhere in the Platonic corpus, the term refers to an immutable shape or form of a thing or value (e.g., Justice or the Good) that is accessible to the intellect only by abstracting from the senses and sensible particulars. In Book 6 (as discussed below) the form of the Good is the highest object of the philosophic soul's search, but *eidê* are more general than the limited repertory of value terms, such as the Good and the Beautiful. They help typologize (as in Book 8) and organize our sense experiences by rising above particulars subject to observation by the senses.

[2] Jowett and Campbell (1894, 3.281), for example, commenting on the same passage, reflect at length: "The opposition of the few and the many is almost as great in the reading age of the nineteenth century as in the hearing age of Socrates and Plato. In politics, in society, in the realms of thought and imagination, there are two classes . . . the inferior minds and the superior: those who are under the influence of the hour, and those who have character." They moderate these claims a bit by noting that the opposition "is not so entire and absolute as Plato seems to assume."

Democracy in its openness in Book 8 lacks this capacity for adding and for discriminating and thus lays out the tensions and dangers inherent in regimes founded on formlessness and on principles of equality. Claims of equality necessarily entail claims of inequality, of who is not equal. *Eidê* enable us to typologize, to define equal and unequal, but *eidê* also can tyrannize. The openness of democracy is the escape from that tyranny, but at the same time it may leave us lost without the grounds to make choices or structure the world in which we live. Thus, the epistemological critique of democracy in Book 8 points to the tensions underlying current debates concerning "identity" politics and the "politics of difference." In a contemporary world that cares deeply about equality, the formlessness at the heart of democratic principles creates profound contradictions about how to implement such claims. Likewise, contemporary demands that identity, a self-assertion of form, be acknowledged stand in tension with the openness of democratic "formlessness." The freedom we and Socrates in Book 8 associate with democratic regimes entails the rejection of tyrannizing *eidê*; but we cannot function either politically or intellectually without *eidê*. Socrates' examination of democracy as a regime of formlessness helps us understand the limits and contradictions of claims of equality and identity in democratic regimes—of an equality that effaces the *eidê* and of an identity that entails the assertion of *eidê*.

Book 8 traditionally stands as the book that traces the decline of regimes, but attention to that aspect has led scholars to ignore the equally strong focus that Plato places on typology, on the five forms (*eidê*) of regimes and their human counterparts.[3] Socrates traces the movement from aristocracy to tyranny and the parallel personalities, how each one comes into being (the genetic analysis), but he also identifies the different *eidê* of political regime and how we can distinguish one from the other (the eidetic analysis). Among the *eidê* of regimes is Socrates' bizarre description of democracy, one that seems to have little to do with Athenian democracy as practiced in the fifth or fourth century B.C.[4] or with the mechanisms of self-government (the sharing of rule, isonomy, the assembly, the juries) that we may associate with the institutions of democracy past and present. Democracy is instead a regime of freedom and a radical equality arising from the absence of *eidê*, the very concept that controls the structure of Book 8. In Socrates' elaboration, this means that democracy, according to its fundamental principle of freedom, is the regime that is incapable of introducing typologies into the epistemological and political realm. The discussion in Book 8 thus subverts the book's apparent intention to give forms to regimes—as well as perhaps the philosophic explorations and claims of the previous books. A democracy true to its principles of equality and freedom must resist the tyrannizing of *eidê*, the boundaries and limits that define citizenship and the relations of parts. It must resist the typologies and forms that are part of a Platonic philosophical order. The conflict between philosophy and democracy is an epistemological one, not only a moral one.[5]

There often is a tendency among democratic theorists to avoid the theoretical complexity raised by typologies, that which asserts the need to search for differences at the same time as identifying similarities; such endeavors may foreclose addressing the more tractable aspects of democratic theory. Dahl (1986, 191), for example, prefaces a discussion of procedural democracy: "Since their origins in classical Greece, democratic ideas have been plagued by the problem of inclusion: what persons have a rightful claim to be included as citizens with full and equal rights to participate in governing the association. My strategy will be to leave this problem unsolved in order to set out the assumptions and criteria of procedural democracy." Charles Beitz (1989, 5) writes a magisterial volume on political equality, beginning with the exhortation: "For although nothing is to be gained by claiming that equality is not part of the definition of democracy, any philosophical theory of democracy that failed to take up the grounds of and content of political equality would be seriously deficient." Nevertheless, we hear little about the content of equality, and Beitz concludes by emphasizing the indeterminacy of his theory, avoiding the question of "who," though he thoroughly addresses the question of "how." A devotion to equality creates the difficulty of identifying the *eidos* of the citizen for both.

Dahl and Beitz are just two of a notable and heroic heritage of modern democratic theorists who, while acknowledging the importance of asking what equality means and how it may be institutionalized, avoid addressing directly the complexities it poses for the very construction of a community that must assert inequalities by exclusion. Socrates in Book 8 addresses this problem directly by suggesting how equality is the most subversive of political goals, for it undermines the

[3] The word *eidê* appears frequently throughout Book 8; a word for "decline" does not appear in the text. We read of change (*metaballei*, 545d), movement (*kinêthêtai*, 545d) and "mistaken (*hamartêmenas*) regimes" (544a). Yet, volume after volume on Plato records "the decline" but never the frequent appearance of *eidê*: Adam (1902, 2.195) refers to the "order of merit" of the regimes; Benardete (1989) entitles the subsection preceding the discussion of specific regimes "The Fall"; Jowett ([1897] 1962) entitles his chapter on this section "Successive Stages of Decline of Society and of the Soul"; Nettleship ([1901] 1962, 294) entitles his chapter "Successive Stages of Decline of Society and Soul," writes of democracy as a "lower form," and states that "a similar degradation is inevitable" (1901, 294, 299, cf. 300, 308). I am guilty also (Saxonhouse 1996, 90). See Annas (1981, 294).

[4] Roberts (1995, 263–6) catalogs how Athens differs from the democracy in Book 8. See also Annas (1981, 300, 301). In contrast, according to Adam (1902, 2.234): "The materials for Plato's picture of democracy are of course taken from Athens more than any other single city. It is an extraordinarily vivid sketch . . . in spite of manifest exaggeration, [it] brings Athens nearer to us than almost any monument of ancient literature, Aristophanes alone excepted."

[5] Concern with the moral failings of democracy appears earlier in the dialogue (e.g., 488a–489c, 492b–c, 493a–c) and in such dialogues as the *Gorgias*, the *Statesman*, and the *Laws*. A full discussion of Plato on democracy would require analyses of many other passages. For recent efforts see Euben 1994, Monoson 1994, and Saxonhouse 1996. Here I attend only to the epistemological issues.

potential definition of the city itself and of its inhabitants. After being established by violence, the democracy of Book 8 appears as a regime of gentleness and tolerance in this openness. Yet, despite its gentleness, inherent in it are the contradictions that lead to tyranny, the harshest of political forms, and to the violence of the tyrant himself. The key concept for understanding this transition from the gentle to the violent regime, I argue, is *eidê*, which in Book 6 were identified as necessary for the philosophic endeavor and which dominate the typologies of Book 8, but which are lost in the description of the democratic regime.

EIDÊ: BOOKS 6 AND 7 AS PRELUDE

To jump into a Platonic dialogue at midpoint is an interpretive crime; the dialogues are constructed to build upon themselves. Since the discussion of democracy in Book 8 comes after much of the dialogue has been completed, let me set the epistemological stage for my discussion. In Book 6 Socrates introduces the idea or the form of the Good to his interlocutors. Then there is a significant shift in focus in the discussions of democracy, which parallels the shift in focus in the treatments of poetry. In books 2 and 3 Socrates had sanitized the poetry of the Greeks with a view to the moral education of the young. After articulating the theory of the forms in Book 6, Socrates offers in Book 10 an epistemological critique of poetry and art as representation. Similarly, the earlier critique of democracy warns of the appeal of popular demagogues and the dangers of a democracy in which the assembly, loudly praising or blaming its leaders, educates the young. The assembly does not even appear in Book 8.[6] With regard to both poetry and democracy, the theory of the forms refocuses the nature of the discussion.

Book 6 introduces the form of the Good; Book 7 explores the education necessary to bring philosophers into the sunlight of the Good.[7] Socrates and Glaucon begin this investigation of the education of the philosophers with numbers and "this foolish thing . . . of knowing the one and the two and the three" (322c). This leads by nature, Socrates suggests, directly to reasoning, or, as he phrases it, calculation and number are "something that draw in every way toward being [*pros ousian*]" (523a). How can calculation move us in the direction of intellection and the perception of being itself, that realm in which the Good lies? It can because it entails the act of distinguishing and assembling. To count we must distinguish; we must see similarities and differences. We must recognize that apparent differences, what the eyes and other senses recognize, do not preclude construing seen objects as "the same." At the same time as recognizing that differences do not lead to an infinitely variable world, we must also avoid compressing all into one and thus overriding all differences. Once the soul with *noêsis* (mind, intelligence) enters, going beyond sight, we can distinguish (let us say) between the big finger and the little finger and yet also see their similarities; thus, we can add the little finger and the big finger to get two fingers:

> We say that sight saw great and small not distinguished but mixed up together.
>
> Yes.
>
> For the sake of clarity, the *noêsis* was forced to see great and small not mixed up together but separated, just the opposite of sight (524c).

It is the capacity to distinguish one form from another and then to recognize what makes some similar to others that allows us to add and subtract, to count (see, e.g., Crombie, chapter 2 and 101–2). It is the *noêsis* and the ability to separate the parts from the whole—whether to add, subtract, divide, or multiply—that sets the mind on its way toward intellection, providing the mechanisms for comprehending the world we experience around us and for ascending to a vision of the Good.

Calculation is not the end of the process. Socrates takes Glaucon through a series of subjects that must be studied next in order to turn the soul toward the idea of the Good and that which is beyond sight, but we begin the process with the capacity to distinguish, to isolate differences and similarities, to recognize the *eidê*, from which we move on to the comprehension of the highest things. Democracy as portrayed in Book 8, however, is the regime in which we do not make that first step, in which access to the highest forms of knowledge is denied to us because of the inability (or unwillingness) to engage in distinctions, to impose the *eidê* on what we observe around us; democracy in its love of freedom precludes imposing *eidê* on what we see. The consequences are serious. The theory of the forms and the philosophic endeavor require categorization and the *eidê*, which democracy in its concern with freedom rejects.

In Book 6 Socrates articulated the image of the divided line to distinguish the philosopher from the sophist. Shortly before, Socrates had introduced the idea of the Good and used the analogy of the sun to help us understand the role of the Good. As the sun gives the light by which we see and the warmth by which we grow, so does the Good enable us to see and to grow. Though this idea of the Good, or the sun as the analogue of the Good, dominated the earlier discussion, in the image of the line, the idea of the Good appears to be above the line, outside—and perhaps even beyond—intellection.[8] In the image of

[6] This is especially surprising for contemporary readers, since much recent discussion of Athenian democracy has focused on popular participation. (Just to begin, see Hansen 1991 and Ober 1989, 1996.) This concern is entirely absent from Book 8.

[7] As in the discussion of democracy, in this article I focus on the epistemological aspects, not the moral qualities, of the forms. For the latter, see especially 500d–501c, where the philosopher as painter turns to the forms of moderation (*sophrosunê*), justice, and popular virtue (*dêmotikês aretês*), using these divine patterns (*tôi theiôi paradeigmati*) to fill in the city's blank slate (500d–6).

[8] See Jowett and Campbell's diagram (1894, 3.313). Benardete's (1989, 176–7) rendering of the line includes both the sun and the Good, but outside the line, not as part of it. Bloom (1968, 464) draws a similar diagram but does not include the sun or the idea of the Good. See further Gadamer 1986 (28–32, and translator note 22).

the line, the object of *noêsis*, the highest level of intellectual endeavor, is not the idea of the Good. Rather, the objects of our *noêsis* are the *eidê*.

What are these *eidê* that give for many the epistemological, moral, and theological thrust to Plato's work? They are in fact little discussed in the *Republic*, and where they are discussed, the Greek is especially difficult. Yet, they remain at the center of the *Republic* and of Plato's epistemology. As Santas (1983, 233) notes: "A considerable body of literature has been built around the relevant passages. Yet, it is not hyperbole to say we have no satisfying or widely accepted answers to our questions."[9] Here I want only to point to Socrates' insistence that *eidê* are necessary to add and subtract, to see what separates and what unites the indiscriminate world our senses perceive, and that it is our *noêsis*, through which we impose *eidê*, which enables us to comprehend the world around us. Or, as Santas (1983, 256) concludes about what it is that the form of the Good accomplishes: "[It] serves his metaphysics by bringing into relief the very ideality of the Forms, the eternal order and stability of the entities that must exist if this world is not be a 'vast sea of dissimilarity.' " The relevant passage is from Book 6, 507b, which Bloom (1968) translates as follows:

> We both assert that there are . . . and distinguish in speech many fair things, many good things, and so on for each kind of thing. . . . And we also assert that there is the fair itself, a good itself, and so on for all the things that we then set down as many. Now, again, we refer them to one *idea*,[10] each as though the *idea* were one, and address it as that which really *is*.

The thrust behind these confusing sentences seems to be that while there is the idea of the Good and of the Beautiful, there are also the *eidê* that unify the particulars into categories or shapes. Particulars (*polla*) are part of a class, and for us to recognize them as such we must be able to see what makes particulars similar and what distinguishes them from others. To compress the many into one, we must abstract from particular differences that can overwhelm the senses, from what makes the many many.

To clarify his argument, Socrates uses the example of geometers:

> They make arguments for the sake of the square itself and the diagonal itself, not for the sake of the diagonal they draw, and likewise with the rest. These things that they mold and draw . . . they now use as images, seeking to see those things themselves, that one can see in no other way than with thought . . . this is the form I said was intelligible [*noêton men to eidos*] (510d–511a, Bloom 1968).

Glaucon wants Socrates to lead him beyond the forms, to the idea of the Good itself. Socrates resists. The forms are essential for our understanding and comprehension of the world we experience; beyond the forms—toward the idea of the Good—we move beyond that which we need for a comprehension of this world to a theological unity and normative standard that may not be grounded directly in the experiences of our own lives. The forms that categorize and distinguish enable us to order what we experience and enable us to live in this world. The idea of the Good may take us beyond the foundations in this world.

REPUBLIC BOOK 8 AND DEMOCRACY

March and Olsen (1995) identify a series of issues that democracies must address. Among their criteria is:

> Governance involves *developing identities* of citizens and groups in the political environment. Preferences, expectations, identities, and interests are not exogenous to political history. They are created and changed within that history. . . . It is the responsibility of democratic government to create and support civic institutions and processes that facilitate the construction, maintenance, and development of democratic identities (pp. 45–6).

Can democratic governments "facilitate the construction . . . of democratic identities" without raising questions about the fundamental principles on which they are built? This is the question Book 8 makes us confront.

Context

At the end of Book 7, after recommending the expulsion of all who are age ten or older and thus founding Callipolis "most quickly and easily" (514a),[11] Socrates concludes that he and his interlocutors have discovered the city and the man they sought. In the last phrase of Book 7, he states: "It seems to me that it is the end [*dokei moi telos echein*]" (541b). Book 8 begins anew, with Socrates summarizing the qualities of the city that is governed "most highly" by recalling the institutions agreed to in Book 5: the community (*koinas*) of women, the community (*koinous*) of children and their education, the common (*koina*) preparation for war, and rulers who are the best in philosophy and war. *Koinon* (that which is common) and its derivatives dominate this brief introductory paragraph (543a). Absent from this recapitulation is the inclusion of women among the rulers. That may be implicit in the reference to a "common education in war," but Socrates avoids explicit allusion to the comical and radical arguments that filled the first sections of Book 5 and gave that book its outrageous tone.

At the beginning of Book 5 Socrates desexualized the female to incorporate her into the public realm (Saxonhouse 1976). At the beginning of Book 8 he ignores that earlier effort; the sexual female, along with the distinction between the sexes, returns. The change in orientation appears immediately when Socrates curiously refers to what Bloom translates as "houses" (543b). The phrase is *katoikousin eis oikêseis* ("they live

[9] Santas (1983, 257 n. 2) has a helpful bibliography for tracing the history of the debates surrounding these passages. See also Fine 1990, Irwin 1995 (chapter 16), Sallis 1986 (chapter 5), Teloh 1984 (chapter 3), White 1976 (section 3), and the multiple references in each.

[10] Sallis (1986, 383) notes that Socrates uses the words *eidê* and *idea* synonymously.

[11] Callipolis is the community of wives and property, of potential women rulers, and of philosopher rulers that Socrates and his interlocutors founded in Book 5 and called the just city.

in homes"), and the word *oikêseis* (homes, residences, dwellings) is repeated in the next line of the text. The word never appears in Book 5; *oikêseis* are not only the buildings we associate with houses but also entail the notion of what is one's own. The widely used root, *oikei*, means "of the same household" or "related by kinship." In Callipolis, there are no kin and thus no *oikêseis*. Socrates, using the word in his redescription of Book 5, prepares us for the tensions that will explode in the transformation of Callipolis to the regime that loves honor (*philotimon*), which he chooses to call "timocracy" (545b): the deep split between male and female, between what is public and what is private, between an unmoving present and a world of motion. Socrates glided over these tensions by eliminating the private along with female in the construction of Callipolis, making ambiguous how the city might address the passage of time and leaving Glaucon to refer vaguely to "erotic necessities" (458d). The reference to *oikêseis* at the beginning of Book 8 suggests that distinctions between mine and thine may even exist in the "highest" city. As much as any errors in identifying the correct season for the begetting of children that result from miscalculating the nuptial number, these distinctions turn Callipolis into a timocracy.

Glaucon recalls the end of Book 4, when they had digressed from the flow of the argument, and comments on the parallels not between the city and the human being (*anthrôpos*), but between the city and the male (*anêr*) (543c–544b).[12] Glaucon, never completely comfortable with the conflation of the sexes, easily reverts to a world in which differences between male and female exist. No longer do we place male and female warriors and philosophers in the same class or *eidos*, looking to similarities rather than differences; now differences dominate, and separate identities mark male and female. Rather than aggregating into an undifferentiated unity by abstraction (Saxonhouse 1994, chapter 6), Socrates imposes differentiation, reintroducing sexuality, time, and misogyny.

Socrates informs us that change in regimes comes from faction among those who hold office. Faction entails differences. Callipolis was a factionless city. Individuals held nothing, not even bodies, in private: "Whenever anyone of us injures a finger, the entire community . . . senses it and all suffer together with the part that was injured" (462cd). Such a compressed, unified city is not fractured easily. Recalling Homer's invocations, Socrates calls upon the Muses "to speak to us how first faction fell on them" (545d). Homer's Muse spoke of human and divine passions; Socrates' Muses focus on the divisions between the sexes and the difficulty of discovering the proper time for procreation. The complexity of the nuptial number continues to perplex scholars, but that complexity first arises because divisions within the city depend on acknowledging *eidê*, on categorizing male and female.

Mathematizing, as the education of philosophers in Book 7 had shown, entails the ability to distinguish. Even to deal with the nuptial number, as the Muses do, is to acknowledge that we live in a world of similarities and differences—of *eidê*. Callipolis had tried to transcend the categories of male and female by which we compartmentalize humans, impose *eidê* on them, creating realms of public and of private at war with one another. The bodily need to reproduce—a need grounded in a nature that arises from the eidetic differences between male and female—required the "erotic necessities," which the founders of Callipolis would just as well have done without (458d). As we shall see below, the democracy of Book 8 is most similar to Callipolis in this regard: both abstract from differences between the sexes, the former to create the regime of complete unity and commonality, the latter to create the regime of infinite variety and particularities.

In Book 8, Socrates identifies five regimes: aristocracy (Callipolis), timocracy, oligarchy, democracy, and tyranny. He then asserts there are *only* five, challenging Glaucon: "Or do you have some other idea of regime which appears in a distinct form [*en eidêi diaphanei tine*]" (544c)? Likewise for human types: "Then if there are five [*eidê*] of cities, there would also be five arrangements of the souls of individuals" (544e). Glaucon could easily have challenged Socrates. For sure, he knew Herodotus' tale of the debate among the Persian conspirators about the virtues of three regimes (3.81–83); in the *Statesman*, Plato has the Eleatic Stranger identify six regimes. The *eidê* that Socrates proposes in Book 8 obviously do not exist by nature; yet, Socrates argues that they derive from nature in a more profound sense. With a quote from the *Odyssey*, Socrates draws us to the scene where Penelope questions Odysseus, disguised as a stranger and stripped of any identity. She asks: "From where are you? For you are not sprung from an oak of ancient story nor from stone" (19.162–63). Socrates, in turn, asks Glaucon: "Or do you think out of oak and stone emerge regimes rather than from the character of the people in the cities" (544d)? Humans are not born from the froth of the sea, and, contrary to Athenian autochthony myths, citizens do not spring forth from the earth. The regime, in this telling, grows naturally from the inhabitants of a place and takes on the form of those living there. Glaucon listens appreciatively, raising no objections, allowing Socrates to map a limited number of human types onto regimes (544e).

Book 8 begins with the assertion of the *eidê* of human character, of political regimes, and thus of the importance of categorization. Were it not so late in the evening, Glaucon or one of the others still awake might have had the energy to question the proposed typology. As it is, they let Socrates proceed with his five *eidê*. Epistemologically, as we learned in the earlier books, we need *eidê* to understand what it means to add, to subtract, to use our intellects, to set our perceived world into some sort of order. Politically, though, what is the legacy of these categories of regimes? To analyze we need to have *eidê* of individuals and of regimes, but do they become Procrustean beds, originating not in nature but merely in Socrates' fantasies that they are

[12] *Anêr* appears three times in 543d–544a. Socrates continues to discuss the *anthrôpos* (544d).

based on a limited number of human types? Democracy and the democratic man provide the antidote to the argument from the beginning of Book 8; they deny the categorizing and summarizing role of the *eidê* and instead allow variety to flourish. But does the freedom from *eidê* and consequent equality mean that democracy allows monsters to be born? Or does true freedom require the absence of *eidê*?

Book 8 begins by forgetting the effacement of the differences between the male and female that initiated the radical proposals of Book 5. As Socrates addresses the difficulty of deriving the correct nuptial number, he ignores this earlier effacement and worries instead about reproduction and the mingling of the two distinct sexes. Therewith, a latent misogyny surfaces. In Book 5 Socrates dismissed any natural inferiority of female to male; in Book 8 imposing *eidê* on male and female establishes classes and hierarchies that appear prominently when Socrates describes the rise of a timocracy. Miscalculation of the proper nuptial number results in the lawless mixing of iron, silver, and gold (547a), but the emergence of the timocratic man is more dramatic than this obscure lawless mixing suggests. A nagging wife and mother, the *eidos* of the female such as would appear in an Aristophanic comedy, gives rise to the timocrat. He hears his mother express anger that her husband (*ho anêr*) is not a ruler, does not seek wealth or engage in civil suits. He is not ambitious, she complains; he is "unmanly" (*anandros*) (549c–e). Adeimantus, never one to speak positively about women, wholeheartedly agrees: Many such things belong to women. With the reintroduction of the *eidê* of male and female, with the reenforcement of the differences between them, there is the identification of particular qualities associated with each, and the term "unmanly" becomes a derogatory epithet. In Callipolis, the conflation of male and female precluded such language.

Timocracy, itself arising from the reintroduction of sexual difference, in turn becomes oligarchy, since once there are private treasure houses, the desire for private accumulation replaces the desire for honor.[13] The rise of oligarchy exacerbates distinctions between groups within the city; oligarchy is a city torn within itself, divided into two factions, lacking the cohesion of Callipolis, on the one hand, and the openness and freedom from compulsion that we find in democracy, on the other. The concentration of wealth and the creation of a class of those without money leads to beggars, thieves, purse-snatchers, and craftsmen of all sorts of evils. The portrait of the oligarchic city is one of misery and division; the accumulation of wealth for the few means the absence of any cultivation of moderation among the rulers or among the poor. Licentiousness follows for the wealthy, hatreds and plotting for the poor (555d). The move to democracy is no "decline." It is a blessing, with the extremes of poverty and the evils associated with oligarchy left behind and the divisions between rich and poor surmounted. To achieve this, though, there must be the violent overthrow of the oligarchic regime. The strong poor, desirous of new things (*neôterismou erôntes*, 555de), kill and exile the lazy, fat men of wealth (555d–556a). After the violence of the transition to democracy, we arrive at a regime of softness and gentleness, no longer plagued by factions and without compulsion—though its gentle formlessness begins in the violent expulsion of the wealthy and ends in the violence of the tyrant.

Democracy

Dêmokratia then comes into being whenever the poor, not weakened by the overindulgence and the laziness of the rich, are victorious (557a), killing and exiling the "resourceless" (556d) rich. Socrates takes us briefly to the institutions of democracy: "They [the inhabitants of a democratic regime] partake of the regime and the offices from an equality [*ex isou*], and for the most part the offices in it are by lot (557a)." This is all he says about the institutions of a democracy. He quickly turns to the question that will control the rest of the discussion of democracy: What sort of regime is this (557b)? The way of life, its culture, dominates; we hear no more about offices or the lot, much less assemblies or juries. The defining characteristic of democracy, Socrates tells us, is freedom. "The city is full of freedom [*eleutherias*]," freedom of speech, *parrhêsia*, the opportunity (*exousia*) to do in the city whatever one wishes; in a democracy, Socrates reiterates, each one will arrange his private life (*idian*) as he pleases (557b).

From the central theme of Callipolis, the *koinon*, the sharing of friends, property, and family, we move to a privatized world. The democratic regime exalts the *idion*, that which separates, makes us distinct from others; and the democracy of Book 8 has the openness to incorporate all those distinctions, to allow for what is our own, and not to demand the sharing of qualities, place, friends. Democracy here is the private regime in which we act as individuals, not as parts of a common enterprise. This "idiocy"[14] does not bring faction to democracy; it leads rather to an egalitarian gentleness that imposes neither *eidê* nor hierarchy. Differences do not matter, as "each arranges his [her] private thing [*idian*]" (557b). This regime's emphasis on particularity results in a vast variety of human types (*anthrôpoi*, 557c). This appears in contrast to the earlier claim that there were only five human types. We experience democracy's multiplicity without sorting or categorizing, defying any theoretical model that can distinguish and then unify multiplicity into *eidê*. Democracy as it appears here is pre- (or post-) eidetic, allowing for a wide array of visual and sensual experiences.

In the passages on the freedom of democracy,

[13] In this discussion of oligarchy, Socrates (and/or Plato) in no way defends the rich against the poor. Reservations about democracy in Book 8 come not from hostility to the poor or worries about the unskilled having political power. Rather, the portrait of the wealthy is devastating. The Woods (1978) claim that Plato associates poverty and engagement in banausic trades with the lack of moral character. The opposite is true here.

[14] I use "idiocy" in the sense that derives from *idion*, that which is private and distinctive, that which separates an individual, a family, a species from another, that which does not attend to the public. See further Saxonhouse 1983.

Socrates repeats forms of the word *poikilos* four times (557c) and claims that this "multihued" quality makes the many judge democracy the most beautiful (*kalliston*). In Callipolis, the beautiful city by name, there is the uniformity, stability, and oneness of the human *eidos*; in the city which is most beautiful to the many, there is no categorization, no effort to move beyond the particular to identify what unifies. The particular is accepted in its uniqueness, not as a tool to extend beyond itself or to become a part of the larger whole. Consequently, democracy provides the setting in which Socrates can imagine a multiplicity of regimes, including, of course, Callipolis.

Democracy, dismissing the unifying *eidê*, means that nothing and no one is imprisoned in a form. Socrates extends this even to the level of language; in a Thucydidean twist, words unconstrained by definitions take on multiple meanings. Describing the transformation of the youth into the democratic man, Socrates speaks as if he were quoting Thucydides on the Corcyrean revolution (Adam 1902, 2.243). Moderation is called cowardice; arrogance, good education; anarchy, freedom; wastefulness, generosity; and shamelessness, courage (560d–e). The malleability of words matches the fluidity of a democracy; unrestrained by *eidê*, words like people are free, unbounded by history, tradition, or past usage. In Thucydides, the openness of language is the basis for civil strife; in Socrates' democracy it is merely one example of the absence of compulsion. Neither individuals nor words are forced into *eidê*. In Callipolis, the regime of necessity, there was no uncertainty about form; that uncertainty becomes the underlying principle of democracy.

Words are but one example of this formlessness. Socrates offers many other illustrations of this extreme freedom (563b). In democratic regimes, slaves do not differ from citizens. "Men who are bought and women who are bought are no less free than those who bought them" (563d). Slavery, categorizing humans and setting them into hierarchical relationships, is meaningless in a society which sets no common boundaries between individuals, which has no *eidê* by which we classify individuals. This formlessness extends as well to the freedom of women toward men, of men toward women, leading to the complete promiscuity of the sexes. The parallel to the mingling of the sexes practicing gymnastics in the palaestra in Callipolis is striking, but the similarity derives from opposite impulses. In Callipolis, a unifying *eidos* abstracts from the particular differences of the sexes, focusing on what is shared rather than what differentiates; in democracy, the mingling comes from the absence of *eidê* that might give the male or female sex any form or meaning. In each case, no hierarchy exists because each regime considers sex as irrelevant, one by unifying male and female into one *eidos*, the other by allowing for an infinite multiplicity of forms.

As with slaves and their masters, as with male and female, so, too, even with animals in a democracy. Animals are freer in a democracy than in any other regime. Anyone who had not experienced it would not believe how much freedom they have. Thus, "female dogs according to the proverb become of the same sort as their mistresses and also the horses and asses are accustomed to journeying freely on the roads, hitting whoever stands in their way if they do not stand aside, and thus all the rest is full of freedom" (563cd).[15] Animals, having equal access to the streets of Athens, unself-consciously bump into humans. Meanwhile, men condemned to death or exile do not take on the form of "condemned men"; they dwell in the midst of the city. Like the animals, they wander the city unremarked upon (558a).

Freedom, in the problematic meaning that it acquires during Socrates' discussion, entails the failure to distinguish one form from another, to give form to the slave as opposed to the master, to the female as distinct from the male, to the dog as distinct from the human, to the condemned man as distinct from the innocent citizen. This fluidity and malleability, this absence of *eidê*, attractive in its abstraction from the compulsion that marked Callipolis, nevertheless creates an underlying unease as Socrates clarifies the dangers inherent in this formlessness.

From Gentleness to Compulsion

Democracy as a forgetfulness of form extends to the inability to distinguish between good and bad pleasures—to the toleration of all. In Socrates' democracy, freedom is not being forced into a shape. Democracy thus entails a rejection of the very principle that dominated the *Republic* since Book 2, when the first steps were taken toward founding the just city: Each individual is suited for one task, Socrates told us there. Such a principle is meaningless in a multifarious democratic regime, where all do many things, where no one retains a single form that lasts over time.[16] The politics of the first city of the *Republic*, Glaucon's so-called city of pigs (372d) and its extensions throughout the dialogue, depend on the principle that each person performs one task, everyone having a prescripted *eidos* that sets him or her into a category. Once the individual is properly identified according to his or her nature, there is no fluidity; the house builder remains the house builder and does not become the shoemaker. This then becomes the definition of justice in Book 4: "Each one must practice that one thing about the city, the one for which his nature has made him naturally most suited" (433a).

Earlier in the *Republic* the forms gave political—and

[15] Jowett and Campbell (1894, 3.398) write: "Cicero cites, but can't carry over the jest. The most extravagant and comical ideas . . . often occur in the works of Plato. But the manner of saying them . . . does away with the feeling of bad taste and impropriety." We need to read these passages as more than comical. They are integral to Plato's presentation of the fundamental assumptions about democratic principles.

[16] Here we see recollections of Athenian democracy, in which citizenship entailed taking on many forms, from juror to ecclesiast to hoplite to port inspector. See also footnote 4. Annas (1981, 300) misconstrues Athenian democracy when she responds to the tolerance theme of this section by commenting: "What Plato presents as the tolerant indifference of democracy could not be further from the state of affairs in Athens."

epistemological—order. Any blurring of *eidê*, by someone not doing his or her own thing, would erode the potential for justice of the city; yet, just as the just city is built on an adherence to form and the exploitation of form for the structure of society, so democracy, as the regime that rejects forms, is in Socrates' words an "anarchy" (562e). Lest that word take on contemporary connotations of violence, anarchy here means simply being without rule, which leads to a profound gentleness. In this soft regime where compulsion is banished, where forms do not tyrannize, all do as they wish. The parable of the cave in Book 7, which tells of dragging one with the potential to be a philosopher out of the cave and then forcing him back down, makes us harshly aware of the compulsion of Callipolis. About democracy in Book 8, Socrates states: "There is no compulsion to rule in this city, even if you are competent to rule, nor to be ruled, if you do not wish to, nor to war when the city is at war" (557e).

The lack of distinction between male and female, animals and humans, condemned men and innocent citizens makes clear that the forgetting of form, and the absence of compulsion attending it, also means the absence of hierarchy. With the blurring of form, criteria for authority dissolve, claims to rule retreat. As Socrates begins to identify the negative aspects of democracy deriving from the absence of *eidê*, he offers as examples how in a democracy the teacher fears and fawns on the students (563a); how the young show no deference to the old; how there is no difference between the ruler and the ruled, whether in public or in private (562d); how the father habituates himself to his son; how the city dweller fears the metic, the foreigner without rights living in this city (562e); how the old and the young converge and compete (563a); and how there is no distinction between strangers and citizens (563a)—calling into question the very structure of a polis that cannot identify its own citizens. Everything (one) blends into the other. We can no more tell moderation from cowardice than we can tell a father from a son, a teacher from a student, or a citizen from a noncitizen.

In contemporary understandings, the absence of hierarchy translates into equality, but that is too simple a translation for what occurs in the *Republic*; equality entails the identification of those who are equal, an apprehension of the forms that unite and separate. The democracy of Book 8 gives us such a radical view of equality that there is no foundation for recognizing those who are equal or unequal, which in turn would lead to a hierarchy—whether of male over female, humans over animals, or Greeks over barbarians. Likewise, an Aristotelian theory of distributive justice, of equals to equals and unequals to unequals, cannot survive Book 8's radical equality or the formlessness of a democratic regime that, according to its principles of freedom, refuses to impose forms.

The same fluidity, absence of compulsion, and abstraction from hierarchy characterize the soul of the democratic man, "of such a sort in private [*ho toioutos idiai*]" (558c). The transformation of the oligarch's son into the democratic man begins with the unacknowledged opposition between freedom and necessity (561a). There is at first the freeing of all desires, necessary and unnecessary, the submission to all, scorning neither the necessary nor the unnecessary and nourishing each equally. The democratic man, like the democratic regime, lacking the compulsive categories of *eidê*, is unable to distinguish, to place any desire in a box with a label telling him whether to use or repress it or to set it in a hierarchical relation. "If someone says there are good and bad pleasures, he throws his head back and says all are alike and must be honored equally" (561c). There is no order in his life, but he calls his life "sweet, free, and blessed" (561d). Without necessity, he can live multiple lives. Like the city filled with many *paradeigmata* of individuals, he is filled with a multiplicity of life styles:

> He lives his life having set pleasures into an equality [*eis ison*]. To the chance passion he hands over rule of himself until he is full and then to another, scorning none but nourishing all from an equality [*ex isou*]. . . . He lives each day welcoming the desire that falls on him, at one time getting drunk and playing the flute, at another drinking only water and restraining himself, then doing gymnastics and again being idle and careless of everything, and then spending time as if he were philosophizing. Often he is active in the city's affairs and jumping he says and does what he chances on (561bd).

The list goes on and on.

The democratic man who does not recognize some pleasures as equal in their necessity and others as unequal in that they are unnecessary has in his soul "the most characters [*êthôn*], and is beautiful and many-colored [*poikilon*]" (561e). He possesses within the soul the radical equality of the democratic regime of complete freedom. His soul, like the city he reflects, is anarchic. Anarchy etymologically means not only "without rule" but also "without *archê*," without beginning or foundation. There is no primary resting place. The city and the individual float without origins or direction defined by a moment of creation. A nature controlling, limiting what one does, is absent. Both the democratic man and democracy exhibit the fluidity and flexibility of form that opposes the fundamental principles of the philosophy and politics articulated in the previous books.[17]

The portrait of the democratic man suggests a maniacal experimentation with various lives and a loss of the gentleness of the democratic regime. In all the references in this section to the beauty and gentleness of this multiplicity, to the genial—almost comic—democracy, there is nevertheless the threatening undercurrent of the violence of a life without *eidê*, whether public or private, a life without categories. This disturbing undercurrent surfaces when Socrates and Adeimantus, quoting from Aeschylus, recognize that dwellers in a democracy say whatever comes to their lips. Perhaps this is an allusion to Athenian *parrhêsia* (freedom of speech) but also, given the

[17] Wolin (1994, 49–50) captures briefly the point I am making here when he discusses the limitations that constitutionalism imposes when it assigns form and "reconstitutes politics as identity." To underscore his point, he turns to the democracy of Book 8.

reference, to blasphemy. If the inhabitants of a democracy have no worries about blasphemy, are there no gods who care about what they say? Are the citizens irrelevant to the gods? Or the gods to the citizens? For the Greeks, the gods would not control a world in which men say freely whatever comes to their lips. No punishments come from the gods. No image of a bloodied Oedipus or an Antigone hanging in a cave outside the city need disturb their sleep.

Socrates tells us that democratic citizens pay no attention to law (563d), written or unwritten, so that no one can be their despot. Yet, again, behind this delicious freedom remains the ancient readers' recognition of the divine source of the unwritten laws and the tragic consequences of blurring the boundaries between human and divine. Antigone's famous speech about the unwritten laws of Zeus reminds us of those decrees that come from the gods and not from humans. The gentle regime of tolerant men and women who abstract from *eidê* is so gentle that it is profoundly impious and inattentive to the laws of the gods, just as it is inattentive to the judgments of execution or exile by its own citizens. In this softness, however, democracy lays the groundwork for the severest forms of violence.

As boundaries between gods, humans, and animals dissolve, or are never even recognized, freedom becomes slavery, and the epistemological denial of *eidê* finds expression in regimes of severe rather than gentle consequences. The transformation of democracy into a tyranny and the democratic soul into the tyrannical soul, so powerfully depicted in all its depravity in Book 9, is a sad tale. According to Socrates, the cause is a certain greediness for what democracy has defined as a good: freedom and, as we understand now, freedom as the destruction of *eidê*. But the image that captures most profoundly the end of democracy comes at 565d and the mythos of the temple of Lykaian Zeus in Arcadia. Socrates relates the story:

> What is the beginning of the change from a leader into a tyrant? Or is it clear that when the leader begins to do the same thing as in the myth which is spoken about the temple of the Lykaian Zeus in Arcadia . . . how the man tasting human organs cut up with organs of other sacrificial animals necessarily becomes a wolf (565de).

The leader who becomes the tyrant "tastes the slaughter of kin with unholy tongue and mouth" (565e), and if he is not killed, he becomes a tyrant, and he changes into "a wolf from a human being [*anthrôpou*]" (566a).

Democracy ends with a story of human sacrifice and cannibalism. The democratic regime, in which one says whatever comes to one's lips, in which a radical equality means no hierarchy, in which animals are treated no differently from humans, and in which humans see themselves as not needing or as no different from gods, ends with the impious and repugnant cannibalism to which such blurring of boundaries leads. Tyranny takes over the principles that marked democracy and shows the darkness to which a regime that ignores *eidê* leads once the violence of the passions takes over. The tyrant does not distinguish between being human and being a wolf, just as democracy does not distinguish between the human being, the ass, and the horse.[18]

Socrates concludes Book 8 by telling how the tyrant gains power. In a reference to themes from the early passages of the dialogue, we learn that the tyrant does not know how to distinguish friends from enemies. Such categories appeared vivid to Polemarchus when he was first challenged to define justice; they were questioned as Socrates explored our capacity to recognize friend and enemy, our need for a sharper and more profound ability to make those distinctions. The tyrant, the final expression of the democratic man, is the full expression of this incapacity to distinguish. He must (*dei*) do away with those who speak honestly to him about what is happening (those who are his friends) if he intends to rule; no one of any worth among his friends and foes remains (567b). Tyranny likewise is purged of both friend and foe, so the Polemarchian problem that set the interlocutors forth to find justice and injustice disappears, just as in a democracy, according to the principles of Book 8, the Polemarchian problem—and with it the impulse to philosophize—disappears, not by a purging but by its irrelevance.[19]

CONCLUSION: *EIDÊ* AND DEMOCRATIC FREEDOM

In the traditional story of the gigantomachy between ancients and moderns, theories of equality and inequality have divided the two camps. It is argued that the ancients—Plato, Aristotle, Cicero—offer a hierarchical world view in which political regimes succeed insofar as the order they impose matches the natural hierarchy of talents, whether they be those of the philosopher rulers of the *Republic*, the freemen of Aristotle, or the well-born of *De Officiis*. In contrast, from (let us say) Machiavelli onward, the hierarchy based on some natural criterion of worth breaks down. The chain of being which controlled much of medieval thought shattered under the bold assertions of Machiavelli, who urged men to imitate beasts, women to imitate men, and men, in order to outwit Fortuna, to imitate fickle

[18] The tyrant in Book 9 continues the theme of formlessness, not recognizing the difference between waking and sleeping (571cd), gods and humans (573c); he is himself like a woman (579b). The description of the tyrant illustrates the true tragedies that arise when the failure to distinguish is combined with eros and with political power. On the close connection between the tyrannical man and the democratic man, note the use of *poikilos* in the description of the parts of the soul at the end of Book 9 (588c).

[19] Benardete (1989, 200) analogizes between the democracy of Book 8 and philosophy, arguing: "Democracy's failure to enforce its decisions and protect itself parodies philosophy, for which everything is open to revision." (Cf. Barber 1996, Euben 1996, Monoson 1994.) This is too strong. Philosophy as the exercise of the intellect pursues *eidê*. Democratic freedom and equality entail the dismissal of *eidê* and thus create tensions with the philosophic endeavor. This does not mean that philosophy necessarily accepts the *eidê* any political regime imposes. In Book 2 Polemarchus thought he knew who was friend and who was foe. Socrates shows that the *eidê* Polemarchus established were not adequate. Philosophy is willing to question the constructed *eidê*; the democracy of Book 8 dismisses them. Insofar as philosophy seeks *eidê*, it remains an activity of compulsion—dividing and uniting—rather than one of freedom.

women. The heroes of Machiavelli's story transform themselves from subjects to rulers, from advisers to princes, and break through what appeared to be natural distinctions. The flux of nature denies any determination of who or what men and women will be. Machiavelli gives us a world of fluidity, in which forms are not given by nature, to be discovered by the intellect and put into place in the political structure. Forms are the result of human assertion and efforts applied in opposition to a formless nature (Pocock 1975, chapter 6 and 169).

The above discussion suggests that, at least insofar as Plato is concerned, the story of the gigantomachy, with the ancients at swords with the moderns over issues of equality and the fluidity of nature, is far too simple a tale. Plato does not merely favor hierarchy and reject equality as a political principle; he does not merely assert the pre-existence of *eidê* waiting to be forcefully stamped on individuals to set them into an ordered world.[20] Instead, inquiring into the principles of a democracy, he explores the premises of a fluid nature, of a world without *eidê*. Machiavelli, offering a new world view of a fluid nature, challenges medieval thought and prepares the foundation for the modern world's claims of equality, whereby no individual so differs from another that s/he can claim authority over others. Plato's Socrates in Book 8, like Machiavelli, offers a view of a natural world of flux without any fixed point, lacking beginning and form. Precisely because the early modern theorists did not follow through on the theoretical principles behind the rejection of *eidê* in the embrace of equality, as did Socrates in Book 8, they did not address the difficulty of justifying the imposition of *eidê* in a world of flux; they did not confront the problem of identifying the *eidê* of those who were supposedly equal. History has shown and continues to show the indeterminacy of equality in a Machiavellian world of flux.[21] It has been a truism almost since the birth of liberal theory that freedom and equality are at odds. The democracy of Book 8 suggests not that they are incompatible, but that the mistake is the failure to recognize that true equality can only come from the freedom from tyrannizing *eidê*. Plato forces us to see the deepest consequences of our casually held principles.

While the democracy of Book 8 gives us a threatening portrait of the city without *eidê*, at first there is something very appealing about the multihued regime that transcends categories to include all—women, slaves, horses. Socrates uses the word *kalos*, beautiful (three times in 557c), to describe this regime. Though it is easy to dismiss this as ironic, perhaps our prejudices about the *Republic* as extolling the city of Book 5 and our insistence on describing Book 8 as the story of regimes "in decline" make us assume irony here. We lose the richness of Plato's work and let ourselves be drawn into worn-out models of "the ancients versus the moderns" if we see the democratic regime in Book 8 as simply a decline on the way to tyranny. Socrates has other criticisms of democracy. By Book 8, he turns away from familiar critiques about democratic assemblies and identifies the epistemological problem democracy introduces, namely, that a radical equality may entail the incapacity to assess who rules, to decide who—metic, slave, woman, Cyclops, horse—is inside and who is outside the city. And Book 8 may ultimately reveal how that incapacity to impose forms, whether they be by nature or not, leads to the cannibalism of the temple of the Lykaian Zeus, setting the stage for the violence of the tyrant. Taking more seriously this section of the *Republic* than has been done previously points to the political role of the epistemology of the *Republic*'s central books. Plato's concerns about democracy do not come only from an animus toward the regime that executed Socrates; they arise because of the problematic forgetting of *eidê* that is central to the freedom of the democratic regime. The investigation of *eidê* in the democracy of Book 8 forces us to face an inherent tension in all democratic regimes.

March and Olsen (1995, 46), as noted above, ask that democratic governments "create and support . . . [the] development of democratic identities." They reflect the current sensitivity to the value placed on individual and communal identities. What else is "identity" but the imposing of an *eidos* on the self or others? Socrates' portrayal of democracy in Book 8 questions whether democratic governments committed to freedom and the radical equality that is the true companion of that freedom can help create those identities without working against their own fundamental principles and without becoming tyrannical. To develop identities is to impose *eidê*. The freedom of democracy is the rejection of restrictive *eidê* which lead to exclusion and hierarchy. Yet, to live without political and epistemological *eidê* is to create the conditions in which gentleness may be readily transformed into violence, in which thought becomes a disordered melange, in which words have no history, and in which the soul begins an inevitable journey to tyranny.

Book 8 presents the epistemological and psychological challenges to claims that democratic governments should enable the construction of identities. The issue Socrates poses is not whether regimes should or should not do so, but rather that the underlying openness at the heart of democratic principles defies *eidê*, makes such demands contradictory, and points to the potentially tragic incompatibility of such choices. Without addressing directly the need for, as well as the dangers of, *eidê*, we ignore the inherent theoretical contradiction in the democratic regimes we inhabit. To address them effectively, we must understand the challenge Plato posed almost 2,500 years ago to the readers of his dialogues. Most basically, there is the question of whether the very identification of citizens and noncitizens violates the principles on which democracies have

[20] The myth (*pseudos*) of the metals from Book 3 may seem to undermine my claims here, but that passage remains ambiguous. It need not be read as a defense of *natural* human inequality. It is a view of human inequality required by the politics of Callipolis, for which Socrates invents it.

[21] See Thomas (1983) for a discussion of this problem in early modern England, as it struggled with how to differentiate humans from animals and then how to justify distinctions among humans, male and female, black and white.

been founded. One longs for a Platonic theory of statecraft that would lead us out of these contradictions, but Socrates does not offer such a solution. As always, he leaves us in a state of *aporia*, pointing to the depth of the issues and the complexities of any solution to the problem that even the best contemporary thinkers glibly set aside. We cannot expect democratic political regimes to help develop identities until we resolve the incompatibilities Socrates, by confronting radical equality head on, has shown between identity and democratic freedom, between the tyranny of the *eidê* and our need for them.

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The Illusion of Power and the Disruption of Moral Norms: Thucydides' Critique of Periclean Policy

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Scholars in both international relations and political theory have been turning their attention to Thucydides with increasing frequency but with dissimilar questions. We draw on both traditions of inquiry to reexamine Thucydides' view of Pericles. We argue that antithetical reasoning is present in the treatment of Pericles and is manifested in the opposition between the statesman's brilliance and the infelicitous consequences of his statecraft, as become evident in the work as a whole. This antithesis undermines the claim advanced by certain figures in the History, *as well as by contemporary realists, that states (statesmen) should not be held to the same moral standards as individuals because to do so subverts their capacity to conduct prudent policy. We propose that Thucydides' work suggests, instead, that it is precisely when the norms of moral conduct are disrupted that states and individuals find it next to impossible to chart a prudent course of action.*

Stanley Kubrick's *2001: A Space Odyssey* is punctuated by the incongruous appearance of a tall, polished metal shaft in the midst of an otherwise natural setting, portending doom or perhaps hiding secrets, the knowledge of which confers understanding and mastery of life. In the film, humanity is driven by the puzzle the shaft embodies to attain to ever greater technological mastery over nature, yet it remains as uncomprehending as an infant in the presence of this featureless Sphinx. Thucydides' *History of the Peloponnesian War* exercises a similar effect on us. We are excited by the keen analysis, the apparent accessibility of the actors to rational interpretation, the ring of familiarity in the events the historian recounts. Above all, we are excited by Thucydides' claim to have written a work that will become "a possession for all time," a work that we will "judge useful" (I.22.4).[1] But what profit does it contain? Good counsel, a theory, an example to avoid (if so, how)? Having treated Thucydides as a forerunner of modern realism (Dougherty and Pfaltzgraff 1990, 90), international relations scholars today appreciate the complexity of his text and frequently challenge the traditional realist reading (Bagby 1994; Doyle 1991; Forde 1995; Garst 1989; Lebow 1991). Political theorists also increasingly appreciate the *History's* complexity of thought. Recently, for example, theorists have focused on its pessimism (Pouncey 1980), its pervasive humanity (Orwin 1994), its subtle critique of democracy (Farrar 1988; Ober 1993; Saxonhouse 1996), its account of the fragility of political unity (Euben 1990), or its analysis of the tension between love of glory and commitment to the common good (Palmer 1992). The growing attention to the *History* in both fields bears witness to the observation by a leading classicist that Thucydides forever proves resistant "to paraphrase and summation" (Connor 1984, 231).

In this article, we attribute Thucydides' power to resist summation to his pervasive use of antithesis as a tool of narrative and analytical style. Antithesis in the form of paired speeches or the dramatic juxtaposition of, for example, the Periclean funeral oration and the plague narrative is well known. But Thucydides' use of antithesis in his treatment of Pericles, a part of the text usually thought to exhibit a more straightforward teaching, has commanded less attention. We investigate that treatment here, anticipating a more subtle and elaborate judgment on the Periclean virtues and a window onto Thucydides' political thought. We conclude that the historian's treatment of Pericles conveys two antithetical yet complementary attitudes regarding the possibility of conducting ourselves wisely. The first is a relentless skepticism about humanity's capacity to assure its welfare by relying on a kind of strategic brilliance that is exercised in either ignorance or defiance of moral norms. The second is a conviction that moral norms must be buttressed by the effective application of coercive power. Thucydides' driving attention to both these views goes to the heart of much contemporary theorizing in international relations regarding the appropriateness of moral or strategic action.

THUCYDIDES' USE OF ANTITHESIS

The view of the *History* as an "objective" account of the war and its author as a "scientific" historian or a detached reporter of bald fact has been dead in the specialist literature for some time. Scholars today focus more on the artistry of the work and the intensity of the experiences it elicits (see, e.g., Aron 1978; Connor 1977a; Hunter 1973; Orwin 1994). Wanting to understand how the text works as a whole, they have little confidence in the notion that the author's "teaching" can be found in some specific episode, such as the Melian conference, or in some isolated first-person comments, such as those pertaining to Pericles. Instead, they examine how the interrelations of different aspects of the text (speeches, narrative, various episodes, language patterns, and literary devices) work to arouse the emotions and intellect of the reader. In this article, we bring this strategy of reading the text to the examination of Periclean leadership.

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[1] Unless otherwise noted, all translations are from the revised Crawley edition (Strassler 1996).

Much of the power of Thucydides' prose style derives from his use of antithesis, that is, the contraposition of claim and counterclaim in close and rhetorically effective order. Although antithesis is most evident in the paired speeches, we encounter antithetical claims pervasively. This "most instinctive, necessary clothing of [Thucydides'] thought" (Finley 1942, 46; see also Connor 1991, 67) extends to the very structure of the work, to the juxtaposition from phrase to phrase, chapter to chapter, book to book of starkly contrasting images, such that expectations nurtured at one point are dashed at another. Thucydides dresses the images up rhetorically as unchanging, inevitable realities and as matters of fact (see Connor 1984). But in the ensuing narrative he reveals them to be mere social and intellectual constructs, which the protagonists of the drama, through ambition, fear, and conceit, proceed to erode.

Awareness of Thucydides' use of antithesis helps us not only appreciate his style but also understand his political philosophy and use the text to engage debate in international relations theory. The antithesis, when wielded with the skill and relentlessness of a Thucydides, leads to skepticism, that is, to doubt regarding anything and everything that one may have previously accepted as knowledge, wisdom, or truth.[2] In Thucydides' era, the corrosive power of such skepticism, quite ascendant in Athenian intellectual circles, moved some Athenians to doubt the possibility of defining moral conduct in a way that would resist such inquiry. Thus, they fell victim to the idea that the prevailing notion of the Good must simply embody the interests of powerful people. This is the argument of Plato's Thrasymachus and Callicles. It is also the argument that Thucydides lends to the Athenian generals at Melos.

ANTITHESIS IN THUCYDIDES' TREATMENT OF PERICLES

We reopen the file on Pericles because he comes across as the only actor of the *History* who is "on top of things." Thucydides' attitude toward Pericles seems to be one of "reverent admiration" (Cornford 1907, 50). He is, after all, the only speaker of the *History* whose words are never disputed by those of an adversary. The text thus invites us to believe that good politics must be Periclean politics. And yet, alert to Thucydides' use of antithetical argument, we suspect that on closer reading the text invites us to search for good politics in the close, critical examination of Pericles' politics.

Thucydides' catalog of Pericles' virtues is well known, and we need not rehearse it here. Suffice it to recall that, according to Thucydides, "for as long as [Pericles] was at the head of the state during the peace, he pursued a moderate and conservative policy; and in his time its greatness was at its height" (II.65.5). Among the virtues displayed by Pericles are his skills as a military leader (I.111.5, 114.5, 116.3, 117.2; II.31.1–2), his appreciation of and excellence in reasoned deliberation regarding political and strategic issues (II.40.2–3, 62.5), his integrity (II.13.1, 65.8), his grasp of human psychology (II.59.3ff), and his generally accurate estimate of Athenian military strengths and weaknesses (II.65.5). Moreover, he is a master of the art of persuasion. He draws on his oratorical skills to win approval of his ideas even as his popularity flags (II.59.2ff), and he uses rhetoric to stoke Athenian patriotism and self-confidence (II.40.1–41.5). In sum, his grasp of human psychology, his power to persuade, and his military skill all contribute to his ascendance among the Athenians and his ability "to lead them instead of being led by them" (II.65.8).

Nevertheless, we detect in the *History* as a whole a subtle interrogation of this explicit, glowing appraisal. While showing leadership, Pericles panders to the population; while extolling Athenian virtues, he undermines the norms of social cohesion; while advocating a prudent course of action, he engages Athens in an adventure that produces momentous turns of fortune.

Thucydides unquestionably attributes to Pericles a unique ability to persuade citizens to adopt a strategy that involves hardship and self-discipline, as well as an ability to speak frankly to the Assembly to the point of contradicting the wishes (or whims) of the multitude. During the period of Pericles' prominence, "what was nominally a democracy [became] in his hands government by the first citizen" (II.65.9). Each of Pericles' three speeches, as fashioned by Thucydides, shows the Athenian leader turning public opinion in a particular direction. Yet, on one significant point, Pericles comes across as a consummate follower, a caterer to the desires of the many. Pericles is uncritically devoted to the possession of empire for the sake of Athenian glory and for the material luxuries it provides.[3] During his leadership, the Athenians boldly transfer the treasury of the Delian League from Delos to Athens and use it to finance the adornment of the city. The alliance becomes an *archē*, an empire. As crisis unfolds, Pericles positions himself as the leader most capable of protecting the empire. He calls for only momentary restraint in adding to it: "[Do not] combine schemes of fresh conquest with the conduct of the war" (I.144.1). He does not rule out enlarging it once the crisis has passed.

Although Pericles is, in many ways, quite unlike the crop of leaders that gains prominence after his death, he does not differ from them as regards this attachment to empire. Thucydides seems to acknowledge this resemblance explicitly by attributing the same startling phrase—you hold a "tyranny" (II.63.2 and III.37.2)—to both Pericles and Cleon.[4] The term "tyranny" would

[2] Thucydides may have acquired his antithetical style through ties to the Sophists. On this possible link see de Romilly (1988, 86–7, 97–9), Finley (1942, 44), Hornblower (1987, 45–52, 60–2), and Connor (1984, 27–8).

[3] Plato explicitly criticizes Pericles for pandering to the Athenians in this way (see *Gorgias*, 515e ff.).

[4] Pericles says: "What you already hold is like a tyranny" (*hōs tyrannida gar ēdē echete autēn* [2.63.2]). Cleon says: "You don't appreciate that what you hold, the empire, is a tyranny" (*ou skopountes hoti tyrannida echete tēn archēn* [3.37.2]). To preserve the important parallel with Pericles' language, we depart from Strassler's (1996) rendering of Cleon's use of "*tyrannida*" as "despotism." For

have resonated among Athenian readers. It connotes much more than the extralegal seizure of power. It conveys the violent abuse of power and the disregard or outright rejection of customary Greek norms of decent, legitimate conduct. Contemporaries would have been struck by this term. Athenian readers would have remembered how, toward the end of the war, the oligarchs behaved so brutally before being deposed that they came to be known as the "thirty tyrants." Pericles uses the phrase during the crisis that accompanies the plague. Plague, he argues, must not weaken Athenian resolve to persevere in the war. "To take it [the tyranny]," he says, "perhaps was wrong, but to let it go is unsafe" (II.63.3). Cleon uses the term "tyranny" in another context to convince the Athenians to show severity in punishing the rebellious Mytileneans. He does not concede, as does Pericles, that taking the empire "perhaps was wrong." He defends its justice and warns the Athenians that showing mercy "will not oblige them so much as pass sentence upon yourselves. For if they were right in rebelling, you must be wrong in ruling" (III.40.4).

The difference in argument corresponds to a difference in rhetorical styles—Cleon is renowned for his verbal brutality—and to profound differences in character. Thucydides praises Pericles, the "first citizen" (II.65.9), and disparages Cleon, the "most violent man in Athens" (III.36.6). Yet, despite the contrasts, Thucydides suggests that there is some measure of continuity between them. Both sustain the public's attachment to empire. Both claim realistically to assess what that attachment requires in terms of policy and action. Both counsel steadfast resolve in carrying out policies once they are adopted. Both urge assemblies not to reopen debate on policies simply because their execution proves difficult.[5] For Euben (1990, 178), "given Thucydides' singularly explicit castigation of Cleon and his equally explicit praise of Pericles, it is disconcerting to find so many similarities between them" (see Hornblower 1987, 122ff; Hussey 1985).

In the turmoil following the debacle at Syracuse, Athens itself falls victim to the violence of tyranny. A part of the polis turns tyrannical toward the other, just as the Athenians had turned tyrannical in their treatment of their "allies" (Connor 1984, 179–80). But when this happens, the city is victimized not only by the actions of tyrannical leaders but also by the harsh moral character that the people of Athens themselves acquire through their dealings with their empire and begin to display in their dealings with one another. The parallel that Thucydides draws between Pericles and Cleon leads us to suspect that Pericles, too, bears some responsibility for the dissolution of Athenian political life in suspicion, mistrust, and violence.

This observation invites us to examine more closely the influence of Periclean politics on Athenian virtues. Periclean politics may not have promoted social cohesion among the Athenians as effectively as we are first led to think. Indeed, the narrative as a whole implies that Pericles' policies may (perhaps unwittingly) have accelerated the disintegration of Athenian solidarity and thus contributed to defeat. The one issue on which the narrative is thoroughly consistent, after all, is the attribution of that defeat to "intestine disorders" (II.65.12), factionalism, and growing atomization. For the Athenians, affective attachment to their status as members of the polis—their sense of democratic citizenship—pivoted on a norm of reciprocity between individuals and the polis. They imagined democratic citizenship to be characterized by mutual benefactions performed by both parties, citizens and city. In their civic self-image, a thriving democracy was not characterized by the prevalence of self-sacrificing individuals devoted to the common good. Rather, it required the successful cultivation of practices that provided multiple opportunities both for citizens (regardless of social and economic standing) to perform benefactions for the polis and for the collective—the city—to perform benefactions for the citizens. The Athenians extolled devotion to the common good in their civic self-image, but they also represented private interests and public interests as mutually obtainable. This is not to suggest that real citizens never encountered conflicts between private and public interests. Thucydides' *History*, as well as tragedy and many other literary sources, indicate that such conflict was frequent. Nevertheless, the ideal of citizenship as reciprocal exchange both defined a powerful political norm and acted as a force for social cohesion. This image of citizenship was expressed and celebrated on numerous civic ritual occasions during which individual affective ties to the polis were nourished.[6]

Thucydides makes Pericles acknowledge that Athenian democratic ideology pivots on such a view of private/public relations: "I am of the opinion that national greatness is more for the advantage of private citizens than any individual well-being coupled with public humiliation" (II.60.2). In the funeral oration, that recognition informs praise for the Athenian way of life (Monoson 1994). Yet, Pericles' war policies, however brilliant, require that the Athenians behave in ways that depart from this ideal of reciprocity. As he relates the effects of those policies, Thucydides invites us to observe how they involve the Athenians in behavior that manifests a growing tension between their civic self-image and their actual conduct and, therefore, how those policies threaten a normative understanding that had functioned powerfully to nurture Athenian solidarity.

Pericles' call to the citizens to collect behind the city walls is strategically brilliant but socially corrosive. The policy does not reward the Athenians for the suffering

sustained analysis of these passages, see Connor (1977b). For other parallels between the speeches of Pericles and Cleon, see Connor (1984, 79, n. 1).

[5] This is the position of Cleon in the Mytilenean debate and of Pericles regarding the Megaran Decree and the strategy of collecting behind the walls.

[6] The centrality of reciprocity in Athenian democratic ideology is increasingly noted in recent scholarship. See Farrar (1993), Kurke (1991), Millet (1991), Monoson (1994), Ober (1989), and Seaford (1994). See also Monoson (n.d.) for sustained discussion of reciprocity in the Athenian democratic imaginary.

they must endure in these unusual circumstances in the same way that the city traditionally rewards them for the more common sacrifice of the body in combat. In Athenian practice, dying honorably in battle—contributing one's body to the polis (II.43.1)—is met by particular action on the part of the polis. The soldier knows beforehand that, if he perishes, the polis will reward his "contribution" to the collectivity by attaching honor to his memory, by burying him with pomp at public expense, and by assuming certain responsibilities for his family, such as educating his orphaned children and supporting his elderly parents.[7] Pericles' policy proposal, in contrast, does not require the city to express any explicit gratitude toward the individuals who sacrifice their property rather than their bodies to the polis. Instead, Pericles exhorts citizens to subordinate their private interests to a future public good. "Cease . . . to grieve for your private afflictions and address yourselves instead to the safety of the commonwealth," he urges (II.61.4). Pericles not only enjoins the citizens to make extraordinary sacrifices but also minimizes their importance: "You may think it a great privation to lose the use of your land and houses . . . [but] you should really regard them in the light of the gardens and other accessories that embellish a great fortune and as, in comparison, of little moment" (II.62.3).

In defense of his policy recommendations, Pericles makes no attempt at all to fit the citizens' acceptance of property loss into a model of reciprocal exchange. He may implicitly promise participation in the enduring glory and pleasures that attend citizenship in a dominant polis, but he does not stress any "exchange." Rather, he uses his rhetorical skills to shore up the citizens' resolution and their tolerance of "private afflictions" (II.61.4), offering himself as an example. He is resolute, though he, too, has lost property. He admonishes the Athenians to be strong like him: "I am the same man and do not alter; it is you who change . . . the apparent error of my policy lies in the infirmity of your resolution" (II.61.2). But he fails to appreciate that the Athenians have only himself, an extraordinary charismatic leader, to help them achieve such resolve.[8] Their daily activity—camping out, often in deplorable conditions, behind the walls—does not provide them with the customary symbolic reinforcements and expiations that would make self-sacrifice on this scale tolerable. Though Pericles succeeds in assuaging the anger and frustration of the Athenians, Thucydides reports that "still, as private individuals they could not help smarting under their sufferings, the common people having been deprived of the little that they ever possessed, while the higher orders had lost fine properties with costly establishments and buildings in the country and, worst of all, [had] war instead of peace" (II.65.2). Only when Pericles helps them vent their anger by accepting a fine do they finally "become less sensitive to their private and domestic afflictions" (II.65.4).

After Pericles' death the Athenians lose the ability to subordinate their private concerns to the pursuit of victory. Their new leaders, Thucydides suggests, do not supply the model and the rhetorical reinforcements they need in this regard. The Athenians lost the war because they "allowed their private ambitions and private interests . . . to lead them into [unwise] projects" (II.65.7). They fail utterly to remain fixed on the public interest and fall prey to civil discord. Pericles' policy contributes to that failure by involving the Athenians in a departure from dominant normative understandings.

PERICLES AND THE POWER OF CHANCE: THE PLAGUE

Pericles is an extraordinary leader acting in circumstances as unusual as himself. But aspects of Thucydides' narrative invite us to ask if the exceptional circumstances are not to some extent of Pericles' own making. This is not to diminish the effect of events more or less beyond Athenian control, particularly the eruption of conflict between Corinth and Corcyra. But we must recognize that Pericles' response to the events leading up to the war—abandonment of the countryside and engagement in a potentially protracted war of attrition—is an uncommon one.[9] Thus, when the high mortality suffered by the Athenians crowded behind the city walls during the plague—an unforeseen consequence of Pericles' uncommon policy—takes away the sole leader who might have held the polis together under exceptional circumstances, we are forced by Thucydides' narrative to question the wisdom of Pericles' strategically brilliant and apparently prudent course of action. Aspects of Thucydides' narrative style, moreover, lend rhetorical emphasis to those interrogations.

Recall that Pericles recommends in his first speech that the Athenians fight a war of attrition, confident that they, because of their strategic skill, exceptional wealth, and naval supremacy, will prove able to withstand the strains of war far longer than the Peloponnesians. Pericles' recommendation that the Athenians forsake the countryside is a startling one. But he makes it sound reasonable to the Athenians, and they support it despite its unattractive features. They are confident

[7] The Athenian polis celebrated and displayed the generous way in which the city met these responsibilities toward orphans in the "preperformance rituals" that opened the dramatic competitions on the occasion of the grandest civic festival of the year, the City Dionysia. See Goldhill (1987) and Winkler (1985).

[8] Perhaps the success of Pericles' policies was thereby predicated on the subversion of normal institutions of Athenian democracy. This is one way to read Thucydides' famous comment regarding Pericles' relation to democratic institutions: "What was nominally a democracy became in his hands government by the first citizen" (II.65.9). Pericles' ability "at once [to] restore them [the multitude] to confidence" (II.65.9) regarding a previously chosen policy was also a capacity to oppose the democratic impulse to rethink, reconsider, reflect, and possibly decide anew. As Saxonhouse (1996) argues, steadfast devotion to a policy is a stance uncharacteristic of democratic assemblies—their distinguishing mark is the tentative, reversible status of all decisions.

[9] During the Persian Wars, the Athenians were compelled to abandon the city by the press of combat. No such compulsion obtains at the time of Pericles' proposal.

of victory and, the reader is tempted to think, with good reason. The modern reader may see in Pericles' policies an instance of prudent realism: He diminishes the city's vulnerability to attack by land and invests in its advantage at sea. But in order to find Pericles' strategy irresistible it is necessary to discount the power of chance (*tychē*). Thucydides does not suggest that Pericles ignores *tychē*. Pericles refers to it explicitly in his first speech (I.140.1). But Thucydides does suggest that he may not appreciate its full import. Pericles sees chance as the source of possible sufferings that could threaten public resolve, but he is apparently unaware that it can be the source of deeper and more enduring effects.[10]

The plague provides the most devastating example of the power of *tychē* in the entire *History*. Thucydides emphasizes the plague's importance by placing its narration, with almost no transition, after Pericles' funeral oration. The brutal juxtaposition swiftly and effectively contrasts the idealized image of Athenian patterns of life articulated in the funeral oration with the actual behavior of the Athenians during the plague. In the oration, Pericles portrays the Athenians as enjoying, among other things, "ease in our private relations" (II.37.3); "[lawfulness], particularly such as regards the protection of the injured, whether actually on the statute book or belong[ing] to that code which, although unwritten, cannot be broken without acknowledged disgrace" (II.37.3); mutual trust among citizens; and confidence in their good judgment, public spiritedness, and courage. Several times he praises the Athenians for being able to endure ordeals with dignity: "We are still willing to encounter danger; we have the double advantage of escaping the experience of hardships in anticipation and of facing them in the hour of need as fearlessly as those who are never free from them" (II.39.4). He boasts: "I doubt if the world can produce a man, who where he has only himself to depend upon, is equal to so many emergencies and graced by so happy a versatility as the Athenian" (II.41.1).

When the plague hits, Thucydides reports that people initially struggle to nurse the sick and dying but often end up succumbing to the illness themselves (II.51.2). The Athenians try to bury bodies as best they can, but the majority collapse under the strain of the disease (II.52.2–4). The plague soon causes social customs (*nomoi*) to break down and give way to multiple forms of "lawless extravagance" (*epi pleon anomias*) (II.53.1). In dramatic contrast to the orderly and elaborate public funeral at which Pericles delivered his famous speech, the Athenians now neglect to perform proper burial rites for their dead: "The bodies of dying men lay one upon another. The sacred places in which they had quartered themselves were full of corpses of persons that had died there, just as they were" (II.52.3). The Athenians panic in the face of the disease, giving the lie to Pericles' boast about their steadfastness in an emergency. They lose confidence in their judgment and act not as self-reliant and responsible citizens but, Pericles chides them, as slaves (*douloi*) (II.61.3). The rebuke is ironic, for Pericles earlier used the threat of enslavement (*doulōsin*) to justify resistance to Sparta and the adoption of his extraordinary plan (I.141.1). Athens took up that resistance but then failed to elude the servile condition that resistance was supposed to prevent. Fear and the absence of manly virtue drive the Athenians slave-like to pursue immediate personal pleasures for as long as their health permits in utter disregard for the law and social norms. "Perseverance in what men called honor was popular with none.... Fear of gods or law of man there was none to restrain them" (II.53.3). Far removed from the idealized Athens of the funeral oration, we now see the despair of a people besieged, casting aside its recollection of virtue and obligation. "The plague offers the most violent challenge to the Periclean attempt to exert some kind of rational control over the historical process" (Parry 1969, 116).

Thucydides sheds more doubt on Pericles' prudence by linking his strategy of abandoning the countryside to an intensification of the trials of the plague. "An aggravation of the existing calamity," Thucydides notes, "was the influx from the country into the city, and this was especially felt by the new arrivals. As there were no houses to receive them, they had to be lodged at the hot season of the year in stifling cabins, where the mortality raged without restraint" (II.52.1–2). Thucydides reports, moreover, that the plague first appeared among the Athenians only days after the Spartan invasion of Attica had begun. Athenian readers would have observed (and many would have remembered) that the plague came at what was already a most stressful time, as they gathered behind the walls and struggled to accept that their fields, livestock, orchards, and homes were, at that very moment, being ravaged by an enemy no less invisible than the plague. To underscore the severity of the Athenians' situation, Thucydides matter of factly reports that the plague never entered the Peloponnesos (II.54.5). Even though "it was actually asserted that the departure of the Peloponnesians [who enjoy the luxury of mobility] was hastened by the fear of the [plague]," Thucydides writes, "in this invasion they remained longer than in any other, and ravaged the whole country, for they were about forty days in Attica" (II.57.1–2). The reader cannot resist wondering if Pericles' understanding of prudential conduct is not somehow flawed. Pericles' embarrassment is made manifest in the more accusatory tone and rhetoric of the third speech. As spokesman for civic values in the funeral oration, he speaks of "we" and "us," but in his third and final speech he belabors the contrast between "me" and "you." References to "us" disappear (Connor 1984, 65).

Pericles' own fate highlights the limits of his power

[10] See Edmunds (1975) and Orwin (1994, 25, n. 28). Orwin's position is closer to the one we defend here. Cornford (1907, 82–109) discusses the contrast between chance (*tychē*) and foresight (*gnōmē*) in the narration of the events at Pylos. We focus here on the plague because it receives such extensive treatment in the text and because it assumes such importance in Thucydides' narrative of the rapid decay of social norms. Connor (1984, 51) provides other instances of Pericles' possible lack of foresight. Cawkwell (1997, 44–5) suggests that insofar as Pericles' policy succeeds, it does so only accidentally.

to control the course of events. In Thucydides' report of the reception of Pericles' third speech, we learn that Pericles' eloquence, reputation, and forceful personality help the Athenians regain their composure and commitment to the war in the face of the devastation wrought by the plague: "He succeeded in convincing them; they not only gave up all idea of sending to Sparta but also applied themselves with increased energy to the war" (II.65.2). Thucydides never suggests that Pericles himself is not equal to the emergency, but in the midst of describing the positive effects of his third speech on the Athenians, Thucydides almost casually informs us that Pericles, too, has perished. "He outlived [the war's] commencement two years and six months" (II.65.6). Pericles might have helped the Athenians negotiate this unforeseen event, but the power of the unforeseen makes a dramatic reappearance. Pericles is dead. Thucydides could have included some description of events in Athens during the year or more that separates Pericles' third speech from his death. He could have included some narration of the circumstances of that death, but in doing so he would have eased the reader into the knowledge of Pericles' loss. Instead, the narrative leaves the reader asking: "Wait a minute, did Pericles just die?" Thucydides empowers us to experience vicariously the shock and confusion that the Athenians of the period must have felt. The text enables us not only to know but also to feel the power of chance. Thucydides' treatment of Pericles, the most gifted of Athenians, has, through the antithetical encounter of praise and disappointment, the effect of nourishing a corrosive skepticism regarding humanity's very capacity to master its destiny through the use of its intellectual powers.

HUMANITY'S DEPENDENCE ON NORMS OF MORAL CONDUCT

It would be hasty to conclude that the skepticism in Thucydides' treatment of Pericles resolves itself in "mere tragedy," in unalleviated pessimism regarding the capacity of humankind to master its destiny.[11] By our reading, Thucydides does not so much doubt the capacity of human intelligence to master the course of history as he doubts humanity's capacity to chart a prudent course of action while disregarding norms of moral conduct. Thucydides leads the reader to appreciate how Pericles' overweening confidence, both in his own abilities and in the advantages of his city, breeds in him and his compatriots the illusion that power has endowed them with immunity from the sanctions of norms of moral conduct, which, in Pericles own words, "although unwritten, cannot be broken without acknowledged disgrace" (II.37.3). Suspicious of Pericles' self-confidence, Thucydides invites us to observe how prudent action relies on the guidance and restraint provided by moral norms.

The most telling discussion in all of Thucydides of the conditions under which deliberate and prudent conduct is possible is found in the famous passage on the Corcyraean civil war, or *stasis* (III.70–83). There Thucydides describes how factional conflict besets Corcyra as a result of war. The opposing factions, each emboldened by the prospect of reinforcement by an invading power, become increasingly harsh toward one another until normal political interaction is no longer possible. Violence ensues, and "the Corcyraeans engaged in butchering those of their fellow-citizens whom they regarded as enemies" (III.81.4). Under such conditions, Thucydides explains, it became increasingly difficult, and finally impossible, for both "states and individuals" (*poleis* and *idiōtai*; III.82.2) to conduct themselves prudently, moderately, and deliberately. Instead, the circumstances of *stasis* drove them to extremes, to behave with "reckless abandon" (*tolma gar alogistos*; III.82.4) and "frantically" (*emplēktos*; III.82.4). Thucydides characterizes the situation as one in which actors are unable to practice any measure of "hesitation" (*mellēsis promēthēs*; III.82.4), "moderation" (*to sōphron*; III.82.4), and "practical intelligence" (*to pros hapan xuneton*; III.82.4).[12]

Thucydides does more than assert that war makes these things happen. He shows us the mechanics of the link. War encourages actors to expect help from foreign sympathizers, freeing them from the need to treat their adversaries with consideration (III.82.2). As it disrupts normal human commerce, war makes actors desperate because it "takes away the easy supply of daily wants"; "plunges" cities and individuals into dire necessity;[13] and ushers in a situation in which words have no stable meaning (III.82.4). Sincere good naturedness (*to euēthes*) is laughed out of court (III.83.1), and there is no trust (III.83.2).[14] War disrupts the norms on which we rely for some measure of predictability and civility in daily life, the conditions of rational, deliberate, prudent action.

Thucydides' precise words at III.82.1 help us appreciate the subtlety of his insight. He pauses in his narration to note that revolution at Corcyra, being one of the first revolutions to occur, made a strong impression. "Later on, one may say, the whole Hellenic world (*pan to Hellēnikon*) was convulsed (*ekinēthē*)." What

[11] See Euben (1990, 194, n. 52) for an accounting of the camps (pessimist versus modernist) into which fall many interpreters of Thucydides' treatment of Pericles.

[12] We depart from Strassler's (1996) translation of *to xuneton*, "ability to see all sides of a question," by proposing "practical intelligence," which conveys the notion of commonly understood virtue. See I.79.2, where the term is used to describe the Spartan leader Archidamus, who uses the term himself (I.84.3) to designate a characteristic virtue of the Spartans and a reason for their stability and success in war. It appears again (II.15.1) in reference to Theseus, the mythic founder of Athens as a unified polis. Thucydides applies the term, always positively, to a handful of revered historical Greek figures, including Themistocles (I.74.1 and I.138.2–3) and the Peisistratids (VI.54.5). He also applies it to actors of his history who cut a good and often brilliant figure, such as Brasidas (IV.81.2) and Hermocrates (VI.72.2), as well as in discussing the oligarchs of 411 (VIII.58.4) and by implication Pericles (II.34.6, 8). See Gomme (1945–81) II.49.377.

[13] Thomas Hobbes offers "plunges" in translation of *piptein* (Grene 1989).

[14] *To euēthes* combines "*eu*" (good) and "*ethos*" (custom or habit), and it thus designates etymologically the moral virtue of being good as a matter not of calculation but of habit.

kind of convulsion did the whole Hellenic world suffer and with what consequences? The verb *ekinēthē* (a form of *kineō*, the root of "kinetic") suggests movement and instability. The context lends the word a negative connotation, captured nicely by "convulsed." The image is one of chaos, terrible flux, a destructive kind of motion. Yet, the verb *kineō* as such does not connote that negative image. In the first paragraph of the *History*, for example, Thucydides uses the noun *kinēsis* in an apparently neutral way. He explains that the war was "worthy of relation" because it was "the greatest movement" (*kinēsis megistē* I.1.2) in history.[15] This movement engulfed all the Hellenes, the barbarians, and the better part of humankind (*pleiston anthrōpōn*). It is the physical dimensions of the "movement" (*kinēsis*) of people and materials that initially caught Thucydides' attention and made him think the war worth writing about. What, then, endows the term *ekinēthē* with negative connotations later on (III.82.1)?

It is true that the marshaling of physical force for civil war may lend the verb a certain negative force in the Book III passage. But the intestine nature of the war is not the primary source of the negative connotation, for now Thucydides directs our attention to a more intangible yet more powerfully destructive "movement." People and cities are losing their reserve, the power to check their motion. They are acting on impulse and fear. The things that war is now setting in motion, or stirring up, are not only men, money, resources, and ships but also the very customary understandings of what constitute "normal" human values and relationships. War is causing "the distortion of morality and values" (Hornblower 1987, 156; see also Connor 1984, 102, n. 57). The breakdown of the norms that undergird social relations, captured in Thucydides' observation that even words lose their ordinary meanings, is leading to chaos, violence, and the pursuit of immediate wants without regard to their moral value (*pleonexia*). When those norms are "convulsed," as they are at Corcyra, one cannot expect reason to provide a useful guide to action. Reason requires that words convey meaning, that the norms to which we commonly refer in judging the actions, intentions, moral character, and trustworthiness of others are still generally employed.

The Corcyraean *stasis* has the same effect as the Athenian plague on humankind's capacity to act reasonably. At Athens, when physicians found themselves unable to stem the tide of plague, reason (*logos*) "was overpowered" (Connor 1984, 100). The same condition obtains in Corcyra. For Thucydides, war is a "violent teacher" (*biaios didaskalos*; III.82.2, translation modified) because it instructs by setting in motion, poltergeist-like, that which in times of peace is usually at rest. Thucydides opposes this *kinēsis* of social norms antithetically against *hēsychia*, his term for designating peace. (*Hēsychia* literally means quiet or tranquility; the more common term for peace is *eirēnē*.[16]) Thucydides employs the *kinēsis/hēsychia* antithesis to highlight the parallel between *stasis* at Corcyra and plague at Athens. In both instances he opposes an image of *kinēsis* with an antithetical image of *hēsychia*. In the case of the plague, the image of *hēsychia* is developed in the funeral oration. In the case of the Corcyraean *stasis*, it is developed in the description of the Athenian purification of the island of Delos, with its long excerpt from Homer's *Hymn to Apollo* (III.104.1–6). In both instances, Thucydides compels the reader to ponder the contrast.

PERICLES AND THE *KINĒSIS* OF CUSTOMARY NORMS

War and *stasis* destabilize norms and meanings and render reasonable, prudent conduct difficult if not impossible to achieve. But it would belie the complexity of Thucydides' text to call war and *stasis* the cause of that destabilization. Indeed, war and *stasis* themselves can, to a considerable extent, be seen as the consequence of a prior subversion of moral norms. Thucydides, at V.25.1, inverts the variables. The Peace of Nicias obtains, but Corinth and other Peloponnesian cities are trying mightily to "disturb" the arrangement. The verb employed here is *diekinoun* (which appends the prefix *dia* [thoroughly] to *kineō*, discussed above). The episode invites us to take a longer view, to ask if the peace that obtained before the outbreak of war was not similarly being disturbed. We hark back to the broad indictment of Athens by the Corinthians (I.70.1–9). There we find the "daring, dynamism, innovation, audacity, immoderation, presentism, and frenetic motion" (Euben 1990, 190[17]) characteristic of *stasis* being applied to the conduct of Athens in peacetime: "They were born into the world to take no rest (*hēsychian*) themselves and to give none to others" (I.70.9).

The corrosive effect of "frenetic motion" saps the capacity of the Athenians as well as other Greeks to act with prudence and deliberation as the story progresses. Early in the *History*, moral norms inform the debate between Corinth and Corcyra. Moral argument provokes some hesitation on the part of the Athenians before entering into the fateful alliance with Corcyra (I.31–44; see Euben 1990, 167, 175–7). Moral scruples again cause the Athenians to rethink their policy toward the rebellious Mytilenians. They still possess the power to pause, to listen to argument, to reconsider, to check their motion and moderate their conduct. Yet, the hold of norms is clearly weakening. As

[15] Connor (1984, 21, n. 4) observes that *kinēsis* "is an unusual word at this point in the development of Greek, clear enough in general meaning but obscure and surprising in this [I.1.2] context." The inference is, therefore, that it was not lightly or casually employed by the author.

[16] Thucydides generally uses the term *eirēnē* to designate formal peace treaties between cities (as well as the absence of war that results) and *hēsychia* to designate the absence of turmoil, both within the city and in relations among cities. At VI.38.3 he explicitly contrasts *hēsychia* with *stasis* and the condition in which fellow countrymen behave toward one another as toward an enemy (*polemios*). Note that the application of *hēsychia* is not limited to domestic peace (V.55.1–2). For *eirēnē*, see I.29.4 and V.13.2, 14.1, and 26.2; for *hēsychia* see I.70.9, II.22.1, and V.22.2 and 53.

[17] Euben never associates those characteristics specifically with Pericles.

Euben points out, Diodotus, the advocate of restraint and decency, is compelled by the press of argument and the hubris of imperialism to "defend the more moderate human policy in relentlessly realistic terms that anticipate the Athenian arguments at Melos" (Euben 1990, 180; see Bagby 1994; Orwin 1984; Saxonhouse 1996). This argument is self-defeating in the long run. Diodotus' feigned realism saves the Mytilenians but legitimates the idea that empire, *archē*, is of necessity as oppressive as it is ineluctable and thus subverts confidence in the legitimacy and value of moral discourse. Diodotus thus unwittingly invites those Athenians who may still be willing to subject action to rational examination to shed their regard for norms of decency in favor of a facile, normatively disembodied strategic calculus. By the time of the Melian dialogue, "a travesty of dialectic" (Hornblower 1987, 62; see also Connor 1984, 87–8; Euben 1990, 198),[18] the process of decay is complete. Language is not used for purposes of deliberation but as an instrument of siege. "Athens is left with self-interest alone, the desire for power without a culture to give it bounds and meaning. Not only is ambition of this sort unlimited, it is incoherent and irrational; for without a comprehensible world there can be no way of reasoning about it or acting within it" (White 1984, 79).

Thus, the norms and understandings that provide guidance to everyday social intercourse are subverted not only by violence but also by "peacetime" politics and political discourse—peacetime, that is, in the sense of *eirēnē* rather than *hēsychia*. Pericles, we can now see, plays no mean role in this history of subversion. He is "not only at home in wartime, and tolerate[s] its climate of aggression, but even foster[s] it" (Pouncey 1980, 36). At I.127.3, Pericles "urges" (*hōrma*) the Athenians to war. The verb *hōrmaō*, which Crawley translates as "urge" (cf. English "hormone"), literally means to set in motion or to rush at something or someone, as in battle. The use of this term further supports our view that Thucydides regards Pericles as a source of *kinēsis*. He takes pride in making "every land and every sea . . . the highway of our daring" (II.41.4). Thucydides portrays Pericles as a source of *kinēsis* and, indeed, embellishes on the historical record to do so (see Lebow 1991, esp. 158, n. 4; Kagan 1989).

In a passage particularly revealing of Thucydides' complex attitude toward Pericles (VIII.97.2), the short-lived Athenian experiment with a mixed constitution is described thus: "For the *first time in my experience* the Athenians appear to have governed themselves well."[19] The judgment clearly embraces the government of Pericles and "implies censure of Pericles' management of affairs" (Hornblower 1987, 160). It revives our initial suspicions regarding Pericles' statecraft. By pandering to the population, by challenging the customary understandings of the bond between citizen and polis, by implementing an extraordinarily innovative and bold war strategy that uprooted established patterns of life, by offending the allies' reverence for *autonomia* (political autonomy), Pericles is a source of *kinēsis* and contributes significantly, though perhaps unwittingly, to the deterioration of norms of social conduct. This strategy not only panders to Athenian *pleonexia*, or desire for more power and wealth, but also, by making Athens invulnerable to external assault, tries to free it from the need to cultivate "normal" relations with other Greek cities. It also tries to free the conduct of the war from the need to secure the customary way of life of the majority of Athenian citizens, who are compelled to abandon that way of life to seek refuge sine die behind the city walls.[20]

But as Pericles and the Athenians undermine the legitimacy and influence of moral norms through rhetorical flair, brazen strategy, and unadorned violence, they not only create a revolutionary world that loses its moral compass but also prove ironically quite incapable of flourishing in the world they create. In the end, the Athenians suffer as much as anyone from the collapse of political order throughout Greece and the concomitant decay of "those general laws to which all alike can look for salvation in adversity" (III.84.3; cf. Pericles at II.37.3).[21] The tragic lament of Nicias following the defeat at Syracuse (VII.77.1–7), with its ironic use of language previously employed at the Melian conference, makes this point with pathos.

The *History* offers a sustained exposition of humanity's dependence on moral norms—not for philosophically transcendent reasons but because they are the necessary support of prudent conduct. When those norms are in shambles, the protagonists of the *History* find it virtually impossible to chart a prudent course of action. "Thucydides' history is unquestionably aimed at an audience that values cleverness, sophistication, intellect, and self-interest, but it does not simply affirm and reinforce those values" (Connor 1984, 15). *Gnōmē* (foresight or rational calculation) trips on *tychē* (chance) or is coopted by *orgē* (passion) (Connor 1984, 55–9, esp. 58, n. 18). In some instances, "sound calculation leads to a poor outcome" (Kauppi 1991, 18; cf. Connor 1984, 118); in others, carelessly concocted schemes, such as Cleon's assault on Pylos (IV.27–41), succeed beyond hope. We see the actors of the *History* treat morality and prudence as if they were rival terms. But Thucydides invites us to consider at what cost they do so.

[18] See Connor's treatment of the Melian dialogue (1984, 147–55): "Whatever our reactions to what happens to the Melians, it is hard to escape a feeling of horror at what is happening to the Athenians." (p. 154).

[19] Translation from Connor (1984, 228 and n. 34), emphasis added (in conformity with the text, which stresses "my experience" with the enclitic particle *ge*). See the discussion of translation problems in Hornblower (1987, 160), who also (pp. 166–8) gives a critical examination of Pericles' stewardship, as do Lebow (1991, 254–9) and Palmer (1992, 113).

[20] See Ober (1991, 254–9) on the development of a defensive ideology in Athens.

[21] Though the authenticity of III.84 is disputed, we are convinced by Connor's defense of the passage (1984, 102, n. 60).

THE ALTERNATIVE TO PERICLEAN POLICY

What are the norms, and how free was Pericles to respect them? The first question challenges us to identify specifically "international" norms, since we have already discussed the importance of norms such as reciprocity in domestic political life. This is an important issue for international relations scholarship, because "international norms" are at the core of one of the most abiding claims of realist theory: Morality is unachievable without first securing "political order" through the exercise of power (Carr 1964, chapter 9; Morgenthau 1946; Wight 1960). It follows from this claim that we can discuss morality meaningfully only as it obtains "inside" states. It is not meaningful "outside" states, in the anarchical realm of international politics and war.

G. E. M. de Ste. Croix (1972) offers a reading of Thucydides that lends some support to the realist argument. But in general the stark dichotomization between "inside" and "outside" does not hold up well in Thucydides' text, which provides grist for the mill of scholars in international relations who take the distinction to task (Kratochwil 1989; Walker 1993). It is beyond the bounds of this article to explore in any depth the norms that regulated relations among Greek *poleis*, but it is important to verify the existence of such norms in the text. Indeed, Thucydides' claim, advanced in his analysis of the Corcyraean *stasis*, that both private persons (*idiōtai*) and states (*poleis*) found prudent action increasingly difficult to achieve constitutes a rather explicit rejection of the "inside/outside" dichotomy. The text reveals here and there that a number of norms of war and diplomacy regulated the exchange of the dead (III.113.1–5; IV.97.2–98.8), the treatment of prisoners and refugees, Olympic truces, diplomacy through proxenies, the obligations of allies, and so on (see Hornblower 1987, 180).[22] Perlman (1991) shows how the term *archē* evolved to become synonymous with *dynamis*, *despoteia*, and even *tyrannis*, precisely because Athens showed such disregard for the norms of *eleutheria* (freedom) and *autonomia*, which, like sovereignty and *cuius regio* in the Westphalian system, were constitutive of the Greek polis-based "international system." Indeed, Thucydides seems to experience the war as a kind of civil war within a normatively unified space. Hermocrates' efforts to forge cohesion among Sicilians, a people who "go by the same name" (*onoma hen keklēmenous*; IV.64.3), serve as a foil to the civil war that is raging on the mainland among people who are also designated by a single name, "Hellenes" (Connor 1984, 126, n. 42). Within this unified normative space, the actors in the drama, as well as the reader, are clearly able to form judgments of the moral character of leaders of opposing cities, that is, across political frontiers, on the basis of shared understandings of such goods as justice, honor, mercy, and other common virtues—at least until those norms lose sway under the combined blows of an overweening self-confidence and self-sufficiency, war, and *stasis*. It is only when those norms become inoperative that the reader finds it difficult to develop a firm grasp of the moral character of such figures as Alcibiades and Phrynichus, who dominate the final books of the *History*.

Do we really mean to argue, then, that Pericles ignored opportunities to legitimate, reaffirm, and defend such norms of moral conduct by adopting the policy course he did? The claim gives pause because scholarship in international relations, goaded by Thucydides' claim that the war was "inevitable" (I.23.6),[23] has become accustomed to trying to understand the text in terms of "necessary outcomes." It therefore seems odd to think of Pericles as something other than an extraordinary leader confronting the great forces of history. But, in addition to Lebow's (1991) well-argued complaint that the popular "power transition" reading of "necessity" is not much supported by the text, it is simply impossible to identify any recurring set of factors in the *History* that render some pattern of outcomes "necessary." It is true that "the Archeology" (Thucydides' digression on the pettiness of previous history in comparison with this great war [I.2–23]) seems to suggest such a theme by alerting the reader to the role of "historical forces" in the genesis of the *History*. But Thucydides' interest in "historical forces" evident in the Archeology all but vanishes in the narrative of the war. "There is, in Thucydides, no systematic treatment of Spartan and Athenian weaknesses other than those of character" (Hornblower 1987, 33). As the narrative unfolds, the attention paid to individual moral character grows rather than diminishes. In the final books, the historian's attention seems to fixate on the moral character of two figures: Nicias and Alcibiades. By paying so much attention to character, Thucydides seems to embrace the belief that "individual human action can change the course of history" (Hornblower 1987, 146; see also Kauppi 1991, 112).

What, then, could Pericles have done otherwise? The question should probably be resisted because it distracts from the text and entangles us in potentially unresolvable debates about counterfactuals. For purposes of illustration only, we will mention one thing, among others, that Pericles might have done otherwise: He could have devised an honorable way to revoke the Megara Decree. Thucydides reports (I.139.1) that lifting the embargo against Megara may well have satisfied Sparta and forestalled (and perhaps avoided) war. In his speech, Pericles acknowledges that some (or many) Athenians look on the Megara Decree as "a trifle" (*to brachy ti*), and he suggests that by refusing this trifle Athens takes a bold and significant step that demonstrates its resolve (I.140.4, 5). But Pericles, perhaps because he authored the "trifle," does not entertain the possibility that to concede it might prove as constructive as refusing it proves provocative. Lifting

[22] At IV.97–8 is an interesting debate regarding what should be regarded as custom (*nomos*) and what should not. See also Connor (1984) on Mytilene (p. 91) and on Nicias (p. 163); and Ducrey (1986, chapter 9) on treatment of prisoners of war.

[23] The use of "inevitable" for *anangkasai* is controversial. See Connor (1984, 32, n. 31).

the decree would do more than satisfy the Spartans. It would convey Athenian commitment both to the norm of *autonomia* that attaches to the *polis* and to the sanctity of its treaty with Sparta, and thus enhance the authority of normative restraint. It would, moreover, challenge Sparta to reciprocate. Nor would it foreclose the ability of Athens to prosecute its complaint against Megara. The Athenians might be obliged to accept Sparta as a party to that prosecution, but given Megara's poor relations with Corinth and its record of diplomatic fickleness, Sparta might well lend Athens a sympathetic ear. In the event of a rebuff, the decree (which apparently did not contravene the treaty) could be reimposed. Pericles' refusal to give this course of action serious thought seems to flow as the unexamined consequence of his principled rejection of any and all meaningful negotiation with the Spartans (I.127.3, 144.3; II.12.2). It also may have its source in the hubris of a "first man" who simply cannot contemplate reversing himself.

HERMOCRATES AT GELA: REALISM, PRUDENCE, AND MORAL NORMS

Thucydides' demonstration of our dependence on moral norms dilutes his realist credentials. In a provocative article, Forde (1995, 154) has suggested that Thucydides even counsels us to "resist" realism. But our alertness to Thucydides' unrelenting recourse to antithetical reasoning makes us pause before advancing categorical statements regarding his teaching. Nicias, after all, resists realism, and the outcome is less than impressive. Recall, moreover, the Archeology's account of the mastery and accumulation of political and military power by the Greeks, the devolution of that power on Athens as set out in the Pentekontaetia (Thucydides' discussion of the growth of Athenian power during the fifty years between the Persian and Peloponnesian wars at I.88–118.2), and Thucydides' amazement at the dimensions of the *kinēsis* with which he introduces the *History*. How could we expect normative restraints to operate effectively when history confers such an overwhelming advantage on a people? The claim that power politics is ineluctable pervades the *History*. In the words of Diodotus, "it is impossible to prevent, and only great simplicity [*euētheias*] can hope to prevent, human nature [*tēs anthrōpeias physeōs*] doing what it has once set its mind upon [*hormōmenēs*], by force of law [*nomōn ischyi*] or by any other deterrent force whatsoever" (III.45.7). The difficulty with Thucydides is finding a coherent rule or prescription in the relentless succession of antithetical claims. How does one reconcile the claim that prudent politics requires guidance by moral norms with the claim that power politics is inevitable? Looking to the text, we find a clue in the remarkable speech delivered by the Syracusan Hermocrates in Book IV.

Hermocrates wants to promote reconciliation between two feuding Sicilian cities, Gela and Camarina, in order to avoid Athenian intervention. He begins by making clear Athenian interest in the feud. Athens, he claims, seeks to ally itself with certain Sicilian cities only as a first step toward subjugating the entire island. Moreover, those ambitions are, he states in a realist-sounding utterance, "very excusable," since "it is just as much in men's nature to rule those who submit to them as it is to resist those who molest them" (IV.61.5). Hermocrates urges the two cities to settle their differences so that Sicily can unite to dispel the Athenian threat. But why should the cities of Sicily join in the collective defense of their island rather than bandwagon or balance by individually joining either the Athenian or the Spartan alliance? Realism counsels only the course of action that is most likely to succeed at the lowest cost. Hermocrates, however, evokes the sentiment of Sicilian identity: "We are neighbors, live in the same country, are girt by the same sea, and go by the same name of Sicilians" (IV.64.3). But what is so important about being "Sicilian"? It is not a source of nationalist sympathy or solidarity. Indeed, national sentiment is held in check by the more ancient rivalry between Dorians and Ionians. The idea of Sicily, for Hermocrates, refers rather to a geographic space where, through the repeated interactions of its cities, a set of shared norms has evolved to facilitate interaction, deliberation, and the settlement of grievances. "Do you not think that the good which you have," he asks, "and the ills that you complain of, would be better preserved and cured by quiet [*hēsychia*] than by war" (IV.62.2)?

Hermocrates greets with skepticism the self-assurance that is bred of power and strategic cleverness. He asks his listeners not to fall prey to the illusion that strength is sure because it is confident (*euelpi*) (IV.62.4). Given that skepticism, he equates prudent action with the defense of Sicily as a normatively coherent and functioning space. He likens war among Sicilian cities to civil war. He evokes civil war using the forceful term *oikeios polemos* (IV.64.5) rather than the more technical *stasis*. The expression produces a powerful image of war in the household (*oikos*). He invites his listeners to feel terror (*ekplagentes*), not only in the presence of the Athenians but also before "the undefined fear of this unknown future" (IV.63.1). *Ekplagentes* means frightened out of one's wits and is laden with rhetorical force. The image of civil war, that is, the destruction of a space and of the facilities for prudent action that space provides, should inspire terror. Terror, in turn, should inspire concessions on the part of the feuding cities in order to preserve that normative space. Hermocrates' argument evokes with irony the words of Pericles, who sought not only to stir confidence in Athenian power and strategic skill but also to inspire disdain for the adversary: "Disdain [*hyperphron*]," Pericles states, "is the privilege of those who, like us, have been assured by reflection of their superiority to their adversary" (II.62.4). In this same vein, Hermocrates' invitation to make concessions contrasts sharply with Pericles' principled rejection of meaningful negotiation.

But Hermocrates realizes that power can cause people to "confuse their strength with their hopes" (IV.65.4). He realizes that the temptation to test one's

power is strong. The defense of Sicily as a normative space will therefore probably require the countervailing application of military power. Hermocrates is not a peace activist. He is trying to forge an alliance. He admits, as does the realist, that violence is inevitable or "natural" in human affairs. He admits that Sicily's cities will probably wage war on one another again (IV.64.3). But he conceptualizes prudent action as participation in that violent struggle with the goal of preserving the social norms that make prudent action possible. He does not invite us to resist realism but to apply the realist's alertness to the permanence of power and violence in human affairs in defense of norms of moral conduct. Pericles, in contrast, knows how to acquire and apply power, and Thucydides values that mastery. But what is missing in Pericles is a full appreciation of what one might do with power such that the normative conditions for its intelligent and prudent application can be sustained.

Hermocrates' speech and policy seem antithetical to Pericles' earlier speeches and policies. They do not engage one another immediately, but they do engage one another at the level of theory. We submit that the antithesis between Pericles and Hermocrates contains a prescriptive lesson. At the same time, however, we acknowledge how difficult it is to pin Thucydides down. Sicily does not unite for long. Hermocrates, moreover, may merely be seeking to assert Syracusan hegemony over the island (though the exercise of the hegemonic power is not per se incompatible with the defense of norms). At another time he employs a remarkably different language that emphasizes ethnic solidarities: "We protest that we are menaced by our external enemies the Ionians, and are betrayed by you our fellow Dorians" (VI.80.3). And when the Athenians finally invade, the Sicilians meet them in disarray. Inversely, Thucydides employs all his rhetorical skills to concentrate our minds on Hermocrates' words at IV.59–64 and lend those words centrality. The scene provides us with our first introduction to Sicily and Syracuse, the principal instruments of Athens's defeat, and to Hermocrates, a central figure in that defeat. At Gela, moreover, Hermocrates gives the only speech, a privilege that compels the comparison with Pericles. Finally, his speech follows immediately and antithetically on the Athenian triumph at Pylos, an unexpected victory and the highwater mark of the city's fortunes.

It is for all these reasons, and because our suspicions are supported by so many other elements of the text as a whole, that we believe the antithetical opposition between the two statesmen, Pericles and Hermocrates, contains a prescriptive lesson. We might formulate that lesson in three parts: Power can be entrusted only to people (1) who enjoy a healthy skepticism regarding their own (and humanity's) capacity for clever strategy, (2) who are inclined by that skepticism to be respectful of social norms, which they value for the prudent guidance they provide, and (3) who will use power to defend (or help craft) those norms, ever alert to humanity's tendency to fall victim to the illusion that power confers freedom from moral restraint.

THUCYDIDES AND CONTEMPORARY THEORY

Both the prescription and the skeptical premises in which it is lodged directly engage modern theorizing in international relations. Thucydides' skepticism, as conveyed by his antithetical style of analysis, invites us to revisit realism, whose ties to skepticism have been noted but not systematically explored (see Beitz 1979, chapter 1; Cohen 1985; Loriaux 1992). For example, while doubting the claim that there is such a thing as a "realist tradition," Walker (1993, 17) nevertheless emphasizes the skepticism that is common to the great thinkers who are usually assigned to that tradition. He writes of a "Rousseauean skepticism," a Machiavellian challenge to "universalist pretentions," a Weberian acquiescence "in a complex and widespread skepticism about modernity" (Walker 1993, 4, 47, 56, 110). Realists rely on skepticism regarding humanity's aptitude for moral improvement to justify their attention to power politics and self-help. Pericles himself, by this definition, is a realist. Indeed, the label fits not only his attitude toward policymaking but also the philosophical premises underlying that attitude. Pericles, like Thucydides, had ties to the Sophists. He was closely linked with Protagoras, and his consort Aspasia animated a lively intellectual "salon" in which Sophistic thought flourished. Pericles may have drawn more or less self-consciously on Sophistic skepticism in order to "clear the decks" of received wisdom and thus make room for his strategic innovations.

But Thucydides' subtle critique of Pericles questions the realists' ability to ground their political philosophy in skeptical premises. Skepticism in Thucydides spreads from doubt regarding humanity's capacity for moral improvement to doubt regarding humanity's capacity to flourish while ignoring or defying moral norms. Thucydides excites such skepticism even when statecraft is, as in Pericles' case, apparently prudent, and even when the conceptualization of the policy is, as in Pericles' case, brilliant. It is difficult to think of a realist argument that might stave off this metastasis of skepticism from doubt regarding human morality to doubt regarding the human intellect's self-sufficiency. Thucydides, in any event, "makes it clear where he stood: it is not with the sophists who denied the validity of any principle of morality but a short-sighted self-interest" (Hornblower 1987, 185, citing Gomme 1945–81).

Thucydides' invitation to appreciate how norms function as a condition for prudent action also intersects with the new constructivist literature on norms and law (Finnemore 1996; Katzenstein 1996; Klotz 1995; Kratochwil 1989; Lynch n.d.; Wendt 1992). The *History* shows how norms can function in a nondeterminist manner to provide guidance to deliberate and prudent action. Thucydides' reliance on a skeptical outlook suggests a way that proponents of constructivism might philosophize about the foundations of that

perspective.[24] Inversely, his acknowledgment of the inevitability of power relations should inspire caution in constructivists as they mount their assault on realist theory. His account of politics in extraordinary times suggests that we can, and should, assess the possibility of supporting or unsettling norms through material struggle.

We cannot develop these ideas here, but we raise them to recall Thucydides' capacity to confront each generation's particular theoretical enthusiasm with challenging and even deadly objections. In our view, his capacity to resist encapsulation—not his putative status as the founder of one or another school of thought—remains the reason his work stands as a "possession for all time."

[24] Kratochwil (1981) sets out from David Hume. There is a parallel between the argument developed here and Hume's own rediscovery of the norms of the "common life" through the exploration of skeptical premises. Compare Michael Oakshott's influence on Terry Nardin (1983).

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The Public's Conditional Response to Supreme Court Decisions

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*To investigate the effect of the Supreme Court on public opinion, we offer the conditional response hypothesis based on a theory of Supreme Court legitimacy and a microlevel social-psychological theory of attitude formation. Together these theories predict that the Court may affect public opinion when it initially rules on a salient issue, but that subsequent decisions on the same issue will have little influence on opinion. To test our predictions, we analyze public opinion data before and after the Supreme Court ruled in a highly visible abortion case (*Webster *v.* Reproductive Health Services *[1989]) and before and after three key capital punishment rulings (*Furman *v.* Georgia *[1972],* Gregg *v.* Georgia *[1976], and* McCleskey *v.* Kemp *[1987]). The results suggest that our theory is not issue bound but is generally applicable to how the Supreme Court affects public opinion when it rules in highly salient cases.*

In the years leading up to the Supreme Court's decision in *Roe* v. *Wade* (1973), debates over abortion became quite heated, but judicial scholars would have predicted a diminution in the high level of public controversy after the Court ruled. Indeed, the prevalent theory of the day was that when the Supreme Court made decisions, the public simply accepted them as legitimate. This theory—established by Dahl (1957) and labeled the positive response hypothesis by Franklin and Kosaki (1989)—suggests that there was nothing more to discuss after *Roe* because the Court had established a clear constitutional right to abortion on demand during the first trimester of pregnancy. Whether or not an individual agreed with this position before *Roe*, she would at least accept it as law after the decision because the Supreme Court was seen as the ultimate arbiter of the law. If this were the rule and not the exception, however, then the public debate concerning abortion should have dissipated in the wake of the *Roe* decision. Clearly, history does not support this conclusion.

While *Roe* provides only one example of how the positive response hypothesis fails to capture the Court's ability to affect public opinion, it suggests to many scholars (e.g., Franklin and Kosaki 1989; Hoekstra and Segal 1996) that Court decisions do not necessarily decrease public controversy. Sometimes public debate increases in the wake of Supreme Court decisions. This leads us to believe that the traditional theory of the relationship between Supreme Court decisions and public opinion is suspect.

We offer a theoretical framework that approaches the puzzle of how the Supreme Court affects public opinion in a novel manner. Specifically, we seek to answer when, if ever, the Court affects public attitudes when it makes decisions. We derive our framework from theories of Supreme Court credibility as well as from a variation on a social-psychological model that suggests that (1) individuals form opinions about an issue when they are provided with information about it, and (2) once individuals form opinions, they do not readily change their mind. With this foundation, we generate predictions about how and when the Court will influence public attitudes about two specific issues: abortion and the death penalty.

EXISTING THEORIES

Judicial specialists offer two competing theories to explain the Supreme Court's effect on public opinion. The first, initially advanced by Dahl (1957), focuses almost exclusively on how the Court legitimizes its own policies or those of the other two branches of government. Building on Dahl's theory, a number of scholars hypothesize that when the Court takes a position on an issue, overall public support for that position inherently increases (for a complete review of this literature, see Hoekstra and Segal 1996). In other words, because the Court is perceived as the ultimate arbiter of the law, its decisions are viewed as legitimate, credible, and therefore correct. This theory, labeled the positive response hypothesis by Franklin and Kosaki (1989), predicts that the public listens to the Court and always supports its decisions.

Franklin and Kosaki (1989) empirically test the predictions of the positive response hypothesis and demonstrate that it cannot account for how the structure of public opinion toward abortion changed in the wake of *Roe* v. *Wade* (1973). In its stead they offer the structural response hypothesis, which predicts that in highly salient cases the Court's policy choices do not necessarily translate into an aggregate increase in positive attitudes toward its position on that issue. Rather, while citizens often accept the Court's choices, their reactions are affected more strongly by the political context within which they live (see, e.g., Huckfeldt and Sprague 1995). In addition, the structural response hypothesis predicts that the Court has a microlevel

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A previous version of this manuscript was presented at the 1996 annual meetings of the Southern Political Science Association, Atlanta, Georgia, and won honorable mention in the 1996 Congressional Quarterly competition for the best graduate-level paper in the field of law and courts. The authors thank Lee Epstein, Valerie Hoekstra, Christina Wolbrecht, and the anonymous reviewers for their comments and helpful insights throughout the writing of this manuscript. However, all remaining mistakes are our own.

effect on public opinion because it increases the intensity of within-group opinions about particular issues. The general point is that the Court does not necessarily legitimate particular policies (although it may, if aggregate public opinion favors a particular policy choice before a decision) but affects attitudes by bringing issues into the public discourse.

The empirical evidence presented by Franklin and Kosaki (1989) is quite compelling. They find that levels of support for abortion on demand diverged between groups (e.g., Catholics versus non-Catholics; whites versus nonwhites) in the wake of *Roe*. More specifically, the difference between these groups changed by approximately 50% as a result of the Court's decision (p. 762). This leads Franklin and Kosaki to posit that the effect of *Roe* was further crystallization of issue preferences and greater homogeneity of within-group beliefs (p. 762). From this they conclude, in line with the structural response hypothesis and contextual theory, that the Court's decision polarized group attitudes toward abortion.

Despite these strong findings concerning public opinion toward *Roe*, two key questions arise about the applicability and generality of Franklin and Kosaki's model. Does the Supreme Court continue to affect public attitudes when it rules on a salient issue more than once? If so, do between-group attitudes become more polarized each and every time the Court rules on that issue? In order to understand the extent to which the Court affects public opinion, we must come to terms with how and when the public reacts to decisions in more than one case and in more than one issue area. To find the answers we turn to theories about Court credibility (e.g., Caldeira 1986; Caldeira and Gibson 1992) and to social psychological theories of attitude formation and change (e.g., Chaiken 1980; Fiske and Taylor 1991; Petty and Cacioppo 1981; Petty and Krosnick 1995).

INFORMATION, CREDIBILITY, AND ATTITUDE CHANGE

We provide an alternative to the positive response hypothesis and an extension of the structural response hypothesis. More specifically, we offer a variation of the social-psychological elaboration likelihood model to explain how the Supreme Court affects public opinion. Our model is based on two different theories. First, because the public generally views the Court as a highly credible institution (Caldeira 1986; Caldeira and Gibson 1992; Hoekstra and Segal 1996), individuals more clearly elaborate their attitudes toward an issue after a ruling (although they do not necessarily agree with the decision). Unlike the positive response hypothesis, however, our account suggests only that the public pays attention to and discusses major decisions because of the Court's high degree of legitimacy. In other words, as a credible source of information, when the Court makes its first major decision on a particular issue, the structure of public opinion changes in a manner consistent with the structural response hypothesis. That is, even if individuals disagree with a particular decision, their opinions on that issue will crystallize (become stronger) for or against the Court's policy choice.[1]

To support this part of our model, we draw on research demonstrating that even people who know very little about the Supreme Court often hold it in high regard. For instance, Caldeira and Gibson (1992, 635) argue that "the Supreme Court has traditionally fared well in the estimations of the public." Other works suggest that individuals perceive the Court as highly legitimate simply because it is the ultimate arbiter of the law (Adamany 1973; Adamany and Grossman 1983). While we take issue with the conclusions of these latter works, the authors do make a point with which we agree: The public would never even pay attention to the Supreme Court if it were not viewed as a legitimate institution. Thus, as Hoekstra and Segal (1996, 1097) point out, individuals may hold well-defined attitudes toward an issue, but the Court will still have an effect on those opinions because of its high degree of legitimacy.

The first part of our model also draws on two key studies from social-psychological theory. Chaiken (1980) studies the persistence of attitude change based on different source and message characteristics. Specifically, she is interested in the likelihood of attitude change when an individual is presented with the same information more than once. To test this process Chaiken uses an experimental setting to explore how attitudes change when individuals are presented with similar information on two separate occasions. She finds that, after subjects heard the same information a second time, "there was a trend for all subjects to show less persuasion" (quoted in Petty and Cacioppo 1981, 259). Furthermore, those presented with information about an issue of high consequence (highly salient for them) were even less likely to change attitudes.

Chaiken's work is consistent with the elaboration likelihood model, a commonly used model of persuasion, which demonstrates that once an issue is elaborated to the extent that individuals form opinions, further elaboration, even if it is in conflict with their opinion, will tend not to change that opinion (Petty and Cacioppo 1981). Petty and Cacioppo (p. 263) find that the intensity of cognitive processes influences the propensity for attitude change: "In studies where issue relevant cognitive activity was likely to be intense ... the attitude changes produced have been found to be relatively enduring." Together with the theories about Court credibility, this work establishes the first part of our model.

The second part of our model relies exclusively on the elaboration likelihood model. We argue that once the Supreme Court helps individuals elaborate their opinions, subsequent decisions within the same issue area—even if they overrule an initial landmark decision—will have little effect on public opinion. In short,

[1] Of course, there are other reasons a first major decision may affect public opinion. One alternative explanation is that the media are much more inclined to cover a new landmark decision (see, e.g., Slotnick, Segal, and Compoli 1994), and the public response may be related to the great amount of coverage.

even though the Court is still viewed as a credible institution, people will not change opinions formed on the basis of the Court's prior decision.

In sum (1) the Supreme Court is viewed as a credible and legitimate institution; (2) individual's opinions on certain issues crystallize when the Court decides its first major case in a highly salient issue area because those opinions are affected by the Court's policy choices; and (3) opinions about the Court's policy choices are unlikely to change once they are set, even when the Court rules again on the same issue. We call this the *conditional response hypothesis* and posit the following two hypotheses:

HYPOTHESIS 1. *When the Supreme Court makes its first major (landmark) decision in a salient issue area, increased cleavages will occur in between-group attitudes about an issue, and there will be an overall change in public opinion toward that issue.*

HYPOTHESIS 2. *When the Supreme Court decides subsequent cases within the same issue area, little if any change will occur in the structure of group attitudes toward that issue regardless of the* direction, intensity, or breadth *of the decision, and there will be no overall change in public opinion toward that issue.*[2]

Each hypothesis is tested separately. Hypothesis 1 involves cases in which the Court makes its first landmark decision in a given issue area. Hypothesis 2 involves subsequent cases concerning the same issue. The findings of Franklin and Kosaki (1989) serve as an initial test of Hypothesis 1, since *Roe* was the Supreme Court's first major and highly visible abortion decision. Another highly visible abortion case (*Webster* v. *Reproductive Health Services* [1989]) is used to evaluate Hypothesis 2. This analysis is meant to discern whether the Court had a similar effect on public attitudes toward abortion in both cases. We argue that *Webster* is an ideal case with which to test our second hypothesis since it was the first (and arguably the best) chance to strike *Roe*, thus making it highly controversial.[3]

DATA AND MODELING STRATEGIES

Data for our test are drawn from two CBS News/*New York Times* polls (1989a, 1989b) concerning public attitudes toward abortion during summer and autumn 1989.[4] The first poll was conducted two months prior to the *Webster* decision, and the second three months after. With data of this nature (about the same issue and of approximately the same importance as *Roe*), we can test Hypothesis 2.[5]

Our dependent measure is the number of circumstances, from a list of possible choices, in which a respondent would permit a legal abortion. Because the first survey listed five possible scenarios for abortion, and the second listed seven, we construct a scale from 0 to 2 and match only the common reasons from each survey, ranging from 0 (no instances of legal abortions) to 2 (two or more instances of legal abortions). While this is not a perfect solution, it still captures the circumstances under which an individual believes abortion should be legal. With these data, we construct two separate abortion scales: One reports responses to *Health-related* reasons for abortion, and the other reports responses to *Discretionary* abortions (see the Appendix for definitions of these two scales).[6]

To explain how group attitudes may have changed after *Webster*, we specify a model with several independent measures (*Education*, *Gender*, *Race*, *Catholic Adherents*, and *Frequency of Church Attendance*) to capture the extent to which attitudes toward abortion are affected by the Court. Not only are these variables theoretically pleasing, but also, because Franklin and Kosaki (1989) invoke them, we can make direct comparisons to their findings. Furthermore, these variables allow us to draw out the relationships among group membership, individual characteristics, and attitudes toward abortion.

We also include a dummy variable, d_1 (which we interact with all the independent variables), to discern whether attitudes prior to the decision change afterward. This *Postdecision Dummy* equals 0 before the decision and 1 afterward, so that it may interact with

[2] Implicit in both of these hypotheses is the following null hypothesis: When the Supreme Court makes a decision, there will be no changes in the structure of public opinion. The null hypothesis affects how we can interpret our results. In our tests of Hypothesis 1, statistically significant results can lead us to reject the implicit null hypothesis, thus yielding credence to Hypothesis 1. For our tests of Hypothesis 2, statistically insignificant results allow us to maintain the null (fail to reject the null), thus yielding credence to Hypothesis 2. We have worded our hypotheses positively to enhance the readability of the paper.

[3] There were other cases before *Webster* (e.g., *Akron* v. *Akron Center for Reproductive Health* [1983]), but *Webster* was still the best chance because the Court finally had a conservative majority. As Justice Blackmun said prior to *Webster*: "Will *Roe* v. *Wade* go down the drain? I think there's a very distinct possibility that it will, this term. You can count the votes" (Epstein and Walker 1995, 462). Aside from being the best opportunity to overturn *Roe*, *Webster* was important for showing how the Court would view specific state regulations on abortion. More specifically, in *Webster* the Court had to decide the constitutionality of a Missouri law which stated that (1) life begins at conception; (2) public employees could not perform abortions not necessary to save the mother; (3) public employees could not counsel women to choose abortion; and (4) physicians must test women to be sure the gestational age of the fetus is less than 20 weeks (adapted from Epstein and Walker 1995, 463). Finally, *Webster* was so controversial that 78 amicus curiae briefs were filed, representing more than 5,000 interested parties. All this suggests that *Webster* was highly salient and controversial.

[4] These data were provided by the Inter-university Consortium for Political and Social Research. The two studies used are the CBS News/*New York Times Monthly Poll* (1989a); and the CBS News/*New York Times Abortion Polls* (1989b). The recoded data may be obtained from the authors to replicate this study.

[5] Because the interest is in the short-term effect of the decision, it is important and intuitive to use data as close to the decision date as possible. We argue that our data are quite good because they were obtained within months of the Court decision.

[6] Because we do not have exact matches for the pre- and postdecision samples, and because there are fewer health questions in the predecision sample, we had to modify Franklin and Kosaki's (1989) technique. Thus, our index combines the responses, so that we had a scale ranging from 0 (abortion should not be permitted under any circumstances), to 1 (in only one instance should abortion be legal), to 2 (in two or more instances abortion should be legal). Although this is not a perfect solution, our marginal distributions on both abortion scales are quite similar to those of Franklin and Kosaki (1989). Thus, this technique does not seem to bias our results.

the $\beta_{2,k}$ coefficients (the postdecision individual characteristics). As Franklin and Kosaki note (1989, 755–6): "The coefficients in this model are specified so that they may vary between the predecision and postdecision samples." The equation we estimate, then, is the following:

$$y = \alpha_0 + \alpha_1 d_1 + (\beta_{1,1} + \beta_{2,1} d_1)x_1 + \ldots + (\beta_{1,k} + \beta_{2,k} d_1)x_k + \varepsilon. \quad (1)$$

The second equation we estimate constrains d_1 to 0, thereby suppressing the interaction effect with the $\beta_{2,k}$ coefficients.

$$y = \alpha_0 + \beta_{1,1}x_1 + \ldots + \beta_{1,k}x_k + \varepsilon. \quad (2)$$

We only invoke this constrained model (and report the results) for use in a likelihood ratio test, which helps us explain whether an overall change occurs in aggregate group feelings toward abortion after the Court's decision.

Because our dependent variable consists of discrete ordinal numbers (0, 1, or 2), traditional linear regression is an inappropriate method for testing our hypotheses. Indeed, Greene (1993, 672) notes that OLS errs by taking the discrete rankings and treating the differences as the same, and he argues that a standard logit or probit model also cannot account for the ordinal nature of the dependent variable. He contends that (under certain conditions) an ordered probit model is the best way to estimate models with discrete dependent variables (p. 673). We therefore turn to this model, first offered by McKelvey and Zavonia (1975), because it takes into consideration the ordinal nature of the data by treating each observation only as a rank ordering. We estimate these models using maximum likelihood estimation (see McKelvey and Zavonia 1975 or Greene 1993 for the derivation of this model and its likelihood function). In each model, a positive coefficient indicates an increased likelihood that a respondent believes more instances for abortion should be legal, while a negative sign indicates that she thinks there should be fewer instances of legal abortion.

A comparison of the constrained and unconstrained models allows us to test whether the postdecision characteristics are jointly constrained to 0 (meaning the overall structure of opinion did not change because of *Webster*). Therefore, the difference in the $\beta_{1,k}$ and $\beta_{2,k}$ coefficients before and after the decision tests the extent to which the structure of between-group attitudes changes as a result of a particular Court decision. As Kmenta (1986, 491) suggests, "if we wish to test the validity of nonlinear restrictions rather than assuming it *a priori*, we can use . . . the likelihood test." We can observe the difference in the likelihood function between the constrained and unconstrained models, which is −2 times the change of the log of the likelihood function, distributed as chi-squared, with degrees of freedom equal to the number of constraints. The results of this test allow us to make different claims about each hypothesis. When testing Hypothesis 1 we expect at least some of the postdecision $\beta_{2,k}$ coefficients to be statistically significant, α_1 to remain small, and the likelihood ratio test between the constrained and unconstrained models to reach statistical significances.[7] If this happens, then we can reject the implicit null hypothesis that the structure of opinion did not change. For Hypothesis 2, however, we expect few if any of the postdecision $\beta_{2,k}$ coefficients to be statistically significant, α_1 still to remain small, and the likelihood ratio test not to reach statistical significance. These results would maintain Hypothesis 2—shifts in public attitudes did not occur when the Court ruled in subsequent cases in the same issue area.

RESULTS

As stated above, we accept the findings of Franklin and Kosaki (1989) concerning the public's reaction to *Roe* as support for Hypothesis 1. That is, the public in general reacted to the Court's decision, and the views of several social groupings (such as Catholics and non-Catholics) became strongly polarized. Our first test, then, focuses on Hypothesis 2. We estimate a model for those respondents who had heard of the Court's decision in *Webster* (see the Appendix for the distributions of all variables used for the model).[8] Table 1 presents the constrained and unconstrained models for both the discretionary and health-related abortion scales. The sample includes all the respondents in the predecision sample but only those in the postdecision sample who said that they had heard of *Webster*. Note that while some of the predecision signs may seem counterintuitive (Catholics have a positive sign in the discretionary model), only education and church attendance are significant predictors of polarized attitudes toward abortion prior to *Webster*. Those with higher education are more supportive of abortion than those with lower education, and those who attend church more frequently are less supportive than those who attend less frequently.[9]

To demonstrate whether attitudes toward abortion changed as a result of the Court's action, we turn to the postdecision coefficients in the unconstrained model. As our second hypothesis predicts, there is little or no change in the estimated coefficients from the predecision to the postdecision attitudes. The most striking change is that the sign on Catholics who had heard of *Webster* becomes negative after the ruling in the discretionary model. This change is marginal evidence, at

[7] The null hypothesis is that none of the coefficients will be different from zero, so if more than one is statistically significant, then there is support for our hypothesis.

[8] We follow Franklin and Kosaki (1989) in this respect because, if we did not distinguish between those who heard of the *Webster* decision and those who did not, then we would be left to answer the same questions, namely, "How do we sort out the impact of the decision from the spurious influence of other factors? To what do we attribute the change?" (Franklin and Kosaki 1989, 754–5). By truncating the sample and only using those who heard of the decision, we should be able to parcel out the effect of the Court's decision. Thus, we only run this model for those who had heard of the decision.

[9] As do Franklin and Kosaki (1989), we fix the church attendance coefficient because preliminary analysis showed no tendency whatsoever for this effect to change in the wake of *Webster*.

TABLE 1. Estimated Abortion Responses for Respondents Who Have Heard of *Webster* (Ordered Probit Estimates)

	Models for Discretionary Abortions		Models for Health-Related Abortions	
Parameters	Unconstrained	Constrained	Unconstrained	Constrained
Constant	.36*	.27*	2.02***	1.92***
	(.15)	(.13)	(.18)	(.16)
Postdecision Dummy	−.15	.43***	−.52	−.02
	(.32)	(.09)	(.36)	(.09)
Education	.16**	.19***	.13*	.17***
	(.04)	(.04)	(.05)	(.04)
Education Postdecision	.14	—	.15	—
	(.09)		(.10)	
Female	.11	.15*	.11	.13
	(.08)	(.07)	(.09)	(.08)
Female Postdecision	.15	—	.06	—
	(.17)		(.19)	
Nonwhite	.15	.20	−.07	−.04
	(.14)	(.13)	(.16)	(.14)
Nonwhite Postdecision	.28	—	.16	—
	(.31)		(.34)	
Catholic	.10	.02	−.09	−.09
	(.10)	(.08)	(.10)	(.09)
Catholic Postdecision	−.34	—	.04	—
	(.19)		(.22)	
Church Attendance	−.34***	−.35***	−.34***	−.34***
	(.03)	(.03)	(.03)	(.03)
Model χ^2	258.18	251.53	172.13	169.86
Cut Point	.25	.25	.85	.85
log (L)	−1,018.11	−1,021.44	−754.91	−756.04
Likelihood test χ^2	—	6.66	—	2.26
Significance	—	ns	—	ns

Source: CBS News/*New York Times Monthly Poll* April 1989; and CBS News/*New York Times Abortion Polls* September–October 1989.
Note: N = 1,251. *$p < .05$; **$p < .01$; ***$p < .001$. Standard errors are in parentheses.

best, of polarization between Catholics and non-Catholics ($t = 1.79$), which accords with Hypothesis 2. In general, these models perform quite well, reaching highly significant chi-squared values of 258.2 and 172.1 for the discretionary and health scales, respectively (d.f. = 10). No reliable evidence exists that the Court affected intergroup attitudes toward abortion with its *Webster* decision, however, because no postdecision coefficient is significantly different from zero.

To test whether the overall structure of opinion changed, we next compare the constrained and unconstrained models (refer to Table 1). The first test compares the discretionary abortion scale models, and it is evident that the overall structure of opinion did not change—the chi-squared value of the difference between the constrained and unconstrained models only reaches 6.66 with four degrees of freedom (equal to the number of constraints). This indicates little, if any, overall shift in attitudes toward abortion. The same holds for the health scale, for which the chi-squared value only reaches 2.26. Thus, in neither instance did the Court significantly alter the structure of public opinion toward abortion. While there is some evidence of the positive response effect in the health-scale model (all the signs become positive in the wake of *Webster*), the fact that no coefficient is significantly different from zero suggests little aggregate increase in support for health-related abortions.

The most interesting finding here is that Catholics (who might be expected most likely to oppose abortion, see Gilbert 1993) did not seem to crystallize their beliefs in the wake of *Webster*. Even on the health-related abortion scale—where one may predict a general increase in the level of support for abortion—the Court elicited scant reaction. Furthermore, those groups most likely to support abortion rights (Protestants, those with high education, and females) barely reacted as well. Attitudes seem to have neither changed in the aggregate nor polarized between groups as a result of *Webster*.

In general, these findings clearly demonstrate that people who had heard of *Webster* responded significantly less to the Court's action than did people who had heard of *Roe*. While Catholics who had heard of *Webster* became slightly more opposed to discretionary abortions after the ruling, the evidence in Table 1 still indicates little support for the notion that the Court significantly altered group attitudes in general. Rather, the public hardly responded, which suggests that when the Court rules following a major decision (*Roe*), the public is not likely to react to its policy choice.

A FURTHER TEST: THE DEATH PENALTY

The results of the previous test suggest that the structural response hypothesis (explicated here as Hypothesis 1) cannot account for the insignificant shifts in public opinion toward abortion after *Webster*. This tells us that the Court's ability to affect public opinion toward abortion may be conditional. In other words, the Court may only affect public opinion under specific circumstances. At this point, however, we cannot claim that our approach has explanatory power beyond this specific issue. It may be that the Court's effect is unique to abortion because it is such a salient issue for so many people. To explore the general applicability of our hypotheses, we must test them in another issue area. If the results of additional tests mirror those from the abortion cases, then we have compelling evidence that the Court's ability to affect public opinion is conditioned on the temporal ordering of its decisions. To discern whether this effect holds, we turn to another salient issue area, the death penalty.

We begin with an analysis of how the Supreme Court affected public opinion toward the death penalty with its first landmark (and highly publicized) ruling on the constitutionality of this punishment—*Furman* v. *Georgia* (1972). Then we analyze whether the Court affected public opinion in two of its many subsequent capital punishment cases: *Gregg* v. *Georgia* (1976) and *McCleskey* v. *Kemp* (1987). We expect that *Furman* will elicit significant change in group attitudes and also will alter overall public opinion toward the death penalty (Hypothesis 1), but that after *Gregg* and *McCleskey* there will be little change on either level (Hypothesis 2).

Data for the death penalty analyses are drawn from the General Social Survey cumulative file, 1972–94 (Davis and Smith 1994). The GSS conducted interviews from February to April in 1972, 1973, 1976, 1977, 1987, and 1988.[10] This allows for a comparison of public attitudes toward the death penalty several months before each of the decisions and then within a year afterward. The GSS also provides a number of useful measures for the purpose of testing our theory. Thus, we specify models that are very similar to the models used to test attitude change in the wake of *Webster*. The independent measures in these models are *Education*, *Gender*, *Race*, *Party Identification*, *Jewish Adherents*,[11] and *Frequency of Church Attendance*. These variables allow us to draw out the relationships among group membership, individual characteristics, and attitudes toward the death penalty. Again, we include a dummy variable to distinguish between pre- and postdecision attitudes.

The dependent measure is whether a respondent *Favors the Death Penalty* for convicted murderers.[12] The dependent variable is coded 1 (favors the death penalty) or 0 (opposes this punishment). Given the dichotomous nature of this variable, an alternate to regression analysis is again warranted. We use binary probit models, which attempt to account fully for the relevant causal factors (see Greene 1993, 635–51 for the derivation and discussion of the probit model), and we estimate these models using maximum likelihood estimation. As discussed in the abortion section, the comparison of the constrained and unconstrained models allows us to discern whether the Court affected overall public opinion toward the death penalty.[13]

OPINION CHANGE AND DEATH PENALTY CASES

In *Furman* v. *Georgia*, the Court issued a short per curium decision holding that Georgia laws governing the death penalty violated the Eighth Amendment protection against cruel and unusual punishment (see Epstein and Walker 1995). Table 2 presents the constrained and unconstrained models for public attitudes before and after *Furman*. Note that prior to this decision, females and nonwhites were significantly more opposed to capital punishment than were males or whites, and Republicans favored the death penalty more than did Democrats. These findings are intuitive and conform to existing theories concerning different group attitudes toward the death penalty (see, e.g., Epstein and Kobylka 1992).

The postdecision coefficients in the unconstrained model help determine whether the structure of public opinion changed as a result of *Furman*. As Hypothesis 1 predicts, the decision did elicit specific social cleavages in two areas: Those with higher education clearly became more opposed to the death penalty than did those with less education, while partisan attitudes

[10] These data were provided by the Inter-university Consortium for Political and Social Research (Davis and Smith 1994). Recoded data may be obtained from the authors.

[11] We include Jewish adherents for reasons similar to including Catholic adherents in the abortion model. Just as Catholics tend to have very conservative views on abortion, Jews tend to have very liberal views opposing capital punishment (see, e.g., Melton 1989).

[12] The wording of this question in the GSS changed after 1973. At the time of *Furman*, the question was: "Are you in favor of the death penalty for persons convicted of murder?" At the time of *Gregg* and *McCleskey*, the question was "Do you favor or oppose the death penalty for persons convicted of murder?" Since we are not comparing individual responses over time, the difference does not matter for our purposes.

[13] Since the GSS does not provide a question for the death penalty that asks whether a respondent had heard of the Court's decision, it may be difficult to sort out the influence of the decision from the spurious influence of other factors (see Franklin and Kosaki 1989, 754–5). By not excluding individuals who had not heard about the particular death penalty cases, our models incur some degree of bias, specifically against Hypothesis 1 and toward Hypothesis 2. This means that our findings in support of Hypothesis 1 (an effect from *Furman*) could be stronger than they appear if these individuals were excluded from our sample. In other words, if we could include only those who had heard of the decision, then we would expect much stronger changes in group attitudes in the wake of the decision, because we would capture only the attitudes of those who probably cared about the decision. Our results are still strong, which means we overcome this bias against Hypothesis 1. Unfortunately, the bias toward Hypothesis 2 (no effect from *Gregg* and *McCleskey*) cannot be easily overcome because when all those who had not heard of the decision are included in the sample, it is much easier to find no effect. In short, if someone had not heard of the decision, there is little reason to assume an attitude change as a result of the decision (see Franklin and Kosaki 1989, 760). Nonetheless, since these cases were quite salient and were part of the public discourse, we are confident that this bias is not severe.

TABLE 2. Estimated Capital Punishment Responses for *Furman* v. *Georgia* (Probit Estimates)

Parameter	Unconstrained Model	Constrained Model
Constant	.34***	.34***
	(.08)	(.07)
Postdecision Dummy	.14	.15**
	(.11)	(.05)
Education	−.02	−.08**
	(.04)	(.02)
Education Postdecision	−.13*	—
	(.05)	
Female	−.40***	−.40***
	(.07)	(.05)
Female Postdecision	.00	—
	(.10)	
Nonwhite	−.74***	−.69***
	(.10)	(.07)
Nonwhite Postdecision	.11	—
	(.15)	
Party Identification	.04*	.61***
	(.02)	(.01)
Party Identification Postdecision	.05*	—
	(.02)	
Jewish	.41*	.32*
	(.20)	(.15)
Jewish Postdecision	−.23	—
	(.30)	
Church Attendance	.02*	.02*
	(.01)	(.01)
Model χ^2	216.57	205.21
log (L)	−1,713.19	−1,718.87
Likelihood test χ^2	—	11.36
Significance	—	$p < .05$

Source: *General Social Surveys* 1972–94 (November 1994).
Note: $N = 2{,}716$. $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$. Standard errors are in parentheses.

TABLE 3. Estimated Capital Punishment Responses for *Gregg* v. *Georgia* (Probit Estimates)

Parameter	Unconstrained Model	Constrained Model
Constant	.75***	.77***
	(.09)	(.07)
Postdecision Dummy	.15	.12
	(.12)	(.05)
Education	−.10**	−.09***
	(.03)	(.02)
Education Postdecision	.16**	—
	(.05)	
Female	−.40***	−.35***
	(.07)	(.05)
Female Postdecision	.04	—
	(.10)	
Nonwhite	−.77***	−.78***
	(.12)	(.08)
Nonwhite Postdecision	−.02	—
	(.17)	
Party Identification	.04*	.02*
	(.02)	(.01)
Party Identification Postdecision	−.03	—
	(.03)	
Jewish	.18	.09
	(.27)	(.17)
Jewish Postdecision	−.18	—
	(.36)	
Church Attendance	.00	.00
	(.00)	(.00)
Model χ^2	153.49	152.06
log (L)	−1,619.47	−1.,620.19
Likelihood test χ^2	—	1.44
Significance	—	ns

Source: *General Social Surveys* 1972–94 (November 1994).
Note: $N = 2{,}784$. $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$. Standard errors are in parentheses.

toward capital punishment became more polarized. These findings support Hypothesis 1, as α_1 remains small, while some of the $\beta_{2,k}$ coefficients are large. Both models are significant, with chi-square values of 205.21 and 216.57, respectively.

To discern any overall shift in group attitudes toward capital punishment after *Furman*, we also compare the constrained and unconstrained models using the likelihood ratio test. The chi-square value for this test reaches 11.36 with five degrees of freedom (refer to Table 2). This suggests (with $p < .05$) that overall public opinion toward the death penalty changed as a result of *Furman*, the first major case in which the Court ruled on the constitutionality of the death penalty, which conforms to Franklin and Kosaki's (1989) finding based on *Roe*.

Since 1972 the Court has issued many additional opinions covering the death penalty, two of which are particularly important and salient: *Gregg* v. *Georgia* (1976) and *McCleskey* v. *Kemp* (1987).[14] In *Gregg* the Court declared that Georgia's new death penalty law, which stipulated a separate trial and sentencing hearing for specific crimes like murder, did not per se violate the Eighth Amendment. Several justices in the majority thought that this law was "a model death penalty scheme" (Epstein and Walker 1995, 631). Although this ruling did not overturn *Furman*, it affected death penalty laws throughout the states; many legislatures simply conformed current laws to the Georgia scheme. According to Hypothesis 2, we predict little or no response after *Gregg*, just as after *Webster*. Table 3 shows how public opinion changed after this ruling.

It is clear from an initial inspection of the model that there was little shift in group attitudes toward the death

[14] We use *Gregg* because it seemed to settle the debate over the death penalty, mainly by ruling that it does not violate the Constitution (Epstein and Walker 1995, 633). This change in Court policy, just two years after *Furman*, makes *Gregg* the next highly significant death penalty case. We use *McCleskey* because of its highly controversial issue of whether African-Americans receive the death penalty more often than others. This case was highly controversial and highly publicized. Since both cases were likely to elicit major reactions by the public, they are excellent tests for our conditional response hypothesis.

penalty after *Gregg*. The only group split is between those with high education and those with lower education.[15] No other postdecision independent measure indicates group cleavage. Although one might expect an exacerbation of the cleavage between Democrats and Republicans, and possibly between whites and nonwhites, none occurred.

The above finding, combined with the results of the likelihood ratio test, suggests that the Court had little effect on public opinion as a result of *Gregg*. Indeed, the chi-square for the likelihood test is nowhere near significant (refer to Table 3). We therefore conclude that the Court's decision had no significant effect on overall public attitudes, or on specific group attitudes, toward the death penalty.

In *McCleskey* v. *Kemp* (1987) the Court was again asked to rule on the constitutionality of Georgia's death penalty law. This time the issue focused on whether it violated the equal protection of African Americans, since statistical evidence documented that they were four times more likely to be given a death sentence in Georgia than were whites (see Epstein and Walker 1995, 634). Despite the strong evidence of a clear racial disparity in death sentences, the justices upheld the constitutionality of the Georgia law. Even though this case was highly controversial, we do not expect a pronounced public response. Table 4 explicates the results.

Compared to *Gregg*, there is even less evidence of social cleavage concerning the death penalty after *McCleskey*. There is marginal evidence that females became slightly more opposed to the death penalty in the wake of this ruling ($t = 1.81$), but no other postdecision cleavage is statistically different from zero. This indicates that the model in Table 4 comports with our conditional response hypothesis. Indeed, because the Court had already spoken about the death penalty issue, we expect little change in public attitudes despite the salience of this decision.

As a final test of Hypothesis 2, we perform the likelihood ratio test for the constrained and unconstrained models in *McCleskey* to determine whether overall public opinion moved after this ruling. The chi-square is quite small and therefore not significant (refer to Table 4). This suggests even more strongly that there was no discernible effect of this decision on public attitudes toward the death penalty.

In sum, our results indicate that the first time the Court rules on issues such as abortion and the death penalty, it affects between-group attitudes, and the overall structure of public opinion toward these issues.

TABLE 4. Estimated Capital Punishment Responses for *McCleskey* v. *Kemp* (Probit Estimates)

Parameter	Unconstrained Model	Constrained Model
Constant	.67***	.73***
	(.09)	(.08)
Postdecision Dummy	.21	.05
	(.13)	(.05)
Education	−.04	−.05*
	(.03)	(.02)
Education Postdecision	−.02	—
	(.04)	
Female	−.06	−.14**
	(.07)	(.05)
Female Postdecision	−.20*	—
	(.11)	
Nonwhite	−.67***	−.70***
	(.08)	(.06)
Nonwhite Postdecision	−.09	—
	(.13)	
Party Identification	.10***	.10***
	(.02)	(.02)
Party Identification Postdecision	.00	—
	(.03)	
Jewish	.53	.16
	(.40)	(.22)
Jewish Postdecision	−.55	—
	(.49)	
Church Attendance	−.02*	−.02*
	(.01)	(.01)
Model χ^2	317.41	311.64
log (L)	−1,553.88	−1,556.76
Likelihood test χ^2	—	5.76
Significance	—	ns

Source: *General Social Surveys* 1972–94 (November 1994).
Note: $N = 2{,}920$. *$p < .05$; **$p < .01$; ***$p < .001$. Standard errors are in parentheses.

This structural response is conditioned, however, as predicted by Hypothesis 2. Indeed, our analysis of *Webster*, *Gregg*, and *McCleskey* indicates that when the Court speaks more than once, its effect is minimal in later cases. These results strongly support the predictions of the conditional response hypothesis.

CONCLUSION

The Supreme Court can and does influence public attitudes toward highly salient issues, but its effect is conditional. At times the public will react (when an issue is initially brought to the forefront of political discourse by a landmark Court decision), but at others it will not (when the Court rules on an issue again). Despite the limitations of our data (different wording for the death penalty dependent variable, and the hard-to-match questions for the abortion dependent variables), our findings are quite consistent with our theory. Indeed, they suggest that the conditional response hypothesis has much more explanatory power

[15] The positive coefficient of the postdecision dummy on the education variable substantively implies that, after *Gregg*, individuals with more education supported the death penalty at a higher rate than before the decision. This may be due to the decision itself or to other macropolitical changes during the period between the surveys (such as a salient criminal trial that fostered pro-death penalty sentiments among the well educated). This statistically significant coefficient does not affect the explanatory power of the unconstrained model, however, as the likelihood ratio test between the unconstrained and constrained models does not approach statistical significance, which implies negligible change in the overall structure of public opinion toward the death penalty.

than previous views of how the Supreme Court affects public opinion in the United States.

These results indicate that judicial scholars should adjust the way they approach this political phenomenon. Franklin and Kosaki (1989) demonstrate that neither the positive response hypothesis, nor simple time-series analyses are productive. We agree. But there are three avenues of research that may provide even better insight into the influence of controversial Court decisions. First, studies should focus on attitude change after the Court hands down a first landmark decision in a particular issue area (such as *US Term Limits* v. *Thornton*, term limits; *Romer* v. *Evans*, equal protection for homosexuals; and *Reno* v. *ACLU*, pornography on the Internet), as this is the time when the greatest effect can be expected.

Second, our results complement Hoekstra and Segal's (1996) finding that changes in public opinion are likely to occur in close proximity to case origination. They find that the Court's ruling in *Lamb's Chapel* v. *Center Moriches Union Free School District* (1993) probably affected attitudes in the local community, where the public had abundant information about the decision, even though the case had national implications as well. Thus, attention should be given to areas in which prior elaboration has not occurred but in which information about the Court's decision will be readily available, highly relevant, and clearly salient.

The third avenue adds to the first two. Interrupted time-series analysis may demonstrate more clearly how specific group attitudes change over time. For example, an analysis of an issue on which the Court has decided many cases—such as school prayer—may determine whether the shock from each ruling influences aggregate public attitudes toward that issue. In this way, we may be able to discern the movement of opinion and how it is changed by both landmark and subsequent decisions within the same issue area, while controlling for other macropolitical factors. This would allow a comparison of how different decisions in different issue areas elicit similar or dissimilar public responses.

At the end of the day we believe that the conditional response hypothesis should be viewed as a general explanation for how the Supreme Court affects public opinion. Indeed, it suggests that, as a credible institution, the Court can highlight issues in the public discourse (as it did with *Roe* and *Furman*), thereby changing the structure of public attitudes. Despite its high degree of legitimacy, however, the Court's ability to affect public attitudes is muted when it alters, overturns, or reiterates its initial policy choices in subsequent cases.

APPENDIX

Abortion Cases

A. Dependent Variable

The pre-*Webster* sample was asked the following question: Please tell me whether or not you think it should be possible for a pregnant woman to obtain a legal abortion if:

- there is a strong chance of serious defect in the baby. (used in Health Scale)
- the woman's own health is seriously endangered by the pregnancy. (used in Health Scale)
- she is not married and does not want to marry the man. (used in Discretionary Scale)
- the family has a very low income and cannot afford any more children. (used in Discretionary Scale)
- the pregnancy interfered with work or education. (used in Discretionary Scale)

For the Post-*Webster* sample, the abortion question was as follows: Should it be legal or should it be against the law for a pregnant woman to obtain an abortion if:

- the woman's health is seriously endangered by the pregnancy. (used in Health Scale)
- the pregnancy is the result of rape. (used in Health Scale)
- there is a chance of a serious defect in the baby. (used in Health Scale)
- she is not married and does not want to marry the father. (used in Discretionary Scale)
- the family has a very low income and cannot afford any more children. (used in Discretionary Scale)
- the pregnancy would force the woman to drop out of school or interrupt her career (combined variable). (used in Discretionary Scale)

B. Scales for dependent measures

Discretionary abortions:

- 0 = Abortion should never be legal as a discretionary measure.
- 1 = Abortion should be legal in only one instance as a discretionary measure.
- 2 = Abortion should be legal in two or more instances as a discretionary measure.

Health-related abortions:

- 0 = Abortion should never be legal even as a health measure.
- 1 = Abortion should be legal in only one instance as a health measure.
- 2 = Abortion should be legal in two or more instances as a health measure.

C. Question used to truncate sample into those who had heard and those who had not heard about the Supreme Court's decision in *Webster* (and marginal distributions)

In July, the Supreme Court decided *Webster* v. *Reproductive Health Services*, a major case about abortion. Do you approve or disapprove of the Supreme Court's decision in this case, or haven't you heard enough about it to have an opinion?

- Respondents who either approved or disapproved: 1,251
- Respondents who did not have enough information to approve or disapprove: 1,587

D. Questions used for independent variables in abortion models (both samples)

- Catholic: What is your religious preference today?
- Nonwhite: Are you white, black, or some other race?
- Gender: Respondent's sex.
- Church Attendance: Do you attend religious services every week, almost every week, once/twice a week, once a month, a few times a year, or never?
- Education: What is the last grade in school you completed?

E. Distributions of independent variables for those who heard of *Webster*

Church Attendance (0 = Never; 1 = Few times a year; 2 = Once/twice a month; 3 = Almost every week; 4 = Every week): 0 = 177; 1 = 379; 2 = 200; 3 = 143; 4 = 352
Education (0 = No high school degree; 1 = High school degree; 2 = Some college; 4 = College graduate and beyond): 1 = 122; 2 = 414; 3 = 340; 4 = 375
Gender (0 = Male; 1 = female): 0 = 570; 1 = 681
Nonwhite (0 = White; 1 = Black): 0 = 1,142; 1 = 109
Catholic (0 = Other; 1 = Catholic): 0 = 940; 1 = 305

F. Distributions of independent variables for those who had not heard of *Webster*

Church Attendance (0 = Never; 1 = Few times a year; 2 = Once/twice a month; 3 = Almost every week; 4 = Every week): 0 = 192; 1 = 475; 2 = 275; 3 = 211; 4 = 434
Education (0 = No high school degree; 1 = High school degree; 2 = Some college; 4 = College graduate and beyond): 1 = 184; 2 = 630; 3 = 419; 4 = 354
Gender (0 = male; 1 = female): 0 = 692; 1 = 895
Nonwhite (0 = White; 1 = Black): 0 = 1,434; 1 = 153
Catholic (0 = Other; 1 = Catholic): 0 = 1,179; 1 = 408

Capital Punishment Cases

A. Dependent variables (see footnote 12 for complete wording of questions used for this variable):

0 = Do not favor capital punishment for murderers.
1 = Favor capital punishment for murderers.
Furman: 0 = 1,151; 1 = 1,750
Gregg: 0 = 847; 1 = 2,002
McCleskey: 0 = 847; 1 = 2,211

B. Questions used for independent variables for capital punishment models

Jewish: What is your religious preference? Is it Protestant, Catholic, Jewish, some other religion, or no religion?
Nonwhite: What race do you consider yourself?
Gender: Respondent's sex.
Church Attendance: How often do you attend religious services?
Party ID: Generally speaking, do you think of yourself as a Republican, Democrat, independent, or what?
Education: Respondent's degree.

C. Distributions of independent variables for capital punishment cases

Church Attendance (0 = Never; 1 = Less than once a year; 2 = Once/twice a year; 3 = Several times a year; 4 = About once a month; 5 = 2–3 times a month; 6 = Nearly every week; 7 = Every week):
Furman: 0 = 354; 1 = 254; 2 = 372; 3 = 452; 4 = 201; 5 = 267; 6 = 214; 7 = 768; 8 = 213
Gregg: 0 = 406; 1 = 263; 2 = 407; 3 = 421; 4 = 219; 5 = 238; 6 = 117; 7 = 626; 8 = 256
McCleskey: 0 = 450; 1 = 221; 2 = 408; 3 = 466; 4 = 270; 5 = 358; 6 = 206; 7 = 650; 8 = 255

Education (0 = Less than high school; 1 = High school degree; 2 = Associate/junior college; 3 = Bachelor's degree; 4 = Graduate):
Furman: 0 = 1,187; 1 = 1,482; 2 = 37; 3 = 256; 4 = 117
Gregg: 0 = 1,064; 1 = 1,469; 2 = 58; 3 = 291; 4 = 135
McCleskey: 0 = 835; 1 = 1,719; 2 = 142; 3 = 417; 4 = 176

Party Identification (0 = Strong Democrat; 1 = Not very strong Democrat; 2 = Independent, close to Democrat; 3 = Independent; 4 = Independent, close to Republican; 5 = Not very strong Republican; 6 = Strong Republican):
Furman: 0 = 557; 1 = 819; 2 = 352; 3 = 302; 4 = 241; 5 = 449; 6 = 249
Gregg: 0 = 500; 1 = 803; 2 = 407; 3 = 416; 4 = 235; 5 = 440; 6 = 203
McCleskey: 0 = 658; 1 = 708; 2 = 391; 3 = 385; 4 = 288; 5 = 541; 6 = 299

Gender (0 = male 1 = female):
Furman: 0 = 1,508; 1 = 1,609
Gregg: 0 = 1,362; 1 = 1,667
McCleskey: 0 = 1,416; 1 = 1,884

Nonwhite (0 = White; 1 = Black):
Furman: 0 = 2,656; 1 = 444
Gregg: 0 = 2,700; 1 = 305
McCleskey: 0 = 2,456; 1 = 730

Jewish (0 = Other; 1 = Jewish):
Furman: 0 = 3,021; 1 = 96
Gregg: 0 = 2,867; 1 = 62
McCleskey: 0 = 3,249; 1 = 51

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A Political Explanation of Variations in Central Bank Independence

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Although central banks possess a similar function across the industrial democracies, their institutional structures—their levels of independence—differ greatly. My explanation of this variation emphasizes the informational asymmetries of monetary policymaking. Government ministers have informational advantages in the policy process, potentially creating conflicts with backbench legislators and, in a multiparty government, coalition partners. An independent central bank can help alleviate these conflicts. Politicians will choose an independent bank (1) if government ministers, party legislators, and coalition partners have different monetary policy incentives and (2) if government ministers fear that party legislators and coalition partners will withdraw their support over a policy dispute. I statistically test the argument against the cross-national variation of central bank institutions. I also use the theoretical framework to examine episodes of institutional choice and reform in Germany, Britain, and Italy.

Across the industrial democracies, central banks perform a similar function: to implement the government's monetary policy by regulating the supply of money and credit to the economy. Nevertheless, the structures of these bureaucratic institutions—their levels of independence—differ across systems. This variation presents a puzzle. On the one hand, independent central banks limit the ability of governments to manipulate monetary policy for their own short-term gain, which implies that governments would never choose an independent central bank. On the other hand, empirical research demonstrates that independent central banks are associated with superior inflation performance, which suggests that all countries would have an independent bank to improve their economies (Alesina 1989; Alesina and Summers 1993; Burdekin and Willett 1991; Grilli, Masciandaro, and Tabellini 1991; Havrilesky and Granato 1993; Neumann 1991).

Rather than focus on the economic consequences of central banks to explain the variation, I emphasize the politics surrounding the choice of these institutions. I argue that the information asymmetries of the monetary policy process can create conflicts between government ministers, their backbench legislators, and, in a multiparty government, their coalition partners. An independent central bank can help overcome these conflicts by providing information about government (i.e., cabinet) policy choices and their economic consequences. In systems in which the government's informational advantages in the policy process are likely to produce conflicts, politicians will adopt an independent central bank. If these conflicts are unlikely to occur, politicians will choose a dependent central bank to maximize the government's control over monetary policy. Differences in the relationship between legislators, parties, and government ministers across systems, therefore, can account for the cross-national variation of central bank independence.

The first section discusses the variation of central bank independence. The second section develops the argument. The third section statistically tests how cross-national differences in legislative-government relations explain the variation of central bank institutions. The fourth section examines episodes of monetary reform in three countries to illustrate how legislative-government relations shape the choice of central bank institutions.

THE VARIATION OF CENTRAL BANK INDEPENDENCE

Political economists often rely on a variety of formal and legal indicators to measure central bank independence. These indicators usually include the appointment, term duration, and dismissal of central bank directors; government veto over the bank's policy choices; explicit policy goals for the central bank; budgetary autonomy for the central bank; performance incentives for bank directors; limitations on monetary financing of budget deficits; and control over monetary instruments. In countries with an independent central bank, the government possesses limited ex ante and ex post influence over the bank. Typically, the government appoints only a portion of an independent central bank's governing board. The government also lacks the ability to veto the bank's policy decisions or to punish central bankers through dismissal or budget cuts. In contrast, there are few restrictions on the government's authority over dependent central banks. In these systems, the government often directly controls monetary policy instruments.

Table 1 ranks central bank independence for 18 industrial democracies from 1970–90. Interestingly, independent central banks tend to be associated with federal systems, including Germany, Switzerland, and the United States (Banaian, Laney, and Willett 1983). In contrast, Britain, France, and Italy are usually classified as countries with dependent banks. During this twenty-year period, the institutional status of most central banks remained stable, despite the collapse of the Bretton Woods system of fixed exchange rates and

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I thank Robert Bates, William Bianco, Kathryn Firmin-Sellers, Robert Franzese, Jim Granato, Tom Havrilesky, Torben Iversen, William Keech, Herbert Kitschelt, Peter Lange, David Leblang, Emerson Niou, Phil Paolino, Ron Rogowski, and Patrick Sellers for helpful comments. I also thank Adam Posen for sharing his data and Sang-Hyun Lee for research assistance.

TABLE 1. Ranking of Central Bank Independence, 1970–90

Country	Grilli, Masciandaro, and Tabellini Total[a]	Alesina and Summers[b]	Cukierman[c]	Mean Independence[d]
Germany	0.87	1.00	0.66	0.84
Switzerland	0.80	1.00	0.68	0.83
United States	0.80	0.875	0.51	0.73
Canada	0.73	0.625	0.46	0.61
Austria	0.60	0.625	0.58	0.60
Netherlands	0.67	0.625	0.42	0.57
Denmark	0.53	0.625	0.47	0.54
Ireland	0.47	0.625	0.39	0.49
Australia	0.60	0.50	0.31	0.47
France[e]	0.47	0.50	0.28	0.42
Britain	0.40	0.50	0.31	0.42
Japan	0.40	0.625	0.16	0.40
Norway	0.44[f]	0.50	0.14	0.40
Sweden	0.44[f]	0.50	0.27	0.40
Belgium[g]	0.47	0.50	0.19	0.39
Italy	0.33	0.45	0.16	0.33
Spain[h]	0.33	0.375	0.21	0.31
New Zealand[i]	0.20	0.25	0.27	0.20

Note: All rankings are normalized from 0 (low independence) to 1 (high independence).
[a]The total independence scale developed by Grilli, Masciandaro, and Tabellini (1991).
[b]The Alesina and Summers (1993) index was computed by averaging the Bade and Parkin (1982)/Alesina index (1989) and their conversion of the Grilli, Masciandaro, and Tabellini scale.
[c]The index of legal independence developed by Cukierman (1992).
[d]Mean independence is computed from Grilli, Masciandaro, and Tabellini (1991), Alesina and Summers (1993), and Cukierman (1992).
[e]Ranking prior to 1994 reform.
[f]Unranked by Grilli, Masciandaro, and Tabellini (1991). The missing values were imputed by regressing their scale on the Cukierman and Alesina and Summers indexes for all countries. Using the coefficients from the equation and the values of the missing countries on the Cukierman and Alesina and Summers indexes, I computed the missing values. The correlation between that scale and the Cukierman index is $r = 0.86$. The correlation between that scale and the Alesina and Summers index is $r = 0.89$. The correlation between the Cukierman index and the Alesina and Summers index is $r = 0.85$. All three correlations are significant at the 0.001 level.
[g]Ranking prior to 1993 reform.
[h]Ranking prior to 1994 reform.
[i]Ranking prior to 1990 Reserve Bank Act.

the oil shocks in the 1970s. Beginning in the late 1980s, however, politicians in a number of countries increased the independence of their central banks.

INFORMATIONAL ASYMMETRIES AND CENTRAL BANKS

The informational asymmetries of the monetary policy process shape the incentives of legislators, coalition partners, and government ministers over the choice of central bank institutions. I begin with the assumption that politicians and parties are motivated by a desire to retain office. Consequently, they have incentives to pursue policy outcomes that satisfy their constituents' preferences. But in a complex area such as monetary policy, politicians may have difficulty choosing the policy that will produce those outcomes (Krehbiel 1991). First, changes in monetary policy do not always lead to expected results. Monetary policy has indirect effects throughout the economy, often with uneven and variable lag times (Beck 1987; Friedman 1968). These outcomes are vulnerable to outside shocks and international influences. Second, changes in monetary policy may have different effects across economic sectors. Finally, the relationship between monetary policy outputs and outcomes changes over time, reflecting technological innovations in the financial and banking industry. Successful monetary policymaking, therefore, requires that politicians invest resources to develop the expertise necessary to predict the consequences of a variety of policy proposals.

Party Legislators

Legislators face a prisoners' dilemma in the development of policy expertise (Krehbiel 1991). Information about the relationship between policy outputs and outcomes represents a collective good for legislators. It enhances the quality of legislation and ensures that policy outputs lead to outcomes that accurately reflect the desires of the enacting coalition. Each legislator, however, has high opportunity costs in the development of the expertise necessary to legislate and to oversee policy implementation effectively. Instead, legislators' time and effort are better spent campaigning or focusing on issues with specific electoral benefits. Delegating policy responsibility to government ministers helps solve this collective dilemma.

Coalition Partners

In forming a coalition government, parties make both policy compromises and policy logrolls—trading policies across issue areas. They also divide the cabinet portfolios (Laver and Schofield 1990; Laver and Shepsle 1994). Although formally constrained by the

coalition agreement, the party that controls the monetary policy dimension, the "portfolio party," has some discretion in developing new policies and in responding to situations unforeseen in the coalition agreement.

The nonportfolio party (or parties), which does not control coalition policy on the monetary dimension (although it may control other issue areas), has interests similar to those of the backbenchers in the portfolio party. Since the government's economic policy performance affects the nonportfolio party's electoral support (even if that party does not hold the monetary policy portfolios), the nonportfolio party wants policy outcomes to reflect the interests of its constituents as closely as possible. At the same time, it does not want to make costly investments in either developing policy expertise in portfolios it does not control or monitoring the portfolio party's compliance with the coalition agreement. Instead, the nonportfolio party prefers to devote resources to its own portfolios and electoral campaigns.

Government Ministers

To overcome these policy information dilemmas, backbench legislators and coalition partners delegate monetary policy authority to the government (i.e., the president, prime minister, or cabinet ministers). Government ministers acquire policy expertise, allowing them to develop legislative proposals and oversee policy implementation. By gathering information about the relationship between monetary policy outputs and macroeconomic outcomes, government ministers may help guarantee that policy outcomes reflect the desires of their party's and their coalition partner's constituents.

As an incentive for ministers to develop expertise, legislators and parties grant them institutional power over monetary policy, including agenda control and discretionary authority. First, agenda control allows a minister to limit the policy proposals considered by the cabinet and the legislature as a whole (Huber 1996; Laver and Shepsle 1994). Therefore, she may manipulate government policy by acting as a gatekeeper. Second, discretionary authority permits ministers to choose policies without explicit approval as long as those choices do not cause backbenchers or coalition partners to withdraw their support.[1] In a parliamentary system, the government needs the support of party legislators and, in multiparty systems, coalition partners to remain in office. In a presidential system, the government does not need the support of party legislators to remain in office, but it does require their support to achieve its policy goals.[2]

Although ministers must maintain the support of backbenchers and coalition partners, they may be tempted to use their expertise to manipulate monetary policy for their own electoral and policy goals—goals that may differ from those of their legislative and coalition supporters. In fact, the government has two informational advantages over backbenchers and coalition partners in monetary policy. Not only are ministers in a position to know more about the relationship between policy outputs and outcomes, but also they may conceal their actual monetary policy choices from party legislators and coalition partners. Since no single indicator captures the direction of monetary policy, the government can obfuscate its policy intentions. Indeed, the government has incentives to maintain secrecy about its plans (Cukierman 1992). Secrecy allows the government to generate politically expedient temporary booms in real economic conditions through surprise monetary expansions. Providing information to its legislative and coalition supporters about monetary policy could negate the ability of the government to manipulate short-term economic outcomes.

Asymmetric Information and Political Conflict

The delegation of policy authority to the government solves the policy information dilemma, but it creates the potential for new conflicts between legislators, coalition partners, and ministers—conflicts which could threaten the ability of each to remain in office.

First, the government's monetary policy choices influence the electoral fortunes of party legislators and coalition partners. Although macroeconomic outcomes may not be a decisive factor for an individual legislator or for a nonportfolio party, empirical evidence across systems indicates that inflation negatively affects the incumbent government's electoral fortunes (Lewis-Beck 1988; Powell and Whitten 1993). Backbenchers and coalition partners, therefore, must decide whether to trust government ministers to pursue policies that will help their electoral fortunes.

Second, information asymmetries can threaten the government's position in office. Despite the expertise of ministers, policy outputs may have unintended consequences, producing outcomes unacceptable to the government's legislative and coalition supporters. Without the necessary technical expertise and oversight capability, backbenchers and coalition partners may blame the government for inflationary outcomes beyond the government's control and withdraw their

[1] Political scientists have technically described this area of agent discretion in a variety of contexts, including legislative agenda setting (Aldrich 1995) and bureaucratic policymaking (Calvert, McCubbins, and Weingast 1989; Ferejohn and Shipan 1990; Hammond and Knott 1993).

[2] The range of governmental discretion reflects the configuration of policy preferences among the government's legislative and coalition supporters as well as the expected policy outcome that would result from a veto of the government's policy choice. In a single-party government, the government's policy discretion depends on the distribution of policy preferences of both legislators as a whole and legislators in the governing party (Aldrich 1995; Cox and McCubbins 1993). In a multiparty government, the government's policy discretion reflects the nature of the coalition bargain and the policy distance between parties as well as the need to maintain the support of party members and the legislative floor (Austen-Smith and Banks 1990; Laver and Schofield 1990; Laver and Shepsle 1996). As the policy preferences of government supporters become more diverse, the government's policy discretion generally increases.

support, even though the government may have attempted to pursue policies in their interest.

Independent Central Banks

One way for politicians to overcome these potential conflicts is to rely on the central bank for policy information.[3] Central bankers possess private information about the government's policies and their outcomes. Since the government depends on the central bank to implement its policy choices, central bank bureaucrats are informed of policy changes. As experts in the policy area, central bankers can forecast and evaluate the consequences of the government's policy.

The structure of an independent central bank lends credibility to the information it reveals about the government's monetary policy choices. First, because the government lacks ex post authority over an independent central bank, central bankers can contradict the government's policy analyses without fear of government retribution. Second, since the government appoints only a portion of an independent central bank's governing board, all the members of the board may not share the government's policy preferences. In fact, the appointment procedures of many independent central banks produce boards that reflect the major divisions over monetary policy within the society, which ensures that board members have policy preferences roughly similar to those in the legislature. In Germany, for example, differences in monetary policy preferences usually follow party lines. Until recently, each Land government appointed a representative to the Central Bank Council, which guaranteed that the political composition of the Bundesbank council echoed the balance of political representation in the country as a whole. In the United States, differences over monetary policy are primarily regional rather than partisan, pitting conservative easterners against southern and western monetary populists (Woolley 1984). Placing Reserve Bank governors elected by regional stockholders on the Federal Open Market Committee, the Federal Reserve's main decision-making body, ensures a balance of regional interests over monetary policy similar to those in Congress. These institutional features enhance the central bank's credibility with the legislature.

An independent central bank's policy information can help solve the conflicts created by the information asymmetries of the policymaking process. The government's legislative and coalition principals can learn about the consequences of the government's monetary policy from the central bank, allowing them to veto policies that may hurt their electoral fortunes. This information also can help government ministers retain their position in office. An independent bank can provide credible verification that ministers attempted to achieve certain outcomes, even if those policy choices unintentionally produced results unacceptable to legislative and coalition supporters.

An independent central bank does entail costs, however. Limited government authority over the bank creates the possibility of bureaucratic drift. Furthermore, an independent bank may constrain the government's policy flexibility or ability to react to unforeseen circumstances. An independent bank also limits the ability of the government to manipulate monetary policy for its own short-term electoral or partisan gains—gains which may benefit some of the government's legislative supporters and coalition partners.

With a dependent central bank, the government retains ex ante and ex post authority. Since the government appoints the bank's board, central bankers are likely to share the government's policy preferences. In addition, central bankers will not criticize government policy, since ministers possess the ability to punish them. Consequently, central bankers cannot credibly signal their private information about the government's policy decisions. Backbenchers and coalition partners will view the bureaucrats' signals as no more than efforts to win the government's approval. A dependent central bank, therefore, preserves the government's informational advantages in the policy process.

Evidence from a number of countries indicates that central bankers and politicians recognize the informational role of independent central bankers in the policy process. Officials at the German central bank (the Bundesbank) and the European Commission point to the informativeness of the Bundesbank as a justification for independent central banks in democratic societies (personal interviews, spring 1993). They argue that the Bundesbank had the responsibility to comment on the direction and consequences of government economic policies. Together with private economic institutes, the Bundesbank's opinions and forecasts provide counterweights to the government's biased statements on economic policy.

Parliamentary debates in New Zealand over the 1989 Reserve Bank bill echoed this informational justification for reforming the central bank. Proponents argued that an independent central bank would make policymaking more transparent. While the government would determine the ultimate objectives of monetary policy, the central bank would report to the parliament about the economic consequences of the government's policy choices and the perseverance with which the government pursued those objectives. Consequently, the legislation would "help to eliminate . . . uncertainty by enhancing the accountability of both the Reserve Bank and politicians. It will force Ministers of Finance to be open about adopting alternative policies and their inflationary effects, and will make the Reserve Bank more independent by placing the implementation of monetary policy outside day-to-day ministerial manipulation" (*New Zealand Parliamentary Debates* 1989, 10425).

In the United States, Congress requires the Federal Reserve chairman to report semiannually on monetary policy. These appearances and the Federal Reserve's

[3] Legislators and coalition partners also can gather policy information themselves (e.g., through legislative committees or deputy-level cabinet positions), monitor the government's compliance with policy rules (Keech 1995), or depend on endorsements from actors affected by the policy process (McCubbins and Schwartz 1983).

regular economic reports give legislators a better understanding of both the central bank's specific activities and the president's monetary policy choices. In addition, the chairman's appearances have become a media event, focusing public attention on the government's economic policy and providing an opportunity for legislators to declare their policy preferences (Havrilesky 1993).

Although most academic accounts of central bank independence do not emphasize this informational role in the policy process, a number of political economists recognize it. Fair (1979) argues that a central bank's independence should be measured by its ability to publicize policy disputes with the government. Havrilesky (1994) suggests that independent central banks function as a "sound money oracle," making regular anti-inflationary public pronouncements. Goodman (1992) argues that independent central banks build social and political constituencies, presumably by supplying expertise and information about monetary policy. Providing such information, therefore, is one of the most critical political functions of an independent central bank.[4]

Politicians and the Choice of Central Bank Institutions

Politicians can structure the central bank to help them overcome the conflicts created by the informational asymmetries of the policy process, but doing so entails some potential costs. Under what conditions will backbenchers, coalition partners, and the government be willing to limit the government's control over the central bank?

Backbenchers and coalition partners will prefer an independent central bank if they cannot trust the government, that is, if the government's monetary policy incentives differ from those of legislative supporters and coalition partners. In this situation, backbenchers and coalition partners recognize that the government is likely to exploit its informational advantages to pursue policies that do not reflect their interests. In contrast, if party legislators and coalition partners are confident that the government shares their monetary policy goals, then they can trust ministers to pursue policies that reflect their interests. Since they have no incentive to monitor the government's policy choices with an independent central bank, they will support a dependent bank.

The government also has an interest in the choice of bureaucratic structure. The first priority of ministers is to maintain their positions in office. If party legislators and coalition partners can credibly threaten to punish the government for its monetary policy performance—for example, by removing it from office or thwarting its policy agenda enough to make reelection unlikely—then the government will want them to have information that demonstrates its policies were intended to have consequences beneficial to their constituents. In this situation, the government has an incentive to support an independent central bank. If the threat of punishment is not credible, however, then the government has little incentive to restrict its informational advantages with an independent central bank.

The choice of central bank structure thus reflects (1) the divergence of policy incentives between the government and its legislative and coalition supporters and (2) the credibility of backbenchers' and coalition partners' threat to punish government ministers. When the government's policy incentives differ from those of backbenchers and coalition partners and when their threat to punish the government is credible, politicians will favor an independent central bank. If one of those conditions is not present, the the bank will be less independent.

THE CROSS-NATIONAL VARIATION OF CENTRAL BANK INDEPENDENCE

Since the relationship between backbenchers, coalition partners, and government ministers strongly influences the choice of central bank institutions, variations in that relationship across systems can explain cross-national differences in central bank independence. This section tests the relationship of incentive divergence, the threat of punishment, and central bank independence. The Appendix contains information about the source and construction of each variable.

Incentive Divergence

In the area of monetary policy, the distribution of constituent preferences and electoral institutions condition the degree of incentive divergence between legislators, parties, and ministers.

Constituent Preferences. The distribution of monetary policy preferences among party supporters may force candidates from the same party to make different appeals to win votes. Manipulating monetary policy can help some politicians in the party. But if constituent preferences over monetary policy are diverse, then changes in monetary policy may generate externalities among certain constituents (e.g., an interest rate increase may help creditors but hurt debtors), damaging the electoral fortunes of other candidates within the party. This problem may be particularly acute for large catch-all parties or for parties that are not based on

[4] In contrast, most recent research argues that independent central banks insulate monetary policy from political pressures. These arguments suggest that an independent central bank enhances the credibility of policy goals because independent central bankers shield policy from competing partisan pressures (Alesina 1989), possess a different time horizon from politicians (Persson and Tabellini 1990), and place more weight on controlling inflation than the median voter in society (Rogoff 1985). These arguments imply that elected politicians must abdicate responsibility for monetary policy to unelected bureaucrats to ensure successful monetary outcomes. Empirical accounts of monetary policymaking, however, suggest that politicians can influence monetary policy, even in countries with an independent bank (Goodman 1992; Havrilesky 1993; Kennedy 1991; Kettl 1986; Lohmann 1995; Marsh 1992; Scharpf 1987; Sturm 1989; Woolley 1984).

economic appeals (Budge and Farlie 1983; Kirchheimer 1966).

To measure the heterogeneity of constituent preferences over monetary policy, I employ the *Alford Index* (Alford 1963). The Alford index subtracts the percentage of voters from nonworking classes who vote for the left party(ies) from the percentage of blue-collar workers who vote the left party(ies). High values reflect a high incidence of class voting. Low values indicate that class is a less salient predictor of vote. Since low values imply that party politicians are likely to face constituents with a variety of preferences over monetary policy, the Alford index should have a negative relationship with central bank independence.

Electoral Institutions. Electoral institutions may provide politicians from the same party with divergent monetary policy incentives. Parties that compete across electoral districts of different size are likely to have conflicts over monetary policy. Candidates in smaller districts may desire to use monetary policy to appeal to particular interests, while candidates in larger districts must balance demands from a variety of constituents (Rogowski 1987). In addition, nonconcurrent elections for the government and part of the legislature provide politicians within the governing party(ies) with different incentives regarding the sequencing of monetary policy. For instance, the government may manipulate policy in order to engineer an economic boom for its election period. The eventual increase in inflation resulting from a monetary surprise, however, may adversely affect legislative elections in the next period (Alesina and Rosenthal 1995).

Politicians are likely to compete in differently sized districts or at different times in systems with "strong" bicameral legislatures. Lijphart (1984) defines strong bicameralism as a situation in which both houses have roughly equal legislative powers and are selected according to different electoral rules. I use a dummy variable for countries with *Strong Bicameralism*: Australia, Germany, Switzerland, and the United States. Systems with strong bicameral legislatures should have more independent central banks.

The Threat of Punishment

The credibility of the threat by backbenchers and coalition partners to punish the government reflects the polarization of the political system, legislative institutions, and the existence of coalition and minority governments.

Polarization. Dissatisfied backbenchers and coalition partners must cooperate with at least part of the opposition in order to challenge government policy. Highly polarized systems present few opportunities for such cooperation, due to the large policy distance between parties (Sartori 1976). Consequently, challenge is less likely. In systems with low polarization, temporary cooperation and, hence, challenges are more likely.

Following Powell (1982), I measure *Polarization* as the average percentage of electoral support for extremist parties for 1960–90. Since high levels of polarization imply that party legislators and coalition partners will be less willing to punish the government, polarization should be negatively related to central bank independence.

Legislative Institutions. Legislative institutions influence the possibility of punishment by affecting the cost to backbenchers and coalition partners of challenging the government's policy. In systems with endogenous electoral timing, dissatisfied party legislators and coalition partners must consider the possibility that a challenge to the government's policy might precipitate early elections (Huber 1996). The possibility of losing their seats in a general election may provide them with an incentive not to challenge the government. In systems with strict party discipline, backbenchers face another disincentive, since party leaders have the institutional authority to punish legislators who fail to support the government. Finally, if the political fortunes of legislators are closely tied to their party's participation in government, they may be reluctant to risk their status by challenging government policy. In contrast, if the benefits of serving in the governing party are low—if, for example, the opposition can still influence policy—then party legislators may be more willing to veto government policy (Strom 1990a, 1990b).

To capture the influence of legislative institutions, I use a dummy variable for systems with "strong and inclusive" committee systems (Powell and Whitten 1993; Strom 1990b). Since party legislators and coalition partners are more likely to withdraw their support from the government in systems with strong and inclusive legislative committees, the *Committee Strength* variable should have a positive relationship with central bank independence.

Coalition and Minority Government. There is relatively more potential for punishment in coalition or minority governments.[5] They are more likely to fall over a policy dispute than are single-party majority governments. I calculated the proportion of time a country was governed by a coalition or minority government from 1960 to 1990 (*Coalition/Minority Government*). In systems with a high proportion, the central bank will be more independent.

The Appendix and Table A-1 summarize the independent variables and their sources. I expect independent central banks in systems with moderate left parties (low Alford index), strong bicameralism, low polarization, strong legislative committees, and regular coalition or minority governments.

[5] There is also higher potential for monetary policy conflicts within the governing parties. If coalition partners serve constituents with different monetary policy preferences, they will disagree about the proper policy course. Therefore, cabinet ministers must make policy choices that will alienate either their own legislative party or their coalition partner.

TABLE 2. OLS Estimation of Central Bank Independence (Mean)

	I	II	III	IV	V
Constant	0.52***	0.40***	0.42***	0.39***	0.33***
	(0.07)	(0.07)	(0.08)	(0.07)	(0.05)
Alford Index	−0.73*	−0.74**	−0.77**	−0.78**	
	(0.27)	(0.25)	(0.25)	(0.25)	
Strong Bicameralism	0.22**	0.25***	0.23***	0.25***	
	(0.06)	(0.05)	(0.06)	(0.05)	
Polarization	−0.50				
	(0.33)				
Coalition/Minority	0.12				
	(0.11)				
Committee Strength	0.08				
	(0.08)				
Punishment Index		0.09**	0.11**	0.09**	
		(0.03)	(0.04)	(0.03)	
Trade/GDP			−0.06		
			(0.11)		
Partisanship				0.10	
				(0.09)	
Financial Sector Strength					0.15***
					(0.03)
R^2	0.82	0.78	0.79	0.80	0.56
Adj. R^2	0.74	0.74	0.73	0.74	0.54
F-Value	$F(5,12) = 10.8$	$F(3,14) = 17.0$	$F(4,13) = 12.2$	$F(4,13) = 13.0$	$F(1,16) = 20.6$
SEE	0.0885	0.0893	0.0915	0.0892	0.1191
Cook-Weisberg $\chi^2(1)$	2.61	2.07	2.94	2.42	1.09
Normality	1.56	0.33	0.87	0.70	1.15
Durbin-Watson	1.76	2.10	2.07	2.09	1.70
RESET	$F(3,9) = 0.73$	$F(3,11) = 0.76$	$F(3,10) = 0.86$	$F(3,10) = 1.02$	$F(3,13) = 0.97$
N	18	18	18	18	18

Note: *$p < .05$, **$p < .01$, ***$p < .001$, two tailed test. Standard errors are in parentheses.

Sample, Dependent Variable, and Methods

The sample includes the 18 countries listed in Table 1. By examining only industrial democracies, I control for a number of factors which may influence central bank independence, including the level of economic development, position in the world economy, and domestic regime stability (e.g., Clark 1992; Cukierman 1992; Maxfield 1997). From the larger set of industrial democracies, I chose countries that were rated by at least two of the three indexes in Table 1. Data availability and reliability further limited the sample.

For the dependent variable, *Central Bank Independence*, I used all four indexes of central bank independence listed in Table 1.[6] I report the results from only the average measure. Unless noted, results for all indexes were similar. I used ordinary least squares (OLS) regression.

RESULTS

The results of model I, reported in Table 2, are partially consistent with the incentive divergence and potential punishment hypotheses. The Alford index has a negative and significant effect on central bank independence. Systems in which the left parties have heterogeneous class support possess more independent central banks.[7] Systems with strong bicameral legislatures also have more independent central banks. None of the indicators associated with potential punishment is significant, although each coefficient is in the predicted direction.[8] Polarization, committee strength, and coalition/minority government are highly correlated. Although each is not individually significant, a Wald test indicates that the three variables are jointly significant at the 0.01 level ($F(3, 12) = 5.17$). Model I also passes all diagnostic tests, indicating that the residuals are not serially correlated (Durbin-Watson),[9] are normally distributed (skewness and kurtosis), and are homoscedastic (Cook and Weisberg 1983). In

[6] I also ran the models using the index from Cukierman, Webb, and Neyapti (1992) as the dependent variable. The results were similar (results not reported). All calculations were performed using Stata 5.0.

[7] The Alford index had a significant negative relationship with the other scales, except for the Cukierman index. There, the coefficient was in the predicted direction but failed to attain conventional levels of significance (t-statistic = −1.53, critical value for two-tailed test = −2.16).

[8] I also ran each of these variables individually. Both committee strength and the coalition/minority government variables were significant and in the predicted direction. Polarization was in the predicted (negative) direction but did not attain conventional levels of significance (t-statistic = −1.46, critical value for two-tailed test = −2.16). The results are not reported.

[9] This is a check for spatial correlation between errors of cases located next to one another.

addition, there is no evidence of omitted variable bias (RESET).[10]

Given the joint significance of the polarization, committee strength, and coalition/minority government variables, I created a composite indicator for the threat of punishment by adding (1 − polarization), committee strength, and coalition/minority government. I expect this indicator to have a positive relationship with central bank independence. The results from this equation, model II, support the argument. The punishment index is significant and in the predicted direction. The Alford index and strong bicameralism variables remain significant and in the predicted direction. Model II explains 74% of the variance in the dependent variable for this sample. The model also passes all diagnostic tests.

Alternative Hypotheses

Given the importance of central banks for economic performance, political economists have offered several explanations of the variation of central bank independence.

Tying the Hands. McCubbins, Noll, and Weingast (1989) argue that politicians structure bureaucracies to guarantee that their policy goals will not be overturned in the future. Similarly, Goodman (1991) claims that governments with a preference for low inflation and which expect a short tenure will adopt an independent central bank to limit the ability of future governments to manipulate economic policy. The "tying the hands" argument suggests that highly polarized systems or systems with highly antagonistic parties should have independent central banks. Systems with moderate, centrist parties should have dependent banks, since politicians can trust the opposition to pursue similar policies. The results from models I and II contradict this hypothesis. Broadly based moderate leftist parties (low Alford index) and low levels of polarization are associated with high levels of central bank independence.

Economic Openness. A second group argues that economic openness influences the level of central bank independence (Maxfield 1997). An open economy, dependent on international trade and foreign capital, requires an independent central bank. An independent central bank can help prevent inflation from eroding the competitive position of the economy, especially in countries with fixed exchange rates. An independent central bank also helps attract foreign capital and investment by demonstrating the host government's commitment to price stability (Maxfield 1997). To test the effect of economic openness, I included *Trade* as a proportion of GDP for 1970–89. Model III indicates that trade has no statistically significant effect on central bank independence.[11]

Partisanship. Some political economists argue for a link between government partisanship and central bank structure. Right parties are traditionally more concerned with controlling inflation, while left parties place more emphasis on employment and wealth redistribution (Alesina 1989; Alesina and Sachs 1988; Havrilesky 1987; Hibbs 1987). Consequently, right parties will institute an independent central bank to counter inflationary pressures in the economy, while left parties will prefer a dependent bank, which allows them to manipulate monetary policy to enhance growth and employment.[12] This argument suggests, however, that new governments would reform the bank to suit their economic policy goals, which implies more cross-temporal variation in central bank independence than is empirically observed. To test the hypothesis, I created a measure of *Partisanship* based on Cameron (1984). The measure multiplies the percentage of cabinet seats held by left parties by the percentage of a legislative majority held by left parties for each year and then averages those measures across the period. Model IV reports the results of including the partisanship measure for 1970–89. No statistically significant relationship between partisanship and central bank independence emerged.[13]

Interest Groups. A fourth set of explanations centers on interest group pressure as a determinant of bureaucratic structure (Moe 1989, 1990). Both Posen (1993, 1995) and Maxfield (1994) argue that the organization and political influence of a nation's financial sector strongly determine the central bank's structure. Posen argues that systems in which the financial sector can organize an effective opposition to inflationary policies will have independent central banks. These explanations focus solely on demanders of institutions and policies—interest groups—rather than their suppliers—politicians. Politicians, however, have their own incentives regarding policy and institutions, independent of group demands.

To test this hypothesis, I used Posen's measure of *Financial Sector Strength*, which is based on the presence of universal banking, the central bank's role in the regulation of the financial sector, federalism, and party fractionalization at the national level (model V). This

[10] RESET is the regression specification error test proposed by Ramsey (1969). The test uses the predicted values of the dependent variable from the original equation, raised to the second, third, and fourth powers, as additional regressors. The significance of these additional regressors is evaluated with a standard F test. A significant test indicates that the model is misspecified.

[11] I also used a population-adjusted measure of trade openness, based on Taagepera and Hayes (1977). The variable was created by regressing the (logged) proportion of trade on the (logged) population for the sample of countries. I then subtracted the predicted proportion of trade from the actual values to form the measure. This measure also failed to attain significance.

[12] It can be argued, somewhat counterintuitively, that left parties will favor independent central banks. Left parties recognize that they have little inflation-fighting credibility with financial and capital markets, which contributes to higher risk premia and the possibility of capital flight (Garrett 1995). To demonstrate their commitment to responsible economic policies, left parties may institute an independent central bank. Right parties have more credibility with markets and, consequently, less incentive to choose an independent central bank.

[13] I employed average measures for other periods, including 1970–79 and 1980–89. These measures were not significant.

TABLE 3. Encompassing Test

Model V ξ Model II	Form	Test	Form	Model II ξ Model V
5.31*	$F(3,13)$	Joint Model	$F(1,13)$	1.24

Note: $^*p < .05$

measure of financial sector strength has a positive and significant relationship with central bank independence. The model also passes all diagnostic tests. When financial sector strength was included with the variables from model II, however, the punishment index and the financial sector index failed to attain significance, since those two variables are correlated ($r = 0.67$; results not reported).[14] Note, however, that the residual variance (SEE) is smaller in model II (0.0893) than in model V (0.1191), suggesting that model II provides a more efficient prediction of central bank independence (Achen 1982).

I also compared models II and V with a parameter encompassing test (Granato, Inglehart, and Leblang 1996; Granato and Suzuki 1996). The test indicates whether the substitution of one model's parameters for the other's is statistically distinguishable. I used a joint F-test to test whether model II encompasses model V (symbolically, model II ξ model V). An insignificant statistic indicates that zero order restrictions on model V have no statistical effect on a joint model, composed of indicators from both models. That is, model II ξ model V. In contrast, a significant statistic signals that model II does not encompass model V, meaning that model V contains information that model II fails to capture. I also examined whether model V encompasses model II. The results of the F-tests indicate that model II encompasses model V (Table 3). In addition, the results indicate that model V does not encompass model II. Consequently, it is possible to conclude that the variables in model II possess more predictive power than does the strength of the financial sector.

Sensitivity Analysis

Small N studies may suffer from a variety of statistical problems, including the possibility that parameter estimates are strongly affected by an individual case (Granato, Inglehart, and Leblang 1996; Jackman 1987). An observation's influence reflects both its discrepancy and leverage. Discrepancy occurs when an observation is associated with a large residual. Standardized and studentized residuals indicate which observations possess unusually large residuals. Examining the residuals of model II, Britain appears to be an outlier, since it has a standardized residual of 2.21, outside the usual cutoff of ±2.0 (results in Table A-2). The standardized residuals also suggest that Belgium (−1.636) and Denmark (1.651) may be outliers, although neither exceeds the cutoff point.[15]

Leverage refers to the potential for the model as a whole to be influenced by a few cases. Two statistics, Cook's Distance (D_i) and DFFITS, measure leverage (Belsley, Kuh, and Welsch 1980). A D_i greater than $4/n$ indicates an influential observation. According to this criterion, Britain (0.499), Australia (0.309), and Denmark (0.229) are influential. A related diagnostic, DFFITS, also points to the influence of Britain (1.69) and Denmark (1.029). Both DFFITS values are greater than the $2^*(k/n)^{1/2}$ cutoff recommended by Bollen and Jackman (1990).

Another diagnostic, DFBETA_i, measures the effect of deleting one observation on each parameter estimate. An absolute value of DFBETA_i greater than one indicates that the observation shifts the coefficient at least one standard error (Bollen and Jackman 1990).[16] According to this criterion, Britain exerts undue influence on the parameter estimate for the Alford index (Alford $\text{DFBETA}_{Britain} = 1.19$). None of the DFBETA for the other independent variables exceeds the cutoff, although the punishment index DFBETA for Britain is extremely close (−0.99). The direction of the DFBETA, however, indicates that Britain actually dampens the size of the coefficient for both the Alford index and the punishment index (i.e., pulls them both toward zero). The fact that both these variables are significant even when Britain is in the sample lends credence to the results.

Overall, the diagnostics suggest that Britain and, possibly, Denmark are problematic cases. Do these outliers invalidate the parameter estimates of model II? Following Granato, Inglehart, and Leblang (1996), I handle these cases in a variety of ways. Table 4 compares the OLS results from model II in Table 2 with a variety of estimation strategies. First, I included a dummy variable for the influential cases (Britain and Denmark) in model 4a.[17] Second, I deleted these cases

[14] Posen's data set did not include Norway and Sweden. Following Posen (1995), I constructed measures for those two countries using a variety of sources. In addition, Posen's measure of financial sector strength can be broken into two dimensions: The financial sector dimension is composed of the presence of universal banking and the regulatory role of the central bank. The political dimension is composed of federalism and party system fractionalization. The financial dimension is highly correlated with the punishment index ($r = 0.85$). The political dimension is correlated with the presence of strong bicameralism ($r = 0.83$). Both correlations are significant at the 0.001 level.

[15] I performed similar analyses for models with other measures of central bank independence as the dependent variable. The other models did exhibit some evidence of influential outliers, although Britain was the only outlier common to all four indexes.

[16] Belsley, Kuh, and Welsch (1980) suggest observations with $|\text{DFBETA}_i| > 2/(n^{1/2})$ exert undue influence. This cutoff is stronger than the standard recommended by Bollen and Jackman. For the Alford index, observations failing to meet this standard include Canada (−0.79), Denmark (0.69), and Britain (1.19). For strong bicameralism, Australia (−0.89) exceeds the cutoff. Finally, Australia (0.63), Belgium (−0.49), and Britain (−0.99) exceed the cutoff for the punishment index.

[17] I also tried individual dummy variables for Britain, Denmark, and

TABLE 4. Diagnostics on Model II

	4a Dummy Variable	4b Cases Deleted	4c Robust Regression	4d Bounded Influence	4e Bootstrapped
Constant	0.45***	0.45***	0.45***	0.41***	0.40***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.07)
Alford Index	−1.23***	−1.24***	−1.24***	−0.93***	−0.74**
	(0.18)	(0.18)	(0.17)	(0.22)	(0.20)
Strong Bicameralism	0.27***	0.27***	0.26***	0.27***	0.25***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)
Punishment Index	0.10***	0.11***	0.11***	0.10***	0.09***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Britain/Denmark Dummy	0.25***				
	(0.04)				

Note: **$p < .01$, ***$p < .001$, two-tailed test. Standard errors are in parentheses. $N = 18$, except for model 4b ($N = 16$). 4a estimated with a dummy variable for Britain and Denmark; 4b estimated without Britain or Denmark; 4c estimated using robust regression (Western 1995); 4d estimated using Welsch's (1980) one-step bounded influence estimator; and 4e estimated using bootstrap resampling of the residuals with 1,000 replications.

from the sample (4b). In both models, the independent variables continue to be significant and in the predicted direction. As expected, the parameter estimate of the Alford index becomes even more negative, while the coefficient of the punishment index increases slightly.

I also reestimated model II using three nonparametric techniques. The first employs a variant of robust regression. Robust regression produces parameter estimates that perform well even if the data slightly violate basic OLS assumptions concerning normality (Western 1995). The method I employ begins by dropping any observation with $D_i > 1$ (no observations in the sample meet this criterion). The remaining observations are weighted according to their residuals, using Huber weights and, subsequently, biweights. Weighted least squares is then used to generate new estimates. The process is iterated until the maximum change in weights drops below 0.01. Interestingly, the robust regression places zero weight on Britain and a very small weight on the Danish case (Table A-2). Consequently, the estimate in model 4c is similar to model 4b.

As with OLS, however, robust regression is also susceptible to highly influential observations (Western 1995). To constrain the influence of such cases, I used a simple one-bounded influence estimator suggested by Welsch (1980) and employed by Granato, Inglehart, and Leblang (1996), which uses the DFITTS values from the original OLS regression to downweight observations.[18] The estimates, shown in model 4d, are similar to the estimates in the original OLS regressions.

Finally, I used a bootstrap technique to determine confidence intervals around the parameter estimates for each independent variable. Bootstrapping estimates the sampling distribution of a statistic, treating the sample as a population (Mooney 1996; Mooney and Duval 1993). I resampled with replacement the residuals of model II one thousand times. The bootstrapped standard errors are presented in column 4e. Using these, I calculated the confidence intervals with the normal approximation method. For each independent variable, the 95% confidence interval does not span zero. The bootstrap confidence intervals, therefore, support the conclusions of the original OLS estimates in model II.

Each of these techniques demonstrates that the parameter estimates obtained in model II are robust. The Alford index, strong bicameralism, and the punishment index retain their significance and remain in the predicted direction.

CENTRAL BANK CHOICE AND REFORM

The statistical results indicate that differences in the relationship between party legislators, coalition partners, and government ministers can account for cross-national variations in central bank independence. This section discusses episodes of reform in Germany, Britain, and Italy to illustrate the influence of legislative-government relations in the choice of central bank institutions. Although specific events may precipitate reform, the relationship between legislators, coalition partners, and government ministers provides the underlying conditions that shape the choice and stability of central bank institutions.

Germany

The divergence of policy incentives among German politicians and the possibility that the government will be punished over a policy dispute help ensure the commitment to an independent Bundesbank.

Backbenchers and coalition partners have monetary policy incentives that differ from those of the government, due partly to Germany's federal system. First, each party's constituents possess a range of monetary policy interests, reflecting the regionalization of the

Australia. The Britain dummy variable attained statistical significance. The Denmark dummy was not significant on its own, but it did become significant when used in a model with the Britain dummy. The dummy for Australia was not statistically significant at any point.

[18] If the absolute value of $DFFITS_i$ is less than or equal to 0.34, then the weight is equal to one. If $|DFFITS_i| > 0.34$, then the weight is $0.34/|DFFITS_i|$. Weights for the bounded regression are contained in Table A-2.

German economy. Since Land party organizations control the party list for elections to the lower house, members of parliament must balance Land interests with the demands of the government to ensure their renomination. Second, the bicameral legislature creates the potential for conflict between the government, selected in the lower house (Bundestag), and party legislators in the upper house, the Bundesrat. Bundesrat representatives are appointed by the state governments. Nonconcurrent elections for the lower house and state governments provide politicians with different incentives regarding monetary policy sequencing. Finally, the proportional representation electoral system strongly discourages the formation of single-party majority governments. Coalition partners often have constituents with different and sometimes conflicting preferences, as illustrated by the tension between the Social Democrats and the Free Democrats in the early 1980s (Goodman 1992; Scharpf 1987). An independent Bundesbank helps prevent the government from pursuing policies that will hurt party legislators and coalition partners.

An independent Bundesbank also helps the government stay in office by bolstering the credibility of its economic policies. Since monetary policy remains a high salience issue in Germany, the government's principals—backbenchers, coalition partners, and, ultimately, voters—use it to gauge the government's competence. The nature of the political system enhances the government's vulnerability on this dimension. The relative proximity of the major parties on the economic and monetary policy dimension contributes to the possibility of punishment. Although each party espouses different priorities, their economic programs reflect a German consensus about the proper balance between market and state. The coalition dynamics of the party system also increase the possibility that the government will lose office over a policy dispute. As the pivot party in the system, the small Free Democrats have participated in coalitions with both the Christian Democrats and the Social Democrats. Consequently, the Free Democrats can threaten to leave the government if their senior partner does not comply with the coalition agreement.

The Bundesbank's independence, therefore, can discourage the defection of party legislators, the Free Democrats, and voters from the governing coalition. During the 1970s, the Social Democrats relied on the policy credibility of the Bundesbank to forge a coalition with the Free Democrats, despite their differences on economic policy. Although many blame the Bundesbank for exacerbating tensions between the two parties in the early 1980s (e.g., Scharpf 1987), the Bundesbank contributed to the longevity of the coalition by helping assuage the concerns of Free Democrats about the influence of the left-wing of the Social Democrats. Social Democratic ministers, therefore, had an incentive to tolerate an independent central bank in the 1970s. In contrast, Christian Democratic governments have less incentive to support an independent central bank. Since they share similar economic priorities with the Free Democrats, it is less likely that the Free Democrats will withdraw their support over an economic policy dispute. Consequently, Christian Democratic governments have less need to rely on an independent central bank to verify their policy performance.

Indeed, Christian Democratic governments have twice attempted to gain more authority over the Bundesbank. During the founding of the Bundesbank in the 1950s and its reorganization in the 1990s, there were attempts to increase government influence over the composition of the central bank's main decision-making body. In both instances, mistrust between the government and its party supporters in the Bundesrat helped ensure the Bundesbank's independence.

After World War II, the Allies created a decentralized central banking structure consisting of legally autonomous Land (state) central banks coordinated by the Bank deutscher Lander (BdL). Article 88 of the 1949 German Basic Law required the new government to reorganize this system, which touched off a prolonged debate about its institutional design. The appointments procedure emerged as a key issue (Goodman 1992; Lohmann 1994). One side called for a centralized system, with a governing board composed only of federal government appointees. Proposals for a more decentralized system sought to retain the BdL's federal structure and increase the influence of the Land central bank presidents, appointed by the state governments, in the policy process.

The issue divided the coalition government led by the Christian Democrats. After heated internal debate, the cabinet eventually agreed to a decentralized plan and sent it to the legislature (Lohmann 1994). The Bundesrat, composed of state representatives, easily endorsed the proposal, which had support from both Christian Democrats and the Social Democratic opposition. Frozen out of power at the national level but with significant representation in the state governments, the Social Democrats perceived a decentralized structure as the best way for them to influence monetary policy (Goodman 1992). The Free Democrats, however, who had very weak support at the state level, believed that a centralized structure would afford them the best opportunity to affect policy. Consequently, they introduced in the Bundestag a counterproposal for a centralized bank. The competing bills were referred to committee, where the legislation stalled (Lohmann 1994).

In 1955–56, the BdL's restrictive monetary policy brought it in conflict with the government. Chancellor Adenauer publicly criticized the bank, and the government called for a new centralized structure, which would integrate the bank with other policymaking institutions. The Bundestag passed the motion, but the Bundesrat rejected it and reintroduced the proposal for a decentralized structure. With a majority in the Bundesrat, the Christian Democrats might have been expected to support the centralized plan, which would increase the influence of the national party over monetary policy. They were motivated, however, both by the desire to maintain state influence over monetary policy and by the need to place an institutional check

on their own party's government, since they had different incentives regarding monetary policy.

The competing proposals went again to a Bundestag committee. This time a compromise was reached: a Central Bank Council composed of up to ten central government appointees and eleven Land central bank presidents, chosen by the respective state governments. Proponents argued that the majority status of Land central bank presidents on the council provided a vital safeguard against central government manipulation of policy. The proposal unanimously passed the Bundestag and overwhelmingly passed the Bundesrat, with support from states dominated by both the Christian Democrats and the Social Democrats (Lohmann 1994). The divergence of policy incentives between the government and party legislators in the Bundesrat, therefore, helped guarantee the Bundesbank's independence.

In the early 1990s, the Bundesbank was reorganized to incorporate the eastern Lander into the system. Again, the composition of the council surfaced as a key issue. Advocates of centralization, including the Christian Democratic-led government and central government appointees to the Bundesbank, argued that the addition of new Land central bank presidents would render decision making cumbersome and unwieldy. Opponents of centralization, including most Land central bank presidents, argued that the majority status of Land central bank presidents on the council ensured the Bundesbank's independence (Henning 1994; Kaltenthaler 1996; Lohmann 1994, 1995).[19] Anticipating opposition from the Bundesrat, the government proposed a council composed of nine Land central bank presidents and eight government appointees, which would increase the relative influence of central government but preserve the majority status of the Land central bank presidents. The Bundesrat rejected that proposal by a two-thirds majority, which included some Christian Democratic states, and reiterated its preference for the "one Land, one Land central bank" principle. The government countered by promising key states that they would retain their central bank, and opposition in the Bundesrat was reduced to a simple majority. Under German law, this margin was not able to block passage of the bill in the Bundestag. Although the reform increased the relative influence of federal appointees, the potential opposition from party legislators in the upper house forced compromises that preserved the independence of the Bundesbank.

Britain

Throughout the postwar period, the dependent Bank of England was firmly under government control. During the early 1990s, however, support for an independent central bank increased among legislators. The Conservative government, however, opposed the idea.[20] Since Britain's political institutions limit the possibility of challenge from backbenchers, the government had little incentive to support a more independent bank.

The Bank of England's dependence reflected the similarity of monetary policy incentives between party legislators and government ministers. Until the 1970s, the Conservative and Labour parties each appealed to constituents with fairly homogeneous monetary policy preferences. The Conservatives represented business and the middle class. Labour's main constituents included trade unions and the working class. The Westminster political system reinforced the ideological coherence between party legislators and the government (Lijphart 1984). The majoritarian electoral system helps ensure single-party majority governments. Since Parliament is essentially unicameral, party legislators do not face nonconcurrent elections, which means they have similar incentives regarding policy sequencing. Because the system places a premium on party unity in the legislature, backbenchers have little incentive to express divergent opinions.

During the 1970s and 1980s, economic developments changed the policy preferences of each party's traditional constituency, causing conflict within both parties over the proper course of economic policy (Hall 1986). Deep divisions between Labour radicals and moderates decimated the party's electoral fortunes in the 1980s. Changes in the Conservatives' base were manifested in conflicts between "Tory wets" and radical free-market Thatcherites.

These conflicts came to a head in debates over British participation in the European Monetary System (EMS). After bringing inflation down in the early 1980s, the Conservative government allowed it to more than double by 1990, to 9.3%. Many Tory leaders, including Chancellor of the Exchequer Nigel Lawson, advocated entry into the EMS to regain control over inflation. Prime Minister Thatcher remained opposed, arguing that participation would sacrifice Britain's policymaking autonomy. Lawson resigned over the dispute, arguing in his resignation speech not only for EMS entry but also for a more independent central bank to help control inflation.

Lawson's resignation placed the Bank of England's

[19] The Bundesbank president, Karl Otto Pohl, originally proposed a council composed of seven government appointees and seven Land central bank presidents, with the council president (appointed by the central government) as a tie breaker. The Bundesbank council rejected this proposal as too centralized. Pohl next proposed reducing the number of Land central banks to eight, with no change in the number of federal appointees (up to ten). By specifying which states would maintain representation on the council, Pohl was able to buy off some Land central bank presidents and forge a winning coalition within the council. Those Land central bank presidents who voted against the proposal sent a letter of protest to Chancellor Kohl requesting that each Land retain a seat on the council. This opposition included Land central bank presidents who had been nominated by both Social Democratic and Christian Democratic state governments.

[20] In May 1997, the newly elected Labour government increased the independence of the Bank of England by granting it authority to set interest rates. The government retains control over appointments to the bank's leadership positions. This reform reflects, in part, the Labour Party's desire to appeal to a more moderate social coalition which includes constituents with heterogenous economic policy preferences.

independence on the agenda.[21] The Commons Select Committee on the Treasury held hearings on the issue. The committee's final report proposed a more independent institution, similar to the newly reformed Reserve Bank of New Zealand (*Guardian*, 17 December 1993, p. 16). The report met with immediate opposition from both the Conservative and Labour frontbenches. Prime Minister John Major declared the report a "dead" issue and blocked further consideration of the proposals.[22] Only the Liberal Democrats formally endorsed the reform.

Conservative leaders had no incentive to support a more independent central bank since British legislative and electoral institutions make it highly unlikely that party legislators would withdraw their support over a policy dispute.[23] Although party legislators have the formal authority to dismiss the government, several institutional factors discourage it. First, the benefits of serving in the governing party are high. Opposition legislators have little influence over policy or distributive benefits (Strom 1990a). Second, governments typically have the authority to call for new elections, forcing rebellious legislators to risk their seat at an early election (Huber 1996). Third, party leaders control a variety of electoral and party resources that allow them to punish legislators who fail to support the government's agenda. Because of these costs, governments rarely face challenges from within their own party, regardless of the opposition's position. Consequently, the Conservative government had little incentive to support an independent central bank.

Italy

While respected within Italy as one of the country's most nonpartisan institutions, the Bank of Italy has been generally classified as a dependent central bank. Beginning in the early 1980s, however, the government has increased the independence of the central bank. The so-called divorce, enacted in 1981, ended the bank's obligation to purchase unsold government securities. Since then, the bank has received greater authority over monetary instruments. These reforms reflect the changing position of the Christian Democrats, Italy's main governing party in the postwar period, during the 1970s and 1980s.

Politicians within Italy's governing coalition have always faced a variety of incentives regarding monetary policy. The noneconomic appeals and factionalization of the Christian Democratic Party attracted constituents with a variety of monetary policy interests (Tarrow 1990; Wertmann 1981). The frequency of coalition governments created another source of conflict over monetary policy. The Christian Democrats had to rely on smaller parties to remain in government, including the Socialists, who appealed explicitly to unions. Labor unrest in the late 1960s and early 1970s increased tensions over economic policy both within the Christian Democratic Party and with coalition partners (Goodman 1992). Within the party, labor militancy increased the influence of the pro-labor wing, as party leaders feared that denying union demands would increase the power of the Communists. Within the coalition, the Socialists became more outspoken in defense of union interests.

As a consequence of the potential for policy conflict within the ruling coalition, most small parties and even some Christian Democrats supported a more independent bank. In 1975, for example, the Treasury minister had to appoint a new central bank governor. The minister, a Christian Democrat, favored his deputy, who had close ties to the party. The small parties and some Christian Democrats, however, opposed the partisan appointment and forced the selection of a compromise candidate (Goodman 1992). Furthermore, most parties responded positively to the 1981 divorce, which occurred under the first non-Christian Democratic prime minister in the postwar period. Only the Socialists opposed the action.

Through the 1970s, however, the Christian Democratic leadership had little incentive to support an independent bank. The polarization of the Italian party system ensured that the Christian Democrats did not face a serious challenge to their governing position. The radical economic program of the Communist Party, the Christian Democrats' main rival, discouraged the defection of smaller parties from the ruling coalition.

The picture changed in the 1970s. First, the Communist Party moderated its economic position, espousing macroeconomic discipline, austerity plans, and limited wage gains. The moderation appeared to pay dividends. While the Communists' vote share had increased steadily throughout the postwar period, it jumped more than seven percentage points between 1972 and 1976, exceeding 34%. Consequently, the Communists could claim to be a serious alternative to the Christian Democrats. In 1976, the Christian Democrats agreed to cooperate with the Communists, offering to consult them on policy decisions in return for Communist support (more specifically, nonopposition) of a Christian Democratic minority government. This agreement gave the Communists a new legitimacy with potential coalition partners and voters. At the same

[21] In 1993, Chancellor of the Exchequer Norman Lamont also advocated a more independent central bank in his resignation speech to the House of Commons.

[22] Conservative MP Nicholas Blugden attempted to place the committee report on the legislative agenda. His arguments in favor of the bill emphasized the informational role of an independent central bank: "The whole object of these proposals is to achieve openness and to insure that when a Government says it is doing one thing, it cannot give directives to the Bank which may do something quite different. My proposal does not prevent the Government giving instructions to the Bank to let inflation rip, but merely means that, if they do that, it has to be done openly and everybody has to understand the consequences.... We want a bit of openness between the House and the Government" (*Guardian*, 29 January 1994, p. 34).

[23] Politicians in New Zealand, another Westminster system, reformed their central bank in 1990, but the step was taken almost simultaneously with other political reforms that rejected significant portions of the Westminster model. In Britain, both opponents and proponents of central bank reform recognized that an independent central bank would be ineffective within the Westminster system. Proponents of independence, such as the *Economist* and the Liberal Democratic Party, have also advocated political reform.

time, the Christian Democratic vote stagnated. Although the elastic and nonprogrammatic appeal of patronage helped retain support, the party was unable to attract new voters, particularly young, educated professionals and immigrants from rural and southern areas (Tarrow 1990). The economic crises of the 1970s also eroded the Christian Democrats' reputation for competent economic management.

Faced with the increased possibility of losing office, the Christian Democrats undertook reforms in several policy areas—fiscal (Della Sala 1988), industrial (Locke 1995), regional (Putnam 1993), and monetary. Italy's participation in the EMS in 1979 and the divorce of the Bank of Italy were both designed to clarify the Christian Democrat's commitment to responsible economic policy and, more important, to help ensure their continued presence in office.

The divorce did clarify the Christian Democrats' responsibility for its economic policy but not in the way the leadership had hoped. In December 1982, the bank ceased all purchases of government securities, which required the government to appeal to the parliament for an extraordinary advance to finance the budget deficit (Goodman 1992). Although the request passed easily, the central bank's refusal to act forced the government to acknowledge responsibility for the fiscal crisis. Instead of highlighting the Christian Democrats' macroeconomic discipline, the divorce focused attention on the party's continuing inability to deal with Italy's long-term economic problems, particularly the national debt, and contributed to the party's popular decline.

CONCLUSION

The information asymmetries of the monetary policy process create potential conflicts between backbench legislators, coalition partners, and government ministers. The severity of these conflicts conditions politicians' incentives regarding the choice of central bank institutions. In systems where politicians have different monetary policy incentives and where backbenchers and coalition partners can credibly threaten to withdraw their support from the government, politicians will choose an independent central bank. In systems where legislators, coalition partners, and government ministers share similar policy incentives or where the government's position in office is secure, then the bank will be dependent. Both the statistical results and case accounts of central bank reform provide support for this argument. The argument has two further implications for the variation of central bank independence.

First, the argument provides a mechanism to explain the association between federalism and independent central banks. In large federal systems, politicians are likely to face a variety of incentives over monetary policy. Constituents will possess a range of preferences, reflecting regional differences in economic activity. In addition, the political institutions of federalism—such as powerful regional offices and bicameral legislatures—compel party politicians to appeal to different sets of constituents and to run for office at different times. Consequently, the possibility of conflict over monetary policy is high in federal systems, which gives politicians an incentive to choose an independent central bank.

Second, the argument helps explain the stability of central bank institutions in the 1970s and 1980s. Despite changes in the international monetary environment and increasing inflation in many countries, domestic political institutions remained stable and provided politicians with relatively constant incentives regarding central bank institutions. As a result, politicians did not pursue central bank reform. Yet, economic changes during this period had consequences that, by the late 1980s and early 1990s, began to alter politicians' incentives (Keohane and Milner 1996). The new cleavages that developed between export sectors and those insulated from international competition contributed to conflicts within the major governing parties over monetary policy. In addition, the relationship between monetary policy outputs and outcomes became less predictable, which decreased the ability of governments to control the macroeconomy. In some countries, the effects of economic globalization significantly altered legislative-government relations, creating the conditions for an independent central bank. In other countries, domestic political institutions insulated those relations from the consequences of globalization and limited the possibility of reform. Thus, changes in the political incentives of legislators, parties, and governments, help account for variations in the timing of recent central bank reforms.

APPENDIX

Appendix Table A-1 provides values for the independent variables. Appendix Table A-2 indicates the regression diagnostics and case weights for model II.

The Alford Index

The Alford index subtracts the percentage of voters from nonworking classes who vote for a left party(ies) from the percentage of blue-collar workers who vote for a left party(ies). To create an Alford index that covers the same period as the dependent variable, I averaged two indexes, one from the 1960s and early 1970s, the other from 1990. For the first I employed data from Powell (1982), which were collected from a variety of sources. For the second I constructed an index using the World Values Survey (1990). Respondents identified both which party they would support in a hypothetical general election and their occupation. Manual workers (skilled, semiskilled, unskilled), foremen/supervisors, and agricultural workers were coded as blue collar. All others were coded as nonworking class. Left parties included Socialist, Labor, Communist, and others. The correlation between World Values index and Powell's data is $r = .52$, significant at the 0.05 level.

Although the two sources had many countries in common, the World Values Survey did not include Australia or New Zealand. I imputed the missing values by regressing the World Values Survey index on Powell's data. Using the coefficients from the equation and the values of the missing countries on Powell's data, I computed the missing values.

TABLE A-1: Independent Variables

Country	Alford Index	Strong Bicameralism	Polarization 1960–1990	Committee Strength	Coalition/ Minority Government	Punishment Index[a]
Australia	0.25	1	0.00	0	0.00	1.00
Austria	0.14	0	0.01	1	0.55	2.54
Belgium	0.20	0	0.17	1	1.00	2.83
Britain	0.33	0	0.02	0	0.02	1.00
Canada	0.01	0	0.00	0	0.32	1.32
Denmark	0.34	0	0.19	1	1.00	2.81
France	0.16	0	0.23	0	0.93	1.70
Germany	0.17	1	0.02	1	1.00	2.98
Ireland	0.13	0	0.02	0	0.61	1.59
Italy	0.15	0	0.36	0	1.00	1.64
Japan	0.15	0	0.15	0	0.20	1.06
Netherlands	0.15	0	0.06	1	1.00	2.94
New Zealand	0.32	0	0.01	0	0.00	0.99
Norway	0.28	0	0.09	1	1.00	2.91
Spain	0.20	0	0.08	0	0.00	0.92
Sweden	0.33	0	0.05	1	1.00	2.88
Switzerland	0.19	1	0.07	1	1.00	2.93
United States	0.13	1	0.00	0	0.60	1.60

Note: For information on variable sources and creation, see the Appendix text.
[a]Computed as: (1 − Polarization) + Committee Strength + Coalition/Minority Government. The calculation of Austria's punishment index, for example, is: (1 − 0.01) + 1 + 0.55 = 2.54.

Powell does not include data for Spain, which was not a democracy until the mid-1970s. Therefore, I used only the data from the World Values Survey for the Spanish case. Finally, I ran each model using Powell's data and the index created by the World Values Survey separately (results not reported). The results for each index were in the predicted direction and were significant ($p < 0.05$), although at lower levels of confidence than the averaged Alford index.

Strong Bicameralism

Lijphart (1984) defines "strong" bicameralism as a situation in which both houses have roughly equal legislative power and are selected according to different electoral rules. I use a dummy variable for countries with strong bicameralism: Australia, Germany, Switzerland, and the United States.

TABLE A-2: Diagnostics and Case Weights

Country	Standardized Residual[a]	Studentized Residual[b]	Cook's Distance[c]	DFFITS[d]	DFBETA Alford Index[e]	DFBETA Strong Bicameralism[e]	DFBETA Punishment Index[e]	Robust W[f]	Bounded W[g]
Australia	−1.35	−1.39	0.30	−1.14	−0.39	−0.89	0.63	0.92	0.30
Austria	0.73	0.71	0.02	0.29	−0.16	−0.12	0.15	0.93	1.00
Belgium	−1.63	−1.75	0.10	−0.70	0.08	0.29	−0.49	0.46	0.48
Britain	2.22	2.66	0.49	0.16	1.19	−0.17	−0.99	0.00	0.20
Canada	1.21	1.23	0.21	0.93	−0.79	−0.25	−0.18	0.97	0.36
Denmark	1.65	1.77	0.23	1.02	0.69	−0.22	0.43	0.01	0.33
France	−0.38	−0.37	0.00	−0.11	0.04	0.05	0.02	0.97	1.00
Germany	0.48	0.47	0.02	0.32	−0.03	0.23	0.14	0.99	1.00
Ireland	0.44	0.43	0.00	0.15	−0.08	−0.06	−0.04	0.96	1.00
Italy	−1.31	−1.35	0.04	−0.44	0.19	0.18	0.10	0.58	0.76
Japan	0.00	0.00	0.00	0.00	−0.00	−0.00	−0.00	0.99	1.00
Netherlands	−0.02	−0.02	0.00	−0.01	0.00	0.00	−0.01	0.96	0.67
New Zealand	−0.86	−0.85	0.06	−0.50	−0.33	0.05	0.31	0.98	1.00
Norway	−0.86	−0.85	0.04	−0.39	−0.15	0.12	−0.24	0.98	0.86
Spain	−0.41	−0.40	0.00	−0.17	−0.01	0.04	0.13	0.99	1.00
Sweden	−0.34	−0.32	0.00	−0.18	−0.11	0.04	−0.09	0.91	1.00
Switzerland	0.47	0.46	0.02	0.30	−0.01	0.23	0.13	0.98	1.00
United States	0.28	0.28	0.00	0.17	−0.42	0.14	−0.05	0.99	1.00

[a]Cutoff for influential observation: |Standardized Residual| > 2.0 (Bollen and Jackman 1990).
[b]Cutoff for influential observation: |Studentized Residual| > 2.0 (Bollen and Jackman 1990).
[c]Cutoff for influential observation: $D_i > 4/n$. When $N = 18$, $D_i > 0.22$ (Granato, Inglehart, and Leblang 1996).
[d]Cutoff for influential observation: $\text{DFFITS}_i > 2*(k/n)^{1/2}$. When $N = 18$, $\text{DFFITS}_i > 0.94$ (Bollen and Jackman 1990).
[e]Cutoff for influential observation: $|\text{DFBETAA}_i| > 1.0$ (Bollen and Jackman 1990).
[f]Robust regression weights (Western 1995).
[g]Bounded-influence weights (Welsch 1980).

Polarization

Following Powell (1982), I measure polarization as the percentage of electoral support for extremist parties. According to Powell, parties exhibit one of the following characteristics: (1) a well-developed nondemocratic ideology; (2) a proposal to break up or fundamentally alter the boundaries of the nation; or (3) diffuse protest, alienation, and distrust of the existing political system. I updated Powell's measure to include the average polarization for 1960–90. I follow Powell's (1982) classifications with the following exceptions: France (include National Front) and Spain (include Communist and separatist parties).

Committee Strength

I use a dummy variable for systems with "strong and inclusive" committee systems. Following Strom (1990b), Powell and Whitten (1993) define a committee system as strong if it has two of the three following characteristics: size (more than ten committees), specialization to fit the government bureaucracy, and limitations in the number of committeeships held by individual legislators. Inclusive committee systems require that committee chairs be distributed proportionally among all parties, regardless of their participation in government. Examples of countries with strong and inclusive committee systems are Austria, Belgium, Denmark, Germany, the Netherlands, Norway, Sweden, and Switzerland.

Coalition and Minority Government

I calculated the proportion of time a country was governed by a coalition or minority government from 1960 to 1990 using data from Woldendrop, Keman, and Budge (1993). I counted instances of "divided government" in the United States as minority government, since the president's party lacks an overall majority in Congress.

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Party Aggregation and the Number of Parties in India and the United States

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We rely on data from India and the United States to show that political and economic centralization can influence the number of national parties in single-member simple-plurality electoral systems. Historically, in both countries the number of parties in local electoral districts has been near two, but the number of national parties has fluctuated. Periods of a small number of national parties in both countries correspond to periods of centralization. We argue that, as national governments centralize power and make policies that affect local areas, candidates have greater incentives to associate with national organizations, and voters have greater incentives to abandon locally competitive but nationally noncompetitive parties.

Prominent explanations for differences in the number of political parties across countries, most notably Duverger's Law and the literature related to it, have focused on the role of electoral systems. Differences in district magnitude, electoral formulas, the number of run-offs, and presidentialism have each been hypothesized as determining the number of parties (Cox 1997; Duverger [1954] 1963; Lijphart 1994; Rae 1971; Riker 1982; Shugart and Carey 1992; Taagepera and Shugart 1989). Ethnic heterogeneity has also been associated with multiple party systems.[1] In accounting for changes in the number of national parties over time within individual countries, however, explanations based solely on electoral systems or population diversity are strained. These features rarely change much within countries, and certainly not as often as party systems undergo change in some countries, such as Italy, India, Mexico, or even, as we shall show, the United States.

We examine party system change in India and the United States. Both countries are federal (with relatively strong state governments), and both have single-member, simple-plurality electoral systems. Yet, in both countries the number of parties has varied over time.[2] Although the United States is widely recognized as a two-party system and India is often categorized as a one-party dominant system, the number of parties in the United States stabilized around two only after the New Deal, and the number of parties in India has fluctuated between two and seven.[3] Since neither country has undergone qualitative changes either in electoral arrangements or in ethnic heterogeneity that correspond to the changes which have taken place in the party systems, one must look elsewhere for explanations of the variation in the number of parties over time in both countries.

We rely on data from India and the United States to argue that the degree of political and economic centralization can influence the number of national parties in single-member, simple-plurality systems. Party systems are formed largely on the basis of voters' and candidates' incentives to coordinate on common party labels. These incentives are related not only to electoral laws and ethnic divisions but also to the role of the national government in local politics and local economies. In particular, the cases of India and the United States suggest that, as national governments exert more political or economic control over local areas, candidates have greater incentives to associate themselves with national organizations, and voters have greater incentives to abandon locally competitive but nationally noncompetitive parties. Under increasing centralization, there will be greater similarities in party systems across different levels of vote aggregation (local, state, national). This means that, in single-member, simple-plurality systems like India and the United States, as the national government centralizes authority, we should expect the number of parties at the national level to decline and approach the number of parties at the local level, a number that will be close to two.

In the next section we propose that national party systems are best understood as aggregations of local party systems and that this conceptualization has con-

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We thank Richard Agin and Dulcey Simpkins for their excellent research assistance. Rob Franzese and Chris Achen provided valuable advice for the quantitative analysis. Comments by Andre Blais, Jenna Bednar, Nancy Burns, Jay Dow, Mike McDonald, Jacqueline Stevens, Rein Taagepera, and Mariano Torcal on earlier drafts are greatly appreciated. Data for the U.S. elections were made available by the Inter-university Consortium for Political and Social Research, University of Michigan, which bears no responsibility for conclusions or interpretations presented. Finally, we acknowledge financial assistance from the Horace H. Rackham School of Graduate Studies and the Office of Vice President for Research, University of Michigan.

[1] Ordeshook and Shvetsova (1994) and Amorim and Cox (1997) have shown that ethnic heterogeneity correlates with multiple parties across countries, especially in proportional representation systems. Key (1949), among others, made the point that the number of parties can be related to the diversity of political interests in the population. Diversity of political interests, of course, may or may not be related to ethnic diversity.

[2] A standard formula from Laakso and Taagepera (1979) is used in this paper to measure the number of parties.

[3] Kothari (1964) first classified India as a one-party dominant system. Lijphart's (1994) analysis of India, as well as our own here, questions that characterization.

sequences for the conclusions we draw from data on electoral institutions and party systems. We then describe how the role played by the national government influences the way local parties aggregate into national parties. Next, we provide evidence from India and the United States to support our argument. We end with a discussion of alternative theories of party aggregation.

NATIONAL AND LOCAL PARTY SYSTEMS

India and the United States have similar electoral and political institutions. Both elect a national lower house from local electoral districts; both are federal systems in which localities, states, and the national government share power[4]; and both apply single-member, simple-plurality rules in all electoral districts for legislative seats.[5] Moreover, most electoral districts in India and many in the United States are ethnically heterogeneous, and all are large enough to require mass campaigning by candidates, thus permitting or even encouraging the development of mass party organizations within district boundaries. One major institutional difference between the two countries is that the United States has a divided executive and legislature, with the executive elected (somewhat) directly, while India vests executive power in parliamentary government.

Duverger ([1954] 1963), of course, held that single-member, simple-plurality systems lead to two parties. His argument, in short, is that voters have incentives to abandon noncompetitive parties and vote for parties that have a chance of winning a share of political power. Under single-member, simple-plurality rules, only two parties consistently have a chance to win seats in the legislature (or so the argument goes). Even with recent amendments, additions, confirmations, and alternatives to Duverger (Cox 1987, 1997; Feddersen 1992; Myerson and Weber 1993; Palfrey 1989; Riker 1982; Sartori 1986), the theoretical conclusion that single-member, simple-plurality rules leads to two-party systems is fairly robust. The expected consequences for India and the United States would seem to be clear.

Empirically, however, matters are not so simple. There are the troublesome outliers to Duverger's Law. India, for one, has always been seen as exceptional. By just about any measure, the number of parties nationally in India has not been two for most periods in recent history, which seems to contradict Duverger's Law directly (Lijphart 1994). Riker (1976, 1982), in explaining the exceptionalism of the Indian case, claims that the one-party dominant system (until the 1980s) resulted from the structure of voters' preferences. The median position occupied by the Congress Party in the national electorate led it to be the consistent plurality choice (Condorcet winner) *nationally*.[6] Other researchers have either dropped India from empirical research on Duverger's Law or have just marked it as a troublesome statistical outlier (King and Janda 1985; Lijphart 1994; Ordeshook and Shvetsova 1994; Rae 1971; Taagepera and Shugart 1993).[7]

A far deeper problem with research on electoral laws in general and on Duverger's Law in particular is a confusion over predictions at the district level and at the national level. In nearly all the empirical research on the political effects of electoral laws, the one major dependent variable of interest is the number of (viable, enduring) parties at the national level. For example, Rae (1971) and Lijphart (1994) use national vote proportions or national legislative seat proportions to construct ordinal or continuous measures of the number of parties. Lijphart, for one, then compares a country's average number of seats in a district with the number of parties in that country. Presumably, the way votes are counted and used at the level of the electoral district to allocate seats in the national legislature influences the formation, sustenance, and success of national parties. The conclusions typically drawn are that Duverger's Law works or does not work because of evidence from national party systems (see, for example, Palfrey 1989, 69).

The problem with this approach is that, as a number of researchers have pointed out (Cox 1997; Cox and Monroe 1995; Gaines 1997; Leys 1959; Sartori 1986; Wildavsky 1959), Duverger's Law and its theoretical descendants essentially deal with district-level phenomena. In a country using single-member, simple-plurality rules with m electoral districts, there could be two parties in each district, but if vote proportions are used to measure the number of parties, there could be as many as $2m$ parties at the national level. This is because each candidate at the local level could hypothetically constitute a single political party. If candidates in other districts do not adhere to the same party label, then district-level votes will not aggregate into sizable or competitive national-level vote totals. In other words, for a two-party system to emerge at the national level, candidates and voters across electoral districts must coordinate on party labels. The focus of attention for national party systems, therefore, should be on what we term *party aggregation*, or the coordination of candidates or voters across electoral districts into national political parties.[8] The conditions that make India an outlier for national-level predictions

[4] The nature of federalism in the two countries, especially the economic and political relationship between the national government and state and local governments, is different and has changed over time.

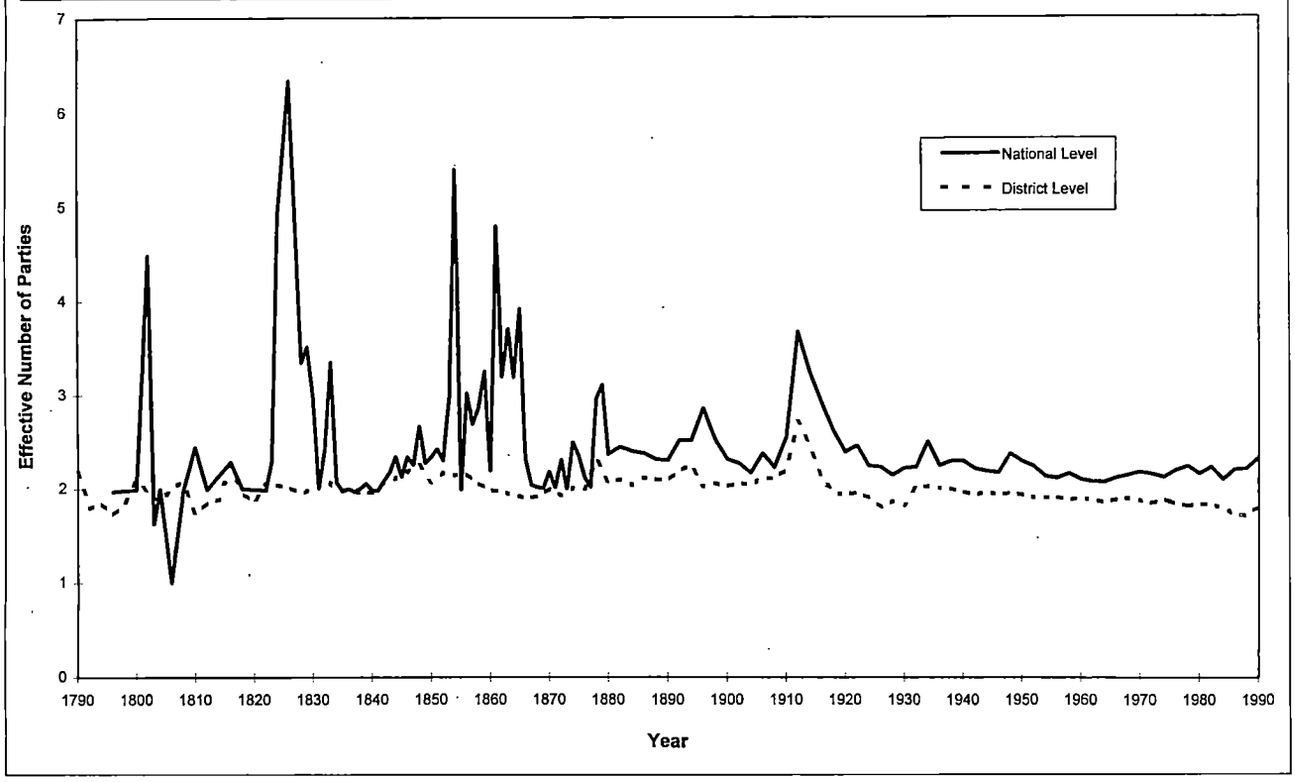
[5] India had a few two-member constituencies in the 1952 and 1957 national elections.

[6] Riker (1982) argues that, by occupying the median position, the Congress Party could maintain its dominance by preventing a majority opposition from forming. A majority of voters preferred Congress to alliances with bitter rivals. Recent research on the effective number of parties in India, most notably that of Lijphart (1994), suggests that Riker may have overstated the party's margin of dominance. Congress faced significant electoral competition even in the period of legislative dominance. Nevertheless, the fact that Congress was the party of the independence movement certainly helped it maintain its electoral predominance.

[7] Other single-member, simple-plurality countries, such as Canada, present empirical problems for Duverger's Law as well (Gaines n.d.; Riker 1982).

[8] Cox (1997) refers to this process as linkage.

FIGURE 1. Effective Number of Parties at the National and District Level in the United States (non-South)



may actually be perfectly consistent with Duverger if local districts approximate two-party systems and party aggregation is taken into account.

To observe the effects of party aggregation, we compared district-level data and national-level data in India and the United States. We calculated the effective number of parties in each electoral district for the lower house in each country (Lok Sabha and the House of Representatives).[9] The measure we use is the effective number of parties, N, from Laakso and Taagepera (1979). N is a continuous measure that weights larger parties (those gaining more votes or seats) significantly more than smaller parties (see the Appendix for more discussion). It is calculated as follows. For n parties receiving votes or holding seats,

$$N = \frac{1}{\sum_{i=1}^{n} p_i^2}.$$

The element p_i can be the proportion of popular votes attained by party i in the election or the proportion of seats in the legislature controlled by party i. In this analysis we use the proportion of popular votes.

The average of N across electoral districts, provides a measure of the effective number of parties at the district level (D_N) for lower house elections. If there are d districts in a given year, then D_N for that year is calculated as follows:

$$D_N = \frac{\sum_{j=1}^{d} N_j}{d}.$$

National data can be obtained by aggregating party vote totals across all districts for lower house elections for a given year. Let v_{ij} be the vote for party i in district j in a given year, and let V be the total vote for all parties in the nation in that year. For n parties nationally,

$$V = \sum_{i=1}^{n} \sum_{j=1}^{d} v_{ij}.$$

Thus, with v_i being the total vote for party i in the given year, our national-level measure of the number of parties (N_N) is

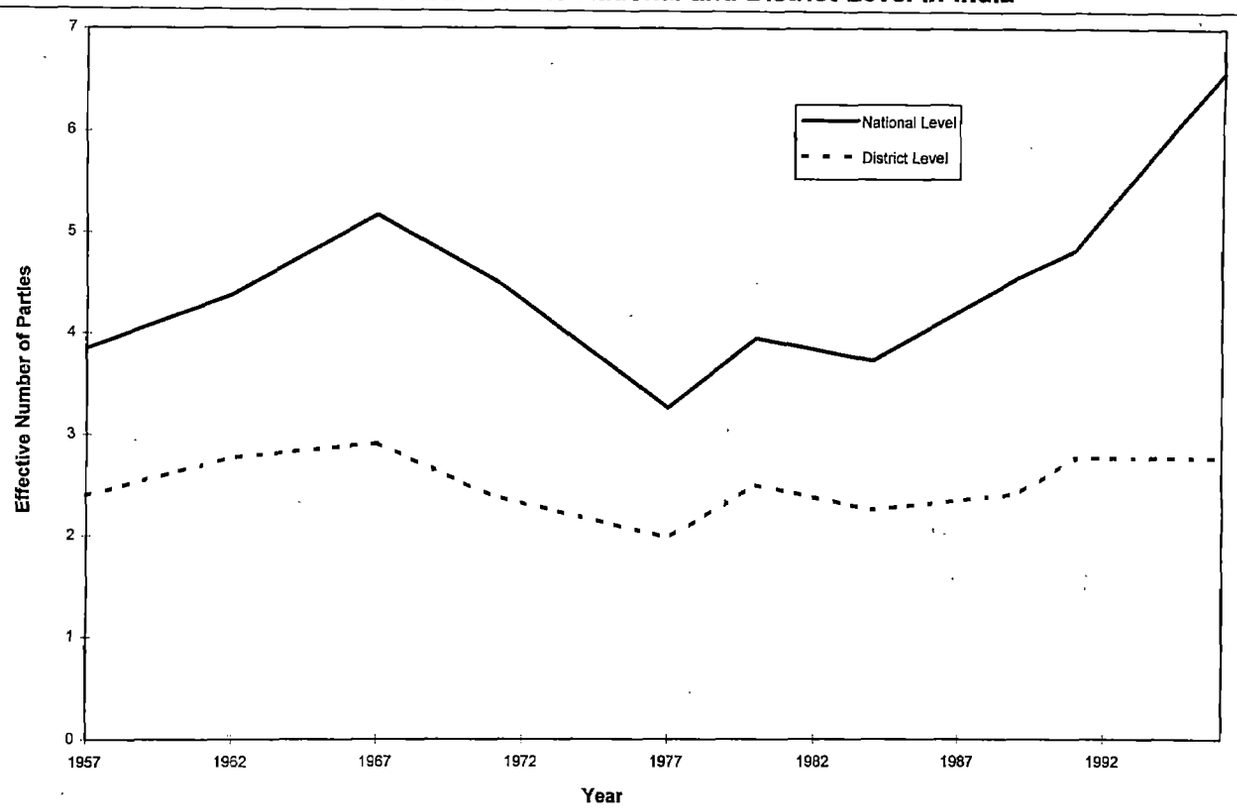
$$N_N = \frac{1}{\sum_{i=1}^{n} \left(\frac{v_i}{V}\right)^2}.$$

Figures 1 and 2 show D_N and N_N for both countries over the course of their history of democratic elections.[10] Several patterns are immediately apparent in

[9] Indian electoral districts are called constituencies. We use the word *district* because most readers are familiar with the term and for consistency with the U.S. data.

[10] Data sources are described in the Appendix. We use the non-South only for the United States for all calculations. Also, we do not include the 1952 elections for India because state boundaries were redrawn before the 1957 national elections and have remained mostly unchanged. There is little reason to believe that adding the 1952 elections would alter our conclusions significantly.

FIGURE 2. Effective Number of Parties at the National and District Level in India



the figures. First, D_N for the United States varies only a little around two, meaning that on average two candidates have received a vast majority of the vote in electoral districts for the House of Representatives. The D_N for India reveals that between two and three candidates receive nearly all the vote on average in the districts.[11] Contrary to the conclusion of many studies that report India as a one-party or even a four-party system, the basic single-member, simple-plurality hypothesis of Duverger receives some support. At a minimum, the number of parties is much lower than the number of deep cleavages in most Indian districts. This important finding seriously questions the widely held belief that the numerous Indian "ethnicities" (castes and religions) axiomatically inflate the number of parties. Put another way, and more generously to those in the Duverger tradition, since D_N hovers around 2.5, it is doubtful that ethnic heterogeneity, irrespective of electoral system effects, can account for the number of parties. Furthermore, while the Congress Party has dominated the national government, others have gained significant percentages of the district vote and can be considered competitive locally.

Second, for almost all years in both countries, D_N is less than N_N, and the difference provides a measure of party aggregation problems. We use that measure in figures 3 and 4, which will be discussed in more detail later. The larger the difference, the more severe is the problem of party aggregation. That is, a small number of candidates receive votes in each electoral district, but the same parties do not exist in all districts, and hence more parties receive votes at the national level. A difference near zero between D_N and N_N would suggest that parties aggregate nearly perfectly. Therefore, the data depicted in figures 1 and 2 suggest that two parties tend to gather a vast majority of the district votes in both India and the United States, but in many periods the same parties are not present in all districts.

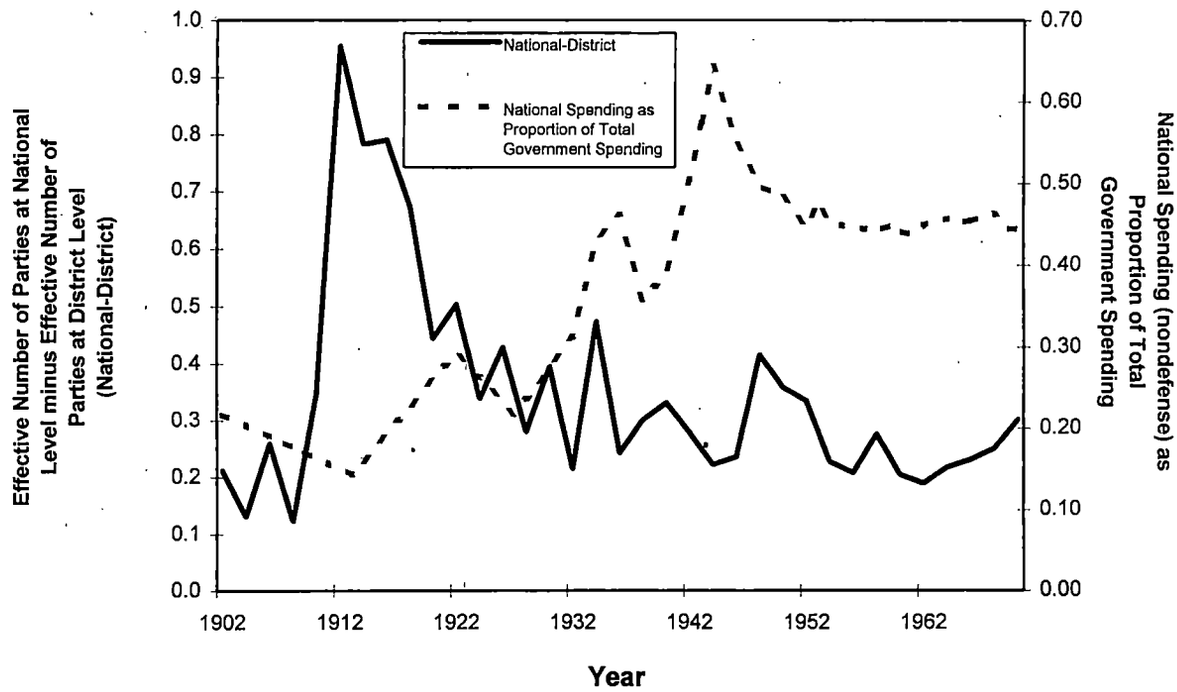
Third, the extent of party aggregation is neither constant over time for either India or the United States, nor is it the same for both countries. On the one hand, in the United States there was a marked change in the 1930s. Before that time, the number of effective parties at the national level fluctuated above two, but then it settled to a stable number near two afterward. On the other hand, India displays a different pattern. The effective number of parties nationally rose in the early 1960s, dropped in the 1970s and 1980s, and then began to rise again.

These variations provide an opportunity to examine why party aggregation is more pronounced in some periods than in others. Since the district-level data stay relatively constant, what accounts for changes in the number of national parties over time?

[11] Third-party candidates have received more than 20% of the vote in only 15% of the Indian constituencies since 1957. The latter number has varied, however, from 22% in 1980 to 2% in 1977.

FIGURE 3. Party Aggregation and Government Spending in the United States (non-South)

3A. Party Aggregation and National Share of Total Government Spending



3B. Party Aggregation and National Government Spending as Proportion of GNP

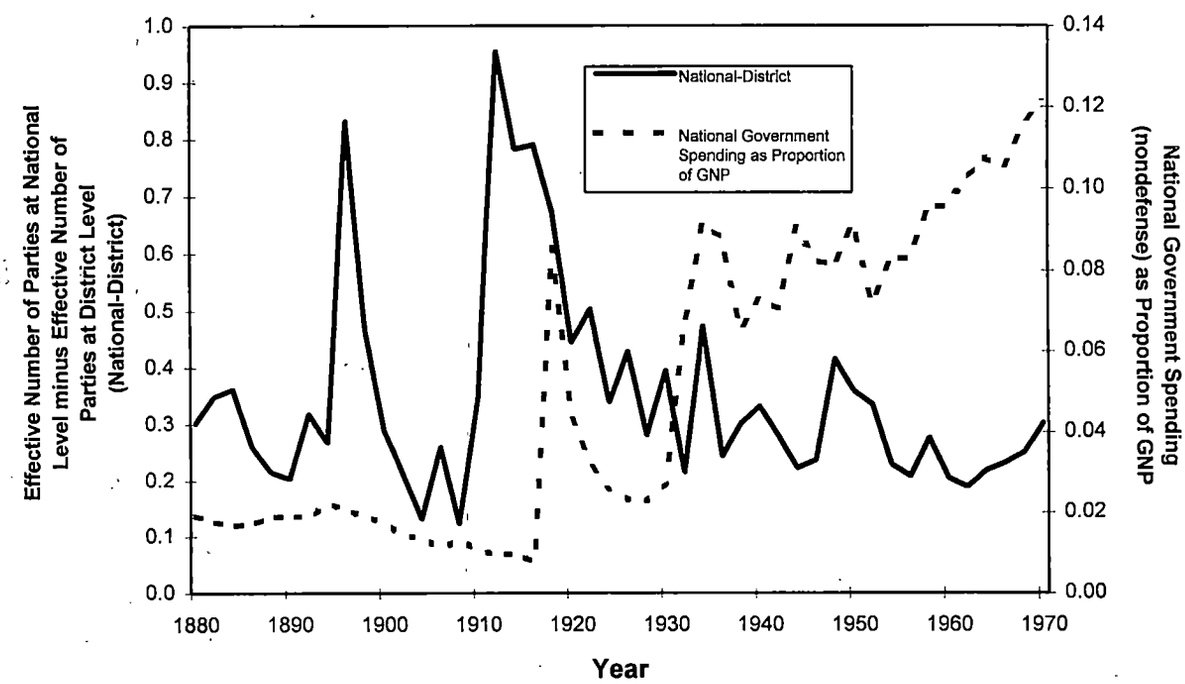
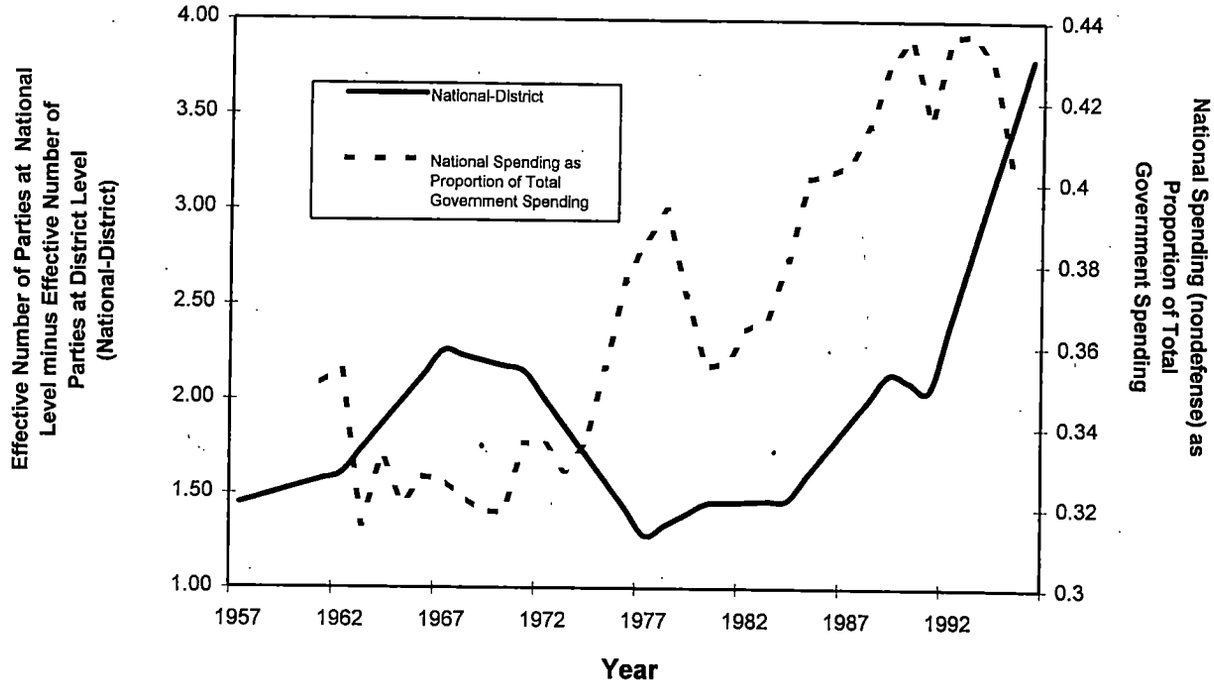
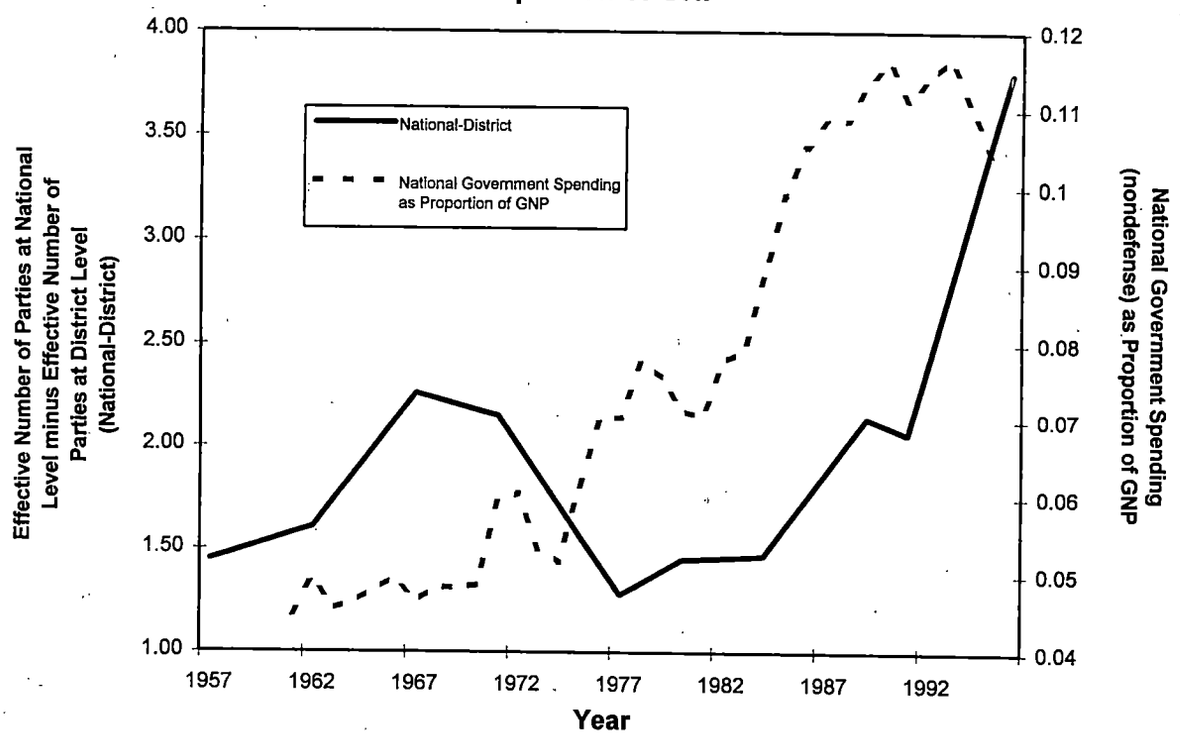


FIGURE 4. Party Aggregation and Government Spending in India

4A. Party Aggregation and National Share of Total Government Spending



4B. Party Aggregation and National Government Spending as Proportion of GNP



WHAT AFFECTS PARTY AGGREGATION?

National party systems reflect party aggregation across electoral districts. Party aggregation, in turn, reflects the incentives for candidates to coordinate on common party labels and for voters to support parties with broader appeal outside a single electoral district.

Candidates

Candidate coordination across districts—that is, adopting common party labels—can occur for two reasons. First, candidates can respond directly to voters' policy preferences, which may be locally or nationally oriented. In some instances, voter preferences are dominated mostly by local concerns, and those preferences are used to decide among legislative candidates. Candidates, whom we can assume to be office seekers, will take on party labels that respond to voters' policy positions on local concerns. These labels may or may not correspond to national party labels. In the United States, for example, local candidates could call themselves Republicans or Democrats, if these labels communicate to voters something about local policy positions, or they could just as well call themselves Greens and Reds, if these labels were better suited to local conditions. The key point is that party labels may very well differ in surrounding districts. In other instances, voters consider mostly national policies when deciding among legislative candidates. In situations where the national government makes most decisions, such as those on taxation, the allocation of state resources, or local school curriculum, these decisions will form the basis of voters' preferences over candidates. Voters will then develop a set of preferences over the party composition of the lower house and vote according to those preferences. Candidates facing these voters will have incentives to take on party labels that communicate national policy positions.

Second, candidates may also want to coordinate across districts to influence national policies. Clearly, if the national government makes most of the decisions on policies of concern to voters, then we can expect local candidates to affiliate with parties that can make it easier for them (upon becoming legislators) to influence those decisions. Taken farther, if part of the task of the national government is to distribute resources to different parts of the country, then local leaders will want to identify with national parties in order to gain a share of the resources. They will want to affiliate not only with the party in power but also with other parties that may have a chance of attaining power. Furthermore, they may seek to associate themselves with other candidates representing similar national policy preferences and communicate to voters their combined potential power to influence national policymaking.[12]

[12] Common party labels make it easier for public officials to cooperate across levels of government (Aldrich 1995; Eldersveld 1982) and also enable ambitious politicians to attain higher level offices (Schlesinger 1991).

Voters

Under a system with centralized authority, voters see themselves as either benefiting or losing from national policies and, as a consequence, develop preferences regarding those policies. It may even be rational for them to vote for a less preferred party if it is nationally competitive (Cox 1997, chapter 10; Leys 1959). Cox and Monroe (1995) discuss voting equilibria in which voters have preferences about the composition of the national legislature and have well-defined expectations about how parties will fare in the elections nationally. They conjecture that, in equilibrium, voters will often vote for less preferred parties. For single-member, simple-plurality systems, this means they will usually abandon nationally noncompetitive parties, even though these may be locally competitive. Furthermore, in such a system, two nationally competitive parties will tend to emerge. As Cox and Monroe (1997, 14–5) summarize their argument, "the net impact of national parliamentary considerations on local Duvergerian dynamics should be clear.... There are multiple dynamics that serve to (1) drive down the number of parties, and (2) reinforce the national party system at the district level.... These dynamics... are particularly strong in single-member district systems."

Voters in India and the United States, of course, have neither purely local concerns nor purely national concerns. They vote for legislators both to represent local interests and to take part in national policymaking. In both countries, however, there have been changes in the degree of political and economic centralization.

Expectations

Party aggregation is more difficult to achieve when voter preferences are mostly locally based and candidates do not have to coordinate with politicians in other districts. Both voters and candidates are inclined to focus on local matters and local labels. In India, for example, many voters choose regional parties that are ideologically similar to parties in neighboring regions, although each maintains a distinctive label and organization.[13] As the national government centralizes power, voters develop national policy preferences, and candidates associate themselves with certain national policy positions. As a result, local party systems and national party systems begin to resemble each other.

To summarize, for single-member, simple-plurality systems, the electoral system has its most direct effect on the district level. Voters tend to abandon noncompetitive local parties for Duvergerian reasons. The dominant tendency is for two parties to capture most of the vote consistently at the district level (figures 1 and 2). At the national level, Duvergerian effects are found

[13] The Samajwadi Party in Uttar Pradesh and the Janata Dal in Bihar represent similarly situated castes, and both parties are not ideologically different. Each prefers to retain its independent organization. Their cooperation at the national level in 1996 was somewhat strained. This pattern of behavior is more noticeable in some periods than in others.

when national policies matter at the local level. We would expect to see two parties at the national level only when voters have national policy preferences and when candidates who represent voters with similar preferences across districts see the obvious advantages to affiliating with other candidates.

EVIDENCE

If party aggregation is sensitive to the degree of centralization in the manner just described, then we should observe a larger difference between N_N and D_N in periods when the central government is less dominant or becoming less dominant. As D_N is expected to stay fairly constant (near two) in single-member, simple-plurality systems, most of the variance across time for India and the United States should occur with N_N. When N_N rises, it reflects increasing problems of party aggregation. This section demonstrates that patterns of party aggregation and political and economic centralization in India and the United States largely support these conclusions. In both countries, party aggregation is more successful when the national government centralizes authority.

United States

Returning to Figure 1 for the United States, before the New Deal, and especially before introduction of the national income tax in 1913, N_N fluctuates from near two to as high as 6.5 in the early nineteenth century. The periods corresponding to the Populist and Progressive movements also show expected punctuations in N_N. These patterns reflect the regional or local bases of minor parties in the United States historically. In some regions in the late nineteenth century, for example, Republicans faced their most significant opposition from Populists, while in other regions the threat came mostly from Democrats. Nationally, both Populists and Democrats aggregated noticeable numbers of votes for the House of Representatives, causing N_N to be higher for that era. In the early twentieth century, the Progressives were largely a collection of locally based parties that did not necessarily coordinate their efforts across districts and even had slightly different labels in neighboring states (Shefter 1994). In sum, before the New Deal, the national political system was one of locally rooted parties that gathered and "handed out divisible economic benefits to meet their patronage requirements" (Finegold and Skocpol 1995, 54). While these local parties often were labeled Democrat or Republican, in some places parties such as Labor, Progressive, Socialist, Prohibitionist, or Farmer were one of two locally dominant political organizations.

The political and economic centralization of the 1930s obviously had a profound effect on local politics and local economic policies. Referring to Figure 3, the dotted lines depict two economic indicators of what occurred during this time. National government spending (nondefense) as a proportion of total government spending (including state and local spending) more than doubled during the period. National government (nondefense) spending as a proportion of GNP rose nearly tenfold from the turn of the century to the 1930s and continued to rise. The New Deal, in many ways, centralized public taxing and spending decisions.

The New Deal also centralized political authority. According to Milkis (1993, 9)[14], it brought about the "nationalization of the political system . . . [and] established conditions for the emergence of a more national and programmatic party system." The new policies in some cases weakened more conservative Democratic machines in big cities, such as New York (Erie 1988, 108). But they also brought into the Democratic fold Republican Progressives in Minnesota and Farmer-Labor members in Wisconsin (Milkis 1993). They strengthened local Democratic machines in Chicago and Pittsburgh and maintained the Democratic hold on many parts of the South (Key 1949). Roosevelt's policies, in sum, marked the most substantial political and economic centralization in American history, causing voters and politicians to orient their party affiliations either in support of or in opposition to them.

After the New Deal, N_N drops to near two and stays there, as shown by the solid lines in Figure 3. Clearly, great surges in third- and fourth-party strength at the national legislative level ceased in the 1930s, causing the problem of party aggregation to recede. Our argument is simply that this aggregation pattern was driven by American voters' increasing consideration of national policies when choosing among House candidates.[15] Nearly all viable legislative candidates, in response, found it increasingly in their interest to affiliate with one of the two major parties as opposed to identifying with local or regional parties.[16]

More systematic evidence for the relationship in the United States is presented in Table 1. Equation 1 summarizes a regression model in which the number of national parties (N_N^t) in a given election year (t) is regressed on its lag (N_N^{t-1}) and on central government spending (nondefense) as a proportion of GNP (X_t) (i.e., the data from Figure 3B). The coefficient of interest is on X_t, and it shows that X_t is negatively related to the number of national parties (and statistically significant), controlling for the previous number of national parties. This is exactly as we would expect, leading to the interpretation that greater fiscal (and by

[14] Our argument is different from that of Coleman (1996), who emphasizes the civil service reform that accompanied the New Deal as a cause of party decline (as measured by party vote discipline in Congress). Our claim does not focus either on party weakness or congressional votes but merely states that, with the New Deal, national policy preferences began to dominate voting decisions in House elections.

[15] This conclusion is supported by numerous studies of American electoral behavior. See Key 1949 and Burnham 1970 for classic accounts.

[16] Another interesting aspect of Figure 1 is the decline in the size of surges in N_N after the Civil War, another great period of centralization. This adds evidence of the effects we highlight here. We believe that N_N declined in response to centralization during and after the Civil War and then declined further (and most completely) after the New Deal. Note, however, that most of the fluctuations in the periods prior to the New Deal are not explained here. Essentially, our argument is that once a threshold of centralization has been passed (after the New Deal), party aggregation occurs rapidly.

TABLE 1. Effects of Centralization on the Number of National Parties in the United States

Variable	Equation 1	Equation 2	Equation 3
Constant	1.23***	1.39***	1.36***
	(0.24)	(0.34)	(0.28)
X_t (fiscal centralization)	−1.91*	−2.14	−2.11
	(0.97)	(1.16)	(1.14)
N_N^{t-1}	0.52***	0.62***	0.62***
	(0.09)	(0.13)	(0.13)
N_N^{t-2}		−0.16	−0.18
		(0.17)	(0.21)
N_N^{t-3}			0.03
			(0.12)
n	46	46	46
F	21.10***	15.61***	16.98***
R^2	.47	.49	.49
Durbin-Watson	1.6	1.8	1.8

Note: Shown are OLS coefficients with robust standard errors in parentheses. The dependent variable is the number of national parties in election year t (N_N^t). The independent variable X_t is national government spending (nondefense) as a proportion of GNP (i.e., the economic data from Figure 3B). *$p < .05$, ***$p < .001$.

implication political) centralization leads to fewer national parties.[17] In equations 2 and 3 in the table we explore the robustness of the result. We lag the dependent variable a second and third time to make sure that the effect of fiscal centralization does not disappear if we change the lag structure of the model. As the table shows, the estimated effect of X_t on the number of parties does not change much from the model summarized in the first column, although the probability level for the coefficients on X_t in the second and third columns goes to .07 in each.

India

The effects of the political and economic centralization that characterized Indian politics in the 1970s and 1980s are as expected. The party system moves in the direction of a two-party system nationally during much of that period. In the late 1980s, however, political centralization and the consolidation of parties were reversed, and in the 1990s India was once again a system with more than four national parties. The pattern—increasingly successful party aggregation and then a reversal—conforms well to our argument.

As evident in Figure 2, from the elections of 1957 to those of 1967, there were increases in N_N (and $N_N - D_N$). In other words, party aggregation increasingly failed in this period. Analysts have noted the weakening power of the central government, especially between 1964 and 1967, when power within the ruling Congress Party was held by a collection of state leaders, commonly called the Syndicate (Brecher 1966; Frankel 1978; Kochanek 1968; Rudolph and Rudolph 1987). As Manor (1988a, 68) succinctly notes, in this period the center's will did not dominate, as "bargaining had long been an important element of relations between New Delhi and the states, even in Nehru's day when Congressmen held sway at both levels."

Political and economic centralization in India began with the 1971 elections. For most of the next decade and a half, Indira Gandhi, who was prime minister between 1971 and 1977 and again from 1980 to 1984, drastically centralized power and resources. "She created a pyramidical decision-making structure in party and government . . . [and this] centralized decision-making" (Kochanek 1976, 104). She also forged direct ties with local leaders, bypassing state leaders. Besides candidates competing formally for elective office, local elites ("brokers") are important in Indian politics.[18] The incentives of these local elites to align with nationally competitive parties changed in concert with the increased dispersal of resources for rural programs by the central government (Brass 1990). As the central government augmented its role in local affairs, local elites aligned with national parties. A similar degree of political centralization was not sustained under Rajiv Gandhi from 1985 to 1989. He was unable to control even the various factions within the Congress Party (Frankel 1987, 38; Manor 1988b, 9; Weiner 1987, 15).

In addition to the well-known centralization of political power under Indira Gandhi, evidence summarized in the two panels of Figure 4 shows that economic centralization occurred as well.[19] Figure 4A demonstrates that national government spending (nondefense) as a proportion of total government spending (nondefense) increased only after Indira Gandhi came to power. And national government spending (nondefense) as a proportion of GNP almost doubled during her reign (Figure 4B). The solid lines in these figures show that party aggregation moved in opposition to the economic data during every period until 1985.

The data summarized in Figure 4 for the period 1985–89 appear to run counter to our theory. During these four years, both the economic data and the number of parties move upward. The data in the late 1980s, however, are somewhat misleading. Much of the increase in national government expenditures can be attributed to larger grants to the states and union territories and to interest payments on debt, the kind of spending that need not indicate centralization of authority. Three-fifths of the growth in national government spending as a proportion of GNP between 1985 and 1989 can be attributed to these types of expenditures, a proportion that is much higher than in earlier periods.[20] The well-known inability of Rajiv Gandhi to

[17] We also recalculated the coefficients using a Hildreth-Lu estimator (because the Durbin-Watson is 1.6). This procedure corrects for serially correlated residuals in small samples (Neter, Wasserman, and Kutner 1989, 500). The coefficient for X_t goes to −2.34 with a standard error of 1.17 ($p = .05$). The transformed Durbin-Watson is then 1.96. This result means that serial correlation does not alter the main conclusion of the model.

[18] For the critical role played by brokers and local elites, see Mitra 1992 and Reddy and Haragopal 1985.

[19] Hart (1976) and Manor (1994) provide evidence for the concentration of central authority in this period.

[20] In fact, when we use an alternative economic measure, the proportion of total government spending (as in Figure 4A) excluding debt payments, the results from statistical analysis look more consistent with our hypothesis in all periods, including these four years. For

maintain his mother's political and economic centralization is not properly reflected in these data.[21]

Accompanying the increased expenditures under Indira Gandhi was greater economic policy intervention by the central government, often in direct contradiction of prevailing political norms. For instance, although agriculture is constitutionally not the province of the central government, the share of federal spending in that sector quadrupled (India 1976). The central government also amended the constitution to deprive states of revenues from advertising on state-owned radio and television. The location of industries—both in the private and public sector—was decided by the national government. Through the 1970s and mid-1980s the central government, through schemes it sponsored, largely determined the spending priorities of state and local governments (Bagchi 1991).[22]

As expected, N_N drops and there is more effective party aggregation between 1971 and 1984. Once again, however, the pattern reverses in the late 1980s because, while Indira Gandhi was relatively successful in centralizing power through the early 1980s, Rajiv Gandhi was not as adept. For instance, in 1989 he attempted to introduce the 64th Amendment to the constitution. It would have redefined government relationships, with state governments ceding a fair degree of fiscal control to local governments, whose finances would have come under the direct purview of the central government. The amendment failed in the upper house.[23]

Problems with party aggregation therefore increased again, especially after 1991. As Yadav (1996, 95) notes, after 1991 the party system "cannot be aggregated at the national level." This change occurred in response to a marked decline in political and economic centralization. National government expenditures as a proportion of GNP showed a steady but small decline after 1990, and between 1991 and 1995 central government revenue expenditures declined almost 2% (India 1996b, 20). Furthermore, the changes in economic policy introduced in 1991 differed from earlier attempts to liberalize the economy in one important way: The central government has reduced its role in economic policymaking. The procedure whereby licenses to set up a manufacturing industry are granted by the central government has mostly been abolished. The states not only have greater autonomy in setting industrial policy but also actively compete for investment. The central government also has altered its financial relations with the states, and its grants to the states have declined (India 1996b, 25). Not surprisingly, party aggregation appears to have decreased in this most recent period.

Because there are too few cases for meaningful regression analyses, we do not replicate Table 1 for India. We can summarize the results briefly, however. Coefficients for a regression of the number of national parties (N_N^t) on its lag (N_N^{t-1}) and on the centralization measure presented in Figure 4B reveal the same underlying pattern as in Table 1. The coefficient for fiscal centralization is negative (−.34, with a standard error of .38).[24] More telling for this low number of cases, perhaps, is a simple correlation of first differences between the dependent and main independent variables. Let $\delta_N = N_N^t - N_N^{t-1}$, where δ_N is the change in the number of parties and N_N^t is the same variable as in Table 1, but for India rather than for the United States. Similarly, let $\delta_X = X^t - X^{t-1}$, where δ_X is the change in centralization and X^t is the measure of centralization from either Figure 4A (with total government spending as the baseline) or Figure 4B (with GNP as the baseline). The correlations between δ_N and δ_X are negative using both measures for δ_X ($r = -.26$ for the data in Figure 4A and $r = -.29$ for the data in Figure 4B). In fact, for every period except the late 1980s, when economic centralization increased, the number of national parties decreases; when economic centralization declines, the number of national parties grows.

These data lead us to offer two conclusions. (1) For the United States, the national income tax and the centralization of economic and political power under the New Deal solidified the national two-party system. (2) For India, the centralization of political and economic power under Indira Gandhi brought about a significant reduction in the number of parties for a temporary period. Before the 1930s in the United States, and both before and after Indira Gandhi in India, candidates and voters did not coordinate party labels as effectively because local concerns were more prominent, and voters were not as concerned about the national competitiveness of local parties.

Our analysis so far has been based on a comparison of data across time within the two countries. It is worthwhile at this point to compare briefly party aggregation across the two countries. There are several noticeable differences. The United States has had a more stable number of parties at all levels than India, though it should be noted that the United States did not attain much of its present stability until a century after its founding. N_N is nearly always higher for India than for the United States during most of the twentieth

comparability with the U.S. data, we do not subtract payments on debt.

[21] Between 1975 and 1984, for instance, except for 1977–78, when the opposition Janata Party was in power, central government grants to the states varied between 1.7% and 2.1% of GNP. In 1977–78, a prominent period of decentralization, central government grants to the states were 2.7% of GNP. This is strikingly similar to allocations made to states by Rajiv Gandhi's government, which averaged 2.7% (India 1989).

[22] There are many commentaries on the centralization of expenditures in India. See, in particular, Bagchi, Bajaj, and Byrd 1992, Gulati 1987, and Lakdawala 1993.

[23] In 1992 the Congress government introduced another version (the 73rd Amendment), which was adopted. It made clear that the central government no longer had the ability to scrutinize the accounts of local governments, a right now in the hands of state governments (India 1989, 1992).

[24] This coefficient is derived from a Hildreth-Lu estimation procedure to correct for serial correlation among residuals. Since the estimation procedure requires that periods be evenly spaced. To transform the data appropriately, each election year was treated as an observation. Years between elections were dropped for the estimation.

century. India also has consistently a somewhat larger number of effective parties at the district level (D_N). The overall comparison can be simply summarized by saying that India has more parties than the United States.

These differences may be explained by the greater ethnic, religious, and linguistic diversity of India relative to that of the United States. There is undoubtedly some truth to this explanation. As Cox (1997) argues, "strong" electoral systems, such as single-member, simple-plurality ones, can significantly lower the number of parties, but population diversity can increase the number of parties, leading to results that are not quite in keeping with Duverger's Law, but close. Throughout recent Indian history, small ethnic parties have been competitive in elections and have garnered representation in the Lok Sabha. Ethnicity and caste are important factors in any full explanation of electoral and political outcomes in India, but they do not tell the whole story. To explain why the number of parties and the extent of party aggregation have changed over time within both countries, one must look at the fiscal relationship among levels of government. The central Indian government is less involved in economic decisions that affect voters at the local level than is the national government in the United States. For instance, to use a simple measure of federal intrusion, direct taxes levied by the national government as a proportion of GNP are much lower in India (around .02 in recent decades) than the United States (around .14 in recent decades). The United States has a far better developed welfare state and accompanying national bureaucracy than India. The average Indian has less contact with the central government than the average American. It is important to emphasize that only two countries are compared. A fuller examination of this explanation requires electoral and economic data on more single-member, simple-plurality countries.

DISCUSSION

We have drawn attention to the role played by political and economic centralization in explaining changes in the number of parties in single-member, simple-plurality systems. Like Sartori (1986), who argues that national party systems influence voter and candidate behavior in local districts, we consider national policies to be influential in the decision of local candidates to affiliate with other candidates and in the decision of voters to support local versus national parties. Taking Sartori one step farther, we attribute the emergence of national party systems that resemble local party systems to the increasing importance of the central government in the lives of voters.

Various other explanations can be offered for party aggregation. In the U.S. case, these focus on the unique institutional features of the U.S. system, such as the presidency, a strong committee system in Congress, ballot access laws, and civil service norms. For example, it has been argued that the direct election of the executive discourages the formation of third parties in the United States or makes party aggregation easier in many kinds of systems (Cox 1997; Jones 1994; McCormick 1975; Shugart and Carey 1992). This argument, while plausible theoretically and not at all inconsistent with our own, does not account for changes over time in the United States. American party aggregation occurred only in the 1930s, although presidential elections began in 1788. Furthermore, focusing on U.S. presidential elections to measure the number of parties is misleading. Direct Duvergerian effects can indeed occur in a national election for a single office, but the legislative data reveal quite clearly that three, four, and even more parties have received a substantial proportion of votes during certain periods.

The committee system and rules of the lower house also have been suggested as discriminating against third parties. One could argue that because the committee system in the Lok Sabha is much weaker than in the House of Representatives, there are more parties in India. Greater floor action in the legislature could mean less effective coordination among legislators on agenda control and voting. While the committee systems are indeed different in the two countries, this alone cannot explain the variance in the number of parties receiving votes in legislative elections before the 1930s in the United States. In the late 1800s, members of the Populist Party often complained that they were not given the chance to speak and that the two major parties essentially divided the House and the committee assignments between themselves. Yet, this did not prevent either the emergence of third parties or the expression of voter preferences for parties that were locally competitive but nationally noncompetitive.

Ballot access rules, too, can prevent third parties from becoming competitive by raising the cost of running a campaign. We found no systematic evidence that ballot access rules changed so dramatically in the United States (non-South) in the 1930s that they caused the number of national parties to drop as it did.

The evolution of the U.S. party system has been linked directly by Shefter (1994) and indirectly by Skocpol (1995) to civil service reform. While these authors do not explicitly address any influence that the emergence of a "national civil service" may have had on the number of national parties, their explanation suggests that a national bureaucracy and a two-party system go hand in hand. Civil service reform may lead to a nationalization of issues.[25] Our argument is similar, but we believe a national civil service is not a sufficient condition for the emergence of national politics. The Indian bureaucracy has always been "national," yet the issues that usually dominate Indian elections and party politics are not national. It is not the existence of a national civil service but the functions it performs that determine whether national concerns will dominate local politics. In the United States, we argue, the emergence of an income tax and the welfare state were crucial to the nationalization of politics.

We end with four observations. First, political and

[25] Coleman (1996) makes a similar observation.

economic centralization can occur for many reasons, including a consolidation of local parties into national parties.[26] In other words, we do not deny the possibility that party systems cause centralization, not the other way around. Yet, the data from India and the United States show that centralization begins prior to periods of party aggregation. The patterns of low levels of N_N and centralization follow each other closely in each country. Both Franklin Roosevelt and Indira Gandhi used their position to, among other things, create more nationalized political parties (Milkis 1993; Rudolph and Rudolph 1987).

Second, while we focus on national party systems in India and the United States, the logic for party aggregation is independent of the level of government. In federal systems, these effects may occur at the state level as well.

Third, it is certainly possible, if not likely, that these effects also may occur in proportional representation systems. Centralization could depress the number of national parties in such systems for much the same reason: Local and regional parties cannot stay competitive in the national arena when voters focus attention on national policies.

Fourth, our argument is consistent with recent trends. The central government in India is ceding control over fiscal policies to the states, and the number of parties at the national level is increasing. This is exactly as we would predict. The problem of party aggregation continues in India. In the United States, the national two-party system appears to be quite stable. If recent trends in the devolution of fiscal authority to the states continue in the United States, perhaps we shall see more regionally competitive but nationally noncompetitive parties represented in Congress, and difficulties with party aggregation may reemerge.

APPENDIX

The district and national measures for the effective number of parties in the United States were calculated using historical election data prepared by the Inter-university Consortium for Political and Social Research (1995). Nongeneral elections, contests with missing vote totals, and contests with more than one winner were dropped from the data set. In calculating the district-level measures, for each election the candidate's proportion of the vote, p_i, was calculated by dividing the candidate's vote total by the total votes for all candidates in the election. We use the formula given on page 331 for the effective number of parties (N) (Lijphart 1994, 68).

There are two other measures used in the literature: NP (see Molinar 1991) and I (see Kesselman 1966). NP puts very heavy weight on the proportion of votes for the winning party. I puts very heavy weight (relative to other measures) on smaller parties receiving low proportions of the vote. We calculated all three measures for the U.S. and Indian data. Since the fluctuations in the national data for both countries are similar across the three measures, our main conclusions are not sensitive to changes in measurement. The district measures are somewhat different for the three measures, but N nearly always lies between the two other measures and strikes a nice middle ground conceptually. Also, N is used most often in the recent literature, so its use here makes it easier to compare our results with others.

In each case for the district elections, votes were summed over all candidates. Yearly averages were calculated using sums of these measures and a count of the total number of elections nationwide (in the data set) for each year. The "non-South" measures—used in all U.S. data reported in this paper, both for district-level and national-level analysis—exclude elections held in Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. Obviously, including the South would lower D_N in the early twentieth century (though only to about 1.7). For much of American history, southern elections were held in circumstances so different from the remainder of the country that including the South in these measures would introduce many empirical and theoretical complications.

In calculating the national-level measure, to allow aggregation of votes by party, if a candidate's party was coded as either missing or unidentified, and the candidate received less than 2% of the vote, then that candidate (but not necessarily the others in the contest) was dropped from the data set. If a candidate's party was coded as either missing or unidentified, and the candidate received 2% or more of the vote, then every candidate in the election (i.e., the entire contest) was dropped from the national measure. This presents virtually no problems (few cases are dropped from any year), except for 1806. For some reason the data on the party affiliation of candidates for that year are extremely thin. All candidates except those of the (Old) Republican Party in Vermont had missing party data (for example, 266 candidates in New York had none). Thus, N_N for that year registers 1.0 (see Figure 1). This is the only year for which missing data on party affiliation is a major problem in either data set.

The source for U.S. economic data was the U.S. Department of Commerce (1975). In that volume, the figures were found in the following locations: total government outlays (Series Y457), defense outlays (Series Y458-Y460), state and local expenditures (Series Y671), and GNP (Series Y590-Y598).

The constituency-level and national measures of the effective number of parties in India were calculated using Indian election data prepared by Singh and Bose (1984, 1994) and India (1996a). The calculations used data for single-member constituencies in 16 states: Andhra Pradesh, Assam, Bihar, Gujarat, Haryana, Himachal Pradesh Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamilnadu, Uttar Pradesh, and West Bengal. These account for more than 95% of the country. For each constituency, N was calculated using each party's proportion of the vote total provided in the data set. Yearly averages were calculated using sums of these measures and a count of the total number of elections per year. The national-level measure was derived by calculating each party's proportion of the total number of votes for all parties in each election year, and then applying the formula for N.

Our results for India differ from those of Lijphart (1994) for two reasons. First, our data are not for the national level (as in Lijphart) but for the constituency level. To get the national measures, we aggregated across all districts. Our cut-off was 2% of total votes in each constituency; that is, candidates receiving less than 2% in any constituency were dropped from the analysis. This could be a potential problem, especially if the proportion of votes received by minor parties in any given district aggregates to, say, 10% or more. In India, this 2% cut-off led to dropping only 1.78% of the total vote

[26] See Skowronek (1982) and Wagner (1958) for causes of fiscal centralization.

for the average district (this varied little across election years) with a range of 0% to 30% for all districts and a standard deviation of 2.18%. Because the proportion of votes dropped was so low, neither the district calculations (D_N) nor the national calculations (N_N) were affected.

Our second and most consequential difference with Lijphart has to do with our treatment of independents. In Indian politics there are many independent candidates. In each electoral district (constituency), they were included in the district calculations (D_N) if they surpassed 2% of the vote in that district. In calculating N_N, each independent was included as a separate party. Lijphart, in contrast, includes them in his "Other Parties" category but leaves them out of the numerator, appropriately. Yet, he includes the number of independent votes in the denominator in calculating the proportion for each party. Taagepera (1997) suggests several procedures for calculating N when there is incomplete data on which parties are included in the "Other Parties" category. Here, we have complete data, and these allow for the most accurate measure available for the effective number of parties (by including each independent as a single party).

The economic data on GNP and direct taxes levied by the central government were collected from India (1992, 1996c). Chandok (1990) provides data on central and state expenditures from 1960 through 1988, and the series was completed with India (1992 and 1996c).

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On the Legitimacy of National High Courts

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The purpose of this research is to examine theories of diffuse support and institutional legitimacy by testing hypotheses about the interrelationships among the salience of courts, satisfaction with court outputs, and diffuse support for national high courts. Like our predecessors, we are constrained by essentially cross-sectional data; unlike them, we analyze mass attitudes toward high courts in eighteen countries. Because our sample includes many countries with newly formed high courts, our cross-sectional data support several longitudinal inferences, using the age of the judicial institution as an independent variable. We discover that the U.S. Supreme Court is not unique in the esteem in which it is held and, like other courts, it profits from a tendency of people to credit it for pleasing decisions but not to penalize it for displeasing ones. Generally, older courts more successfully link specific and diffuse support, most likely due to satisfying successive, nonoverlapping constituencies.

Perhaps the most neglected subfield within comparative politics is law and courts. Despite impressive progress in understanding many aspects of cross-national politics, comparativists know precious little about the judicial and legal systems in countries outside the United States.[1] We understand little or nothing about the degree to which various judiciaries are politicized; how judges make decisions; how, whether, and to what extent those decisions are implemented; how ordinary citizens influence courts, if at all; or what effect courts have on institutions and cultures. The degree to which the field of comparative politics has ignored courts and law is as remarkable as it is regrettable.

Our ignorance is especially unfortunate since one of the most significant developments in comparative politics is the growing influence of judicial institutions in national and international politics. What Tate and Vallinder (1995, 5) call "the judicialization of politics" seems to be a truly global phenomenon and "may be or may become one of the most significant trends in late-twentieth and early-twenty-first-century government." Though the United States seems to have led the way in transforming political questions into legal questions, much the same is true in polities as diverse as Russia (Kitchin 1995), Namibia (Steytler 1995), the Philippines (Tate 1995), Canada (Epp 1996), the European Union (Weiler 1994), and Spain (Guarneri and Magalhaes n.d.). Nor does this trend toward greater judicialization show signs of abating soon.

As courts move more squarely into the political limelight, a host of new questions emerges. Among them is how these institutions relate to their constituents, especially their mass publics. Not even the most powerful courts in the world have the power of the "purse" or "sword"; with limited institutional resources, courts are therefore uncommonly dependent upon the goodwill of their constituents for both support and compliance. Indeed, since judges often make decisions contrary to the preferences of political majorities, courts, more than other political institutions, require a deep reservoir of goodwill. Without institutional legitimacy, courts find it difficult to serve as effective and consequential partners in governance.

One would find no quarrel from social scientists in

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We are indebted to the (U.S.) National Science Foundation (NSF) (SBR-9213905, SBR-9311397, SBR-9213201, and SBR-9312689) and the (German) von Humboldt Stiftung for major support for a portion of this project. We also acknowledge the assistance of the Limited-Grant-in-Aid Program at the University of Houston and the Department of Political Science, College of Social and Behavioral Sciences, and Office of Research at Ohio State University. For technical assistance we are indebted to Dominique Vancraeynest, Director, INRA, and to Anna Melich and Eric Marlier, both of the Eurobarometer. Without the extraordinary support and collaboration of Karlheinz Reif, Director, Eurobarometer, we could not have brought this project to fruition. None of these people nor any agency bears any responsibility for the results or interpretations.

The paper also uses data from a collaborative project that grew out of the Law and Society Association's "Working Group on Orientations toward Law and Normative Ordering." Ellen S. Cohn, James L. Gibson, Susan O. White, Joseph Sanders, Joan McCord, and Felice Levine were responsible for the development and implementation of the research design. Funding for the project was provided by the NSF (SES-9213237 and SBR-9311403) and the National Council for Soviet and East European Research (#810-07). Our European collaborators include Chantal Kourilsky-Augeven (France); Grazyna Skapska, Iwona Jakubowska-Branicka, and Maria Borucka-Arctowa (Poland); Andras Sajo (Hungary); Rosemary Barbaret (Spain); and Stefka Naoumova (Bulgaria). Gennady Denisovsky, Polina Kozyreva, and Mikhail Matskovsky of the Institute of Sociology, Russian Academy of Sciences, were instrumental in conducting the Russian portion of the research. Another part of the paper relies on data collected with support from the NSF (SES-9023565), the Advanced Research Program (003652-164), the College of Social Sciences and the Limited-Grant-in-Aid Program at the University of Houston, and the USSR Academy of Sciences. Without the collaboration of our Russian colleagues, this research would not have been possible.

A number of people provided most helpful comments on earlier drafts, including Larry Baum, Erhard Blankenburg, Paul Brace, Charles Epp, John Hibbing, Bert Kritzer, Melinda Hall, Al Melone, Gad Barzilai, Marie Provine, and Jim Stimson. Of course, we assume complete responsibility for all interpretations and conclusions; none of these colleagues or agencies necessarily endorses our findings.

[1] For concurring assessments see Melone (1996, 231), Volcansek (1992), Shapiro (1980, 1993), and Jacob et al. (1996).

the United States with the foregoing sentiments. American political scientists have long evidenced concern about the mass legitimacy of judicial institutions, and a considerable volume of empirical work on mass attitudes toward U.S. courts has been reported (see Caldeira 1991 for a recent review of the literature). Outside the United States, however, social scientists have devoted little attention to how ordinary people perceive and evaluate courts (but see Barzilai, Yuchtman-Yaar, and Segal 1994). Scholars often assume, for instance, that the U.S. Supreme Court is one of the most revered (and unusual) courts in the world, but no rigorous empirical evidence exists to support that claim. Researchers commonly observe in the United States that those who are more knowledgeable about law and courts are more supportive of judicial institutions (e.g., Caldeira and Gibson 1992). But is this a universal relationship; is it invariably the case that to know courts is to love them; or is it simply that American courts are particularly lovable? How would the public have evaluated the Supreme Court during the early years of the Republic, when Jeffersonians battled the institution? Would Germans who knew much about the Nazi courts have loved them as well? And what of Germany today—do knowledgeable Germans love the Bundesverfassungsgericht? We have little idea whether the flock of important hypotheses generated by the American case has any generalizability whatsoever. This is a lacunae of significant proportions for judicial process scholars.

Moreover, the most important question in legitimacy research—how institutions acquire and sustain legitimacy—has never been investigated in cross-national judicial research. Consequently, we know little about the processes through which the accumulation of satisfaction with institutional outputs gives rise to more obdurate support. Nor do we have solid theories or persuasive empirical tests of the processes that erode institutional legitimacy. Contemporary cross-sectional analysis confined to one of the most powerful and legitimate courts in the world can tell us little about the processes leading to the acquisition, maintenance, and erosion of institutional legitimacy.

Accordingly, our goal is to broaden the study of the relationships between courts and their publics by examining mass attitudes toward the legitimacy of national high courts, mainly in Europe, East and West. Based on twenty surveys we conducted in eighteen countries in the period 1993 through 1996, we first assess the salience of these institutions to their mass publics. We then investigate the degree of support—diffuse and specific—these courts enjoy. Next, we test the conventional (U.S.) hypotheses that greater awareness of institutions increases support for them and that specific and diffuse support are connected, but not too strongly. Finally, unlike earlier researchers, we reexamine these relationships at the macro level in an effort to understand the longitudinal processes that create institutional legitimacy. Though ours is not a longitudinal study, we take advantage of a cross-section of courts that ranges from the newly minted institutions of Central and Eastern Europe (including Russia) to the long-established courts of Great Britain, Belgium, Denmark, the Netherlands, and the United States. Given the great variability in the age of these institutions, our cross-sectional analysis can yield insights into the dynamic processes leading to the creation of institutional legitimacy. We begin by considering theories of institutional legitimacy and prior empirical findings on the origins of attitudes toward judicial institutions.

THE LEGITIMACY OF JUDICIAL INSTITUTIONS

The theory of institutional support first outlined by Easton (1965) and later pursued by a small legion of judicial scholars is now so well known that a great deal of exposition is not required. Although the literature is too diverse to sustain the extraction of rigorous summary propositions,[2] a few conclusions emerge.

First, support for the U.S. Supreme Court is fairly widespread (Caldeira and Gibson 1992; Gibson and Baird 1997). To be sure, public esteem oscillates a bit over time (e.g., Caldeira 1986; but see Mondak and Smithey 1997), but most Americans judge their Supreme Court to be a largely legitimate institution.

Second, although support is relatively durable, it does change in the long term, most likely in response to change in the policy outputs of the Court (Caldeira 1986). We find the clearest support for this proposition in the evolution of the opinions of African Americans toward the Supreme Court (Gibson and Caldeira 1992). After 1968, as the Court resumed its customary role as a quite conservative policymaking institution, it appears that blacks became aware of this change, and their support for the institution—even their diffuse support—diminished over time.

Third, and following from the last point, diffuse and specific support are not entirely disconnected (e.g., Adamany and Grossman 1983), at least when viewed from a longitudinal perspective. If diffuse support cushions the effect of unpopular decisions (Gibson 1989; Tyler 1990), then diffuse support can be high when specific support (approval of policy outputs) is low. Diffuse support may also be high when specific support is high, but this is not necessarily so among newly created institutions (i.e., because it has not had time to accumulate, diffuse support can be low even though specific support is high). And, of course, both specific and diffuse support can be low. When the relationship between these two forms of support is examined cross-sectionally, it is difficult to predict for any given institution whether they will be closely connected.[3]

Fourth, generally speaking, to know something about courts is to be favorably oriented toward them. This is, we think, one of the most interesting proposi-

[2] This diversity refers not only to research design (e.g., whether students or representative samples of ordinary citizens are being studied) but also to such crucial issues as the ways in which key concepts (e.g., diffuse support) are operationalized.

[3] For instance, for the two concepts in the United States in 1980, Lau and Redlawsk (1996) report a correlation of .52; Caldeira and Gibson (1992), using 1987 data, report a correlation of .05; and Lau and Redlawsk (1996) claim a correlation of .34 in 1992.

tions in the literature. In some of the earliest research on this question, Casey (1974, 409) found that respondents more attentive to courts were more, not less, likely to subscribe to the mythology of judicial neutrality and objectivity in decision making (see also Murphy and Tanenhaus 1968; Murphy, Tanenhaus, and Kastner 1973). It may be that ordinary people who know little about courts have few reasons to believe that judges make decisions differently from any other politicians. Those attentive to courts come to adopt a different view, but not the view of legal realists. Greater awareness is associated with the perception that judges are different, that they rely on law not values in making decisions, that they are "objective." Greater awareness of the institution thus creates a less realistic view of the nature of judging, a view that contributes mightily to the legitimacy of the courts (Tyler and Mitchell 1994).

How might citizens acquire this picture of judging? Here we can draw on recent scholarship about the effect of "framing" on political and social attitudes. Framing "is the process by which a . . . source . . . defines and constructs a political issue or public controversy" (Nelson, Clawson, and Oxley 1997, 567). By framing an issue in a certain fashion, an actor defines its causes and consequences and sets out the criteria for evaluating the correctness of the solution (Nelson and Kinder 1996). Perhaps citizens adopt an uncritical and unrealistic view because it is the view that judges themselves promulgate. Courts and judges in civil law nations seek to appear to render formalistic opinions, relying heavily on symbols. Judges in the United States take great pains, at least in constitutional law, to frame decision making as a process of deducing outcomes from first principles—original intent, literal words, legal precedent, and so on. Consider Justice Scalia's statement in a recent opinion: "To hold a governmental act to be unconstitutional is not to announce that we forbid it, but that the *Constitution* forbids it" (*American Trucking Association* v. *Smith*, 110 LED. 2d 148, 1990, at 174, emphasis in original). When ordinary people hear judges of the nation's highest court frame their decisions in this fashion, they often believe the justices' account of why they made the decision (Hoekstra and Segal 1996). Simply put, to know courts is to love them, because to know them is to be exposed to a series of legitimizing messages focused on the symbols of justice, judicial objectivity, and impartiality.

Finally, the slow accretion of positive messages about courts and law leads to legitimacy. Though only a single longitudinal study has been conducted at the micro level (Tanenhaus and Murphy 1981), scholars nonetheless agree that diffuse support is an attitude that evolves over time. Preadult socialization most likely results in positive predispositions toward courts and the legal system, since law is typically presented as a benevolent rather than malevolent institution (Caldeira 1977; Easton 1965; Easton and Dennis 1969). The "crystallization" of approving attitudes toward institutions probably then results from a continuous stream of reinforcing stimuli (Jennings 1989). As citizens move from adolescence to adulthood, attitudes acquire "affective mass," largely through learning and habituation (Jennings 1989, 315). The acquisition of experience, reinforcement, and so on, creates relative attitude stability among adults. Lodge and Taber (1997) refer to this as the accumulation of a "running tally," an apt term to describe the cumulative nature of diffuse support (see also Hibbing and Theiss-Morse 1995, 13).

This model most likely describes the process through which West Germans acquired deep attachments to democratic institutions following World War II (e.g., Boynton and Loewenberg 1973). In the immediate post-war period, democratic loyalties were assuredly anemic and, at best, confined to relatively abstract attitudes. The process of crystallization had not yet occurred; nor had a "running tally" accumulated in the minds of the German people. But as the political system succeeded repeatedly over time, commitment to democratic government became more deep-seated. After about a generation of effectiveness, the political system could afford to fail periodically, since it had acquired a reservoir of goodwill. Had the system failed to produce in the late 1940s or early 1950s, it is unlikely that the Germans would have been so forgiving of the fledgling democratic regime. Thus, in the scheme of long-term change, the development of diffuse support for an institution requires a long and relatively consistent stream of satisfactory institutional outputs (see also Giles and Lancaster 1989).

Unfortunately, thinking of diffuse support in this way renders it resistant to empirical investigation. Not only would a lengthy time series at the micro level be necessary, but also the optimal series would have to be concentrated in (or at least include) the period in which the institution is born. This means that cases such as the United States are particularly unrevealing when it comes to analyzing the origins of diffuse support. Scholars have made creative efforts at longitudinal analysis within the United States (e.g., Mondak and Smithey 1997), but data limitations seriously hinder theoretical and empirical progress on this front.[4]

Cross-national analysis is a viable alternative to a longitudinal research design, especially if the sample of nations includes a heterogeneous array of institutions. By examining attitudes toward a variety of institutions—some relatively new, some in existence for only a few generations, and some with clearly established legitimacy—we may be able to derive insights into the longitudinal processes involved in the development of diffuse support. As noted above, the nature of the connection between specific and diffuse support is heavily contingent upon whether people already have established attitudes toward the institution or whether the institution is relatively unknown. Longitudinal analysis is the most effective (but difficult) way of assessing this relationship. Since changes in the world

[4] An important exception is institutions that function for a long period in secrecy or anonymity but then receive public scrutiny and thereby enter the public domain (e.g., the European Court of Justice). For analyses of diffuse support, the point at which the institution becomes salient to the mass public constitutes the point at which the institution is "created."

have created a virtual laboratory for analyzing the evolution of diffuse support, cross-national research can make a noteworthy contribution as well. That is our central purpose here.

The microlevel model we test in this article is fairly simple.

HYPOTHESIS 1: *As knowledge and awareness of a court increases, diffuse support for it is likely to increase as well.*

HYPOTHESIS 2: *As satisfaction with the policy outputs of a court increases, diffuse support for it is likely to increase as well.*

RESEARCH DESIGN

Our data come from two separate but related projects. The first involved surveys in each country of the European Union in conjunction with research on mass attitudes toward the European Court of Justice and the national high courts in the EU member states. The data we employ here are drawn from a 1993 reinterview of the respondents in the 1992 Eurobarometer, the semi-annual mass survey of the Commission of the European Communities. The Eurobarometer surveys are representative of the populations, age 15 years and older, of the respective countries. In 1993 we reinterviewed subsamples of the 1992 Eurobarometer respondents. The reinterviews were by telephone, except in Ireland, Portugal, and East Germany, where telephone penetration was not sufficiently high to ensure representative samples. Since national law made it impossible to reinterview the Danish respondents, a fresh sample was drawn in Denmark. A number of methodological issues concerning the panel are addressed in Gibson and Caldeira 1995, 1998; and Caldeira and Gibson 1995.[5]

The second set of surveys was conducted in 1995 and 1996 in six European countries and the United States in connection with a project on law, legal consciousness, and democratization. Representative samples of the mass public were interviewed in person in Bulgaria, Hungary, Poland, Russia, France, and Spain and by telephone in the United States (see Gibson 1998 for details). France and Spain were included in both projects.[6] The two projects had different purposes, but the items measuring attitudes toward national high courts are identical. Thus, although we must be mindful that the surveys occurred at various times during 1993–95, in all important respects they are comparable.

In some countries, which institution is the high court is not clear. For example, the Constitutional Council in France exercises a form of judicial review but is not a court; French courts cannot exercise judicial review (Provine 1996; Stone 1994). Consequently, we chose the Cour de Cassation as our target because we thought that some French respondents might not accept the designation of the Conseil Constitutionnel as a court. In nine of the countries (including Germany, Portugal, Spain, and Italy), two or more high courts divide the functions performed by the U.S. Supreme Court. For six of those nine nations, we designated the constitutional court as the court of interest; in three (Russia, Hungary, and Poland) we selected the supreme court because it was the more salient at the time the survey was conducted. In all other nations, we opted for the highest court exercising judicial review or, in its absence, the highest court exercising appellate review. Table 1 lists these institutions.

TABLE 1. National High Courts

Country	National High Court	Year of Creation
Belgium	Hof van Cassatie (Cour de Cassation)	1831
Bulgaria	Kohctutyzuoheh Cad	1991
Denmark	Højesteret	1753
France	Cour de Cassation	1790
Germany (East)	Bundesverfassungsgericht	1990
Germany (West)	Bundesverfassungsgericht	1949
Greece	Anótato Eidikó Dikastério	1975
Hungary	Alkotmánybíróság	1990
Ireland	Supreme Court of Ireland (Chúirt Uachtarach)	1924
Italy	Corte Constituzionale	1956
Luxembourg	Cour Supérieure de Justice	1868
The Netherlands	De Hoge Raad der Nederlanden	1838
Poland	Sad Najwyższy	1984
Portugal	Tribunal Constitucional	1982
Russia	Verkhovniy Sud	1993
Spain	Tribunal Constitucional	1979
United Kingdom	Court of Appeal	1876
United States	Supreme Court	1790

Our sample of courts varies on several dimensions. (1) These judicial institutions range considerably in political importance, from the Supreme Court of the United States and the Bundesverfassungsgericht (Kommers 1989) at one pole, to the Cour Supérieure de Justice of Luxembourg, Belgium's Hof van Cassatie (Verougstraete 1992), and the Cour de Cassation of France (Provine 1996) at the other. (2) The "births" of the courts occurred over more than two centuries, from 1753 in Denmark to 1993 in Russia. Eleven of these institutions were created before World War II, nine after (mean year of founding = 1911, median year = 1952). (3) They also differ in "political age," the age at which they began to make decisions of interest to the political community and especially the mass public. (4) These courts also possess different combinations of formal powers. Some specialize in constitutional review (Spain, Portugal, Italy, Germany, Greece; see Cappel-

[5] This survey was connected to the EU's Eurobarometer, and as such, separate samples for East and West Germany were drawn. Following common practice, we analyze the two samples separately. Although the two portions of Germany were reunified by 1993, when the survey was conducted, the levels of exposure to the Federal Constitutional Court were quite different among East and West Germans. We take advantage of these differences in testing our hypotheses concerning the development of institutional legitimacy.

[6] In the macro-level analysis (below), we weight each of these samples at .5, so as not to "overrepresent" these two countries.

letti 1989); others blend the functions of judicial review of legislation and of lower courts (Ireland, the United States, Denmark, Hungary, Poland, Russia); others exercise only the power to review the decisions of inferior courts (France, Belgium, Luxembourg, United Kingdom).[7] (5) The legal systems in our sample come from several different "legal families" (see David and Brierly 1978)—common law (United States, Great Britain, Ireland), civil law (France, German, Spain, and others), and mixtures of civil and socialist law (Poland, Hungary, Russia).

These institutions vary in political centrality and importance, but all make decisions of interest to ordinary people. We lack the space to provide even a modest sampling of the outputs from these courts (see Landfried 1988), but here are a few examples of these high courts at work.

- In 1996, the Irish Supreme Court accepted an appeal challenging the recent referendum approving a constitutional amendment to permit divorce. Prior to the appeal, the Court had ordered the government to stop spending money to run advertisements in favor of the referendum (Parkin 1996). Both actions had the effect of delaying a majority intent on liberalizing divorce.
- In 1995, the Constitutional Court of Bulgaria struck down a provision of the state's budget as a violation of judicial independence—the sixth time since the previous elections the judges had invalidated a statute. The law placed the Ministry of Justice in charge of funding the Supreme Judicial Council, which appoints judges, prosecutors, and investigators (Melone 1996).
- The Supreme Court of the Russian Federation found in 1996 in favor of Greenpeace and overturned President Yeltsin's decree permitting the importation of nuclear waste into Russia. The government had hoped to use funds earned from the processing of imported nuclear waste to finance the completion of a processing plant in Krasnoyarsk (Grushina 1996).
- The Supreme Court of Poland upheld the presidential election of November 1996 and declared Kwasniewski the winner, despite his violation of electoral law by giving false information about his educational background. The justices unanimously agreed on the violation but disagreed on whether it had made any difference in the election outcome (Karpinski 1995).

In sum, the decisions of high courts in Europe make a difference for both the leaders and the led in society, affecting matters of high policy—such as the waste site in Russia—and the details of everyday life—such as divorce in Ireland.

THE STATISTICAL ANALYSIS

Awareness of National High Courts

We first asked the respondents about their level of awareness of their national high court. Table 2 reports the results.

Although there is considerable cross-national variability, most high courts are reasonably well known. At one extreme lies West Germany, where nearly half the respondents claimed to be very aware of the Bundesverfassungsgericht. Other countries in which awareness is widespread include Great Britain, the United States, Ireland, the former East Germany, Denmark, and perhaps Spain as well. It is interesting to note that awareness of the U.S. Supreme Court is quite high when compared with most other countries, even if awareness is higher (or as high) in a few countries. In Germany, Great Britain, and the United States, more than four-fifths of the respondents claimed at least a moderate degree of consciousness of the highest national court. Countries in which the highest court has very little visibility include Russia (the most extreme case, by far), Bulgaria, Luxembourg, Portugal, and perhaps France as well. We should reiterate, however, that substantial cross-national differences exist in the visibility of the national high court ($\eta = .50$). Strikingly, when we combine the two highest levels of awareness, the percentage fairly aware of the national high court ranges from a low of 17.6 in Russia to a high of 85.9 in West Germany.

Even a cursory examination of Table 2 reveals that awareness is not necessarily related to the age of the institution. With the exception of Russia, awareness of the high court in some of the new democracies is not particularly low. In Hungary, for instance, awareness is about the same as in the Netherlands and Greece; Poland is comparable to Italy; and Bulgaria is similar to Luxembourg. We will investigate this relationship more rigorously below, but it is clear that there is not necessarily a strong relationship between the age of the institution and its salience within the mass public.

Awareness of the national high courts may indicate something of their politicization. After all, these courts become known primarily when they make decisions that attract mass media attention. Rulings on arcane legal issues are unlikely to interest many; decisions on issues of public law—Sunday closing, abortion, gender equality—are much more likely to penetrate public consciousness. To the extent that a court is salient, it probably has made decisions of interest and concern to ordinary people.

For most of the following analysis, we put aside those respondents who are insufficiently aware of their national high court to have meaningful opinions (the first and second columns in Table 2). We do not contend that their views are always unimportant (see Gibson and Caldeira 1995), but for purposes of our analysis it is prudent not to include those who are oblivious to the

[7] European Union law puts this statement about national courts without the power of judicial review in a more complex light. So, for example, although the courts of appeal do not have the power to invalidate a parliamentary law in conflict with the British "constitution," they may do so if the legislation conflicts with European law. Furthermore, as Alivizatos (1995) points out, courts without judicial review may accomplish much the same outcome by interpreting the parliamentary law in conformity with the constitution.

TABLE 2. Awareness of National High Courts

	Level of Awareness[a]							
	Percentage							
Court	Never Heard of	Don't Know	Not Very Aware	Somewhat Aware	Very Aware	Mean	Std. Dev.	*N*
Russia	50.8	2.2	29.4	17.0	.6	.7	.77	772
Bulgaria	13.4	13.7	47.9	22.7	2.0	1.1	.69	1,183
Luxembourg	25.0	2.5	46.0	17.5	9.0	1.1	.89	200
Belgium	9.2	6.0	56.6	23.3	4.8	1.2	.70	249
France (1993)	7.6	.0	69.4	20.3	2.7	1.2	.60	301
France (1995)	9.2	3.7	64.8	18.1	4.2	1.2	.65	759
Portugal	17.7	4.0	45.0	30.3	3.0	1.2	.76	300
Italy	9.0	.7	54.3	31.0	5.0	1.3	.71	300
Poland	11.7	3.0	50.8	27.9	6.6	1.3	.77	821
Greece	6.1	3.5	51.1	32.5	6.8	1.4	.72	311
Hungary	9.0	1.4	48.3	37.7	3.6	1.4	.70	785
Spain (1995)	10.7	3.8	35.8	43.2	6.5	1.4	.79	773
The Netherlands	6.0	.0	55.3	36.0	2.7	1.4	.64	300
Spain (1993)	10.3	3.7	27.3	35.7	23.0	1.7	.95	300
Denmark	1.3	.3	26.7	62.0	9.7	1.8	.62	300
Germany (East)	1.0	1.3	27.5	52.8	17.5	1.9	.71	309
Ireland	1.6	7.8	22.1	44.5	24.0	1.9	.83	321
Great Britain	1.7	.0	11.3	55.3	31.7	2.2	.69	300
United States	.6	.1	16.8	49.8	32.7	2.2	.71	810
Germany (West)	1.5	.5	12.1	38.4	47.5	2.3	.76	198

Note: Countries are rank ordered on the mean level of awareness. In the first five columns, each row totals to 100%, except for rounding errors. The range of the variable "awareness" is from 0 to 3.

[a]The question read: "And what about the [HIGHEST COURT OF THE COUNTRY], that is, one of the [NATIONALITY] courts? Would you say that you are very aware, somewhat aware, not very aware, or have you never heard of the [HIGHEST COURT OF THE COUNTRY]?

institution.[8] Furthermore, with the exception of Russia (on which we comment below), the proportion of the total country sample excluded from the analysis is relatively small and inconsequential. Following earlier research (e.g., Caldeira and Gibson 1992, 1995), we refer to this culled sample as the "attentive public" of the institution.

Specific Support

Table 3 reports the levels of specific support among attentive publics for the national high court, using as a measure overall satisfaction with the performance of the institution. The cross-national differences in awareness of the high courts among their general publics (Table 2) are generally much smaller than the cross-national differences in specific support among attentive publics (Table 3). Among the attentive publics, satisfaction with the performance of the national high court is fairly high. In contrast to awareness, specific support seems to be relatively low in the new democracies of Central and Eastern Europe, even when the respondents who expressed no judgment are excluded. Satisfaction with the U.S. Supreme Court is certainly substantial but pales in comparison to the Danish or Dutch high courts, for instance. Few people are extremely pleased with their national high courts, but few are very displeased. The balance of satisfied to unsatisfied opinion varies by country, with dissatisfaction predominating among the populaces in countries with younger courts, and satisfaction being more common among older courts. The average of the country means is 1.7, which is below the mid-point on the satisfaction scale.

Diffuse Support

Our conceptualization of diffuse support follows prior work in the area; it is "institutional commitment," an "unwillingness to make or accept fundamental changes in the functions of the institution" (Caldeira and Gibson 1992, 638). We have used three items to measure diffuse support for the national high courts, and the responses are reported in Table 4. The propositions put to the respondents closely mirror our conceptualization of diffuse support, with the first item in the table being the clearest and most direct operationalization. If one agrees that it may be better to do away with the court because its decisions displease the majority, then obviously one is not supportive of the institution. Similarly, support is indicated by willingness to defend the court's jurisdiction, as well as by generalized trust in the institution.

There is a great deal of variability across countries in diffuse support of the national high courts. Consider institutional commitment, which among the attentive

[8] The replies of those who claimed little or no awareness are most likely dominated by random variation. Excluding them may restrict somewhat the theoretical variation in the correlation coefficients (although not the empirical variance in the items, since these respondents tended overwhelmingly to give "don't know" responses to our substantive questions), but we believe their exclusion is necessary when analyzing attitudes toward the national high court.

TABLE 3. Specific Support (Satisfaction) with National High Court among Attentive Publics

	Level of Specific Support (Satisfaction)[a]							
	Percentage							
Court	Not at All	Not Very	Don't Know	Somewhat	Very	Mean	Std. Dev.	*N*
Bulgaria	13.9	37.6	25.0	22.8	.7	1.2	.63	856
Russia	8.3	41.1	35.0	15.3	.3	1.3	.52	360
Poland	6.3	36.2	25.7	30.2	1.6	1.4	.58	693
Hungary	5.7	44.6	10.0	37.8	1.4	1.4	.61	699
Germany (East)	6.6	36.4	23.2	31.1	2.6	1.4	.61	302
France (1995)	6.3	19.4	34.9	36.5	2.9	1.5	.59	650
Spain (1995)	5.9	23.2	28.5	38.0	4.4	1.6	.62	656
Italy	4.1	31.0	16.2	45.4	3.3	1.6	.60	271
Spain (1993)	9.7	14.7	24.8	43.4	7.4	1.6	.72	258
Greece	8.9	19.2	15.7	48.0	8.2	1.6	.73	281
Portugal	3.4	12.8	34.5	47.7	1.7	1.7	.50	235
France (1993)	1.1	11.5	45.0	41.0	1.4	1.7	.41	278
Belgium	3.3	16.6	27.5	46.4	6.2	1.7	.58	211
United States	5.5	19.3	5.4	61.3	8.6	1.8	.67	803
Great Britain	3.4	11.9	19.7	55.9	9.2	1.8	.60	295
Ireland	3.8	11.3	24.7	46.7	13.4	1.8	.66	291
Germany (West)	2.1	11.9	11.9	55.7	18.6	2.0	.65	194
The Netherlands	1.1	6.0	12.1	68.4	12.4	2.0	.51	282
Luxembourg	1.4	9.7	21.4	44.1	23.4	2.0	.67	145
Denmark	1.4	3.4	12.2	55.4	27.6	2.2	.61	294

Note: Countries are rank ordered on the mean level of satisfaction. In the first five columns, each row totals to 100%, except for rounding errors. The range of the variable "satisfaction" is from 0 to 3.

[a]In the 1995 survey, the question read: "From what you have heard or read, would you say that you are very satisfied or not satisfied at all with the way the [HIGHEST COURT OF THE COUNTRY] has been working?" In the 1993 survey, the question read: "Now, returning to the [HIGHEST COURT OF THE COUNTRY], would you say that you are very satisfied or not satisfied at all with the way the [HIGHEST COURT OF THE COUNTRY] has been working?"

publics ranges from a low of 18.6% in Bulgaria to a high of 76.0% in the United States. In less than half the countries, a majority of respondents support their national high court. In nine cases—Belgium, Spain (1993), Ireland, Luxembourg, Germany (East), Bulgaria, France (1995), Hungary, and Spain (1995)—there are more opponents of the court than supporters. In most countries, there is even less support in terms of willingness to protect the jurisdiction of the national high court. Yet, trust in the national high court is quite widespread in most countries, with, for instance, an astounding 89.0% of the Dutch expressing some trust in the De Hoge Raad. Apparently, people trust their national high court even if they support reigning it in or even abolishing it should it fail to perform satisfactorily over a long period.

To simplify comparisons across countries, we have created a three-item summated index of support, standardized on a 100-point scale. Table 5 reports the means and standard deviations for each country.[9] According to this index, the Dutch are the most supportive of their national high court; the Spanish (1993) are least supportive; and the Italians and Hungarians score at the middle of the twenty countries. Our data generally indicate that these national high courts vary considerably in the level of legitimacy they have achieved (η = .28). Many have a bedrock of support, but many, perhaps more, do not.

The data in Table 5 provide an enlightening perspective on the U.S. Supreme Court. It is certainly a widely supported institution in the United States, but, in comparative perspective, it is not inordinately well supported. The high courts of the Netherlands, Denmark, Germany (among West Germans), Greece, and even Poland have at least as much institutional legitimacy as the Supreme Court. Though we must be mindful that there are some slight differences in the percentages of the samples that are inattentive to the courts (few of these countries has as broad an attentive public as the United States), the U.S. Supreme Court does not have an exceptionally deep reservoir of goodwill among its mass public.

Connecting Awareness and Diffuse Support

To what degree does increased awareness of the national high court contribute to increased support for the institution? Research from the United States strongly suggests that greater awareness of the Supreme Court directly translates into greater support for it. This is not necessarily because the people who become aware of the Court invariably discover that it is making desirable public policy. Instead, we hypothesize that greater awareness means greater exposure to the legitimizing symbols associated with high courts. Thus, we expect that awareness and support are positively

[9] The three items are moderately intercorrelated. The mean for the 20 countries of the average interitem correlation (within country) is .24 (standard deviation .08), with a minimum of .10 and a maximum of .44.

TABLE 4. Diffuse Support for National High Court among Attentive Publics

	Level of Diffuse Support for the National High Court					
	Percentage					
	Not Supportive	Undecided	Supportive	Mean	Std. Dev.	*N*
Court Jurisdiction[a]						
Belgium	46.9	26.1	27.0	2.8	1.16	211
Bulgaria	31.0	37.5	31.5	3.1	1.15	855
Denmark	25.2	27.9	46.9	3.4	1.26	294
France (1993)	46.4	28.1	25.5	2.8	1.14	278
France (1995)	30.6	39.7	29.7	3.0	1.12	657
Germany (East)	32.0	32.7	35.3	3.1	1.13	300
Germany (West)	21.6	20.1	58.2	3.6	1.18	194
Great Britain	35.6	24.1	40.3	3.1	1.14	295
Greece	24.6	27.4	48.0	3.3	1.06	281
Hungary	25.9	28.6	45.5	3.3	1.15	618
Ireland	34.7	37.5	27.8	2.9	1.07	291
Italy	40.2	20.7	39.1	3.1	1.26	271
Luxembourg	28.3	30.3	41.4	3.2	1.30	145
Poland	32.0	21.6	46.4	3.2	1.26	696
Portugal	25.1	34.0	40.9	3.3	1.11	235
Russia	26.9	39.7	33.3	3.1	.90	360
Spain (1993)	57.0	17.4	25.6	2.6	1.33	258
Spain (1995)	30.9	33.6	35.6	3.1	.97	658
The Netherlands	33.3	12.4	54.3	3.4	1.37	282
United States	35.5	11.7	52.8	3.2	1.12	803
Institutional Commitment[b]						
Belgium	46.9	13.3	39.8	2.9	1.44	211
Bulgaria	57.7	23.7	18.6	2.4	1.22	861
Denmark	23.7	10.8	65.4	3.7	1.33	295
France (1993)	39.2	9.7	51.1	3.2	1.46	278
France (1995)	39.0	26.4	34.6	3.0	1.29	659
Germany (East)	53.5	16.3	30.2	2.6	1.36	301
Germany (West)	34.5	12.4	53.1	3.3	1.40	194
Great Britain	36.6	7.5	55.9	3.3	1.31	295
Greece	23.5	13.9	62.6	3.7	1.37	281
Hungary	43.6	23.7	32.8	2.8	1.30	659
Ireland	46.4	21.0	32.6	2.9	1.27	291
Italy	35.1	11.8	53.1	3.4	1.39	271
Luxembourg	52.4	10.3	37.2	2.7	1.50	145
Poland	23.4	23.4	53.2	3.4	1.29	697
Portugal	39.6	14.9	45.5	3.2	1.45	235
Russia	34.4	25.0	40.6	3.0	1.04	360
Spain (1993)	61.2	8.1	30.6	2.5	1.45	258
Spain (1995)	38.0	27.7	34.3	3.0	1.11	658
The Netherlands	29.1	7.8	63.1	3.6	1.43	282
United States	16.8	7.2	76.0	3.8	1.04	803

correlated. The data relevant to this hypothesis are given in Table 6.

Table 6 shows widely varying relationships between awareness of the national high court and support for it. The connection is strongest in Belgium (b = 12.0), where predicted diffuse support varies from 47.5% for the least aware to 71.5% for the most aware (data not shown). The relationship is also quite strong in the Netherlands and France (1993). In Luxembourg and Bulgaria, the coefficient is slightly negative (although not statistically significant), and awareness and support are completely unrelated in Russia. The average of the unstandardized regression coefficients is 5.5 (standard deviation = 4.1). To know a court is not necessarily to love it, but generally there is a fairly strong tendency in most countries for the more aware to be more supportive of their national high court.

The intercepts reported in Table 6 are also interesting since they represent the expected level of diffuse support when there is little awareness of the national high court. In France (1995), support was low among those without much awareness of the Cour de Cassation, but support was a great deal higher among the uninformed Dutch. Perhaps we can interpret these intercepts as the effect of preadulthood socialization—the level of support for the courts that develops before exposure to any information about the institution. In nearly all the countries, increased awareness results in greater diffuse support for the high court.

TABLE 4. (Continued)

	Level of Diffuse Support for the National High Court					
	Percentage					
	Not Supportive	Undecided	Supportive	Mean	Std. Dev.	*N*
Trust[c]						
Belgium	16.1	22.3	61.6	3.6	1.03	211
Bulgaria	14.1	42.5	43.4	3.4	1.02	861
Denmark	9.2	21.4	69.4	3.9	.94	294
France (1993)	13.3	23.7	62.9	3.6	.95	278
France (1995)	10.6	32.2	57.1	3.6	.91	658
Germany (East)	21.6	32.2	46.2	3.3	1.03	301
Germany (West)	7.7	16.5	75.8	4.0	.92	194
Great Britain	17.6	16.6	65.8	3.6	1.03	295
Greece	10.3	18.1	71.5	3.9	1.04	281
Hungary	10.3	21.6	68.1	3.8	.99	652
Ireland	9.6	23.4	67.0	3.8	.90	291
Italy	25.5	8.9	65.7	3.5	1.19	271
Luxembourg	6.9	11.7	81.4	4.1	.93	145
Poland	9.6	22.2	68.2	3.8	.97	698
Portugal	7.2	25.5	67.2	3.9	.98	235
Russia	5.8	27.2	66.9	3.7	.74	360
Spain (1993)	19.0	24.0	57.0	3.5	1.13	258
Spain (1995)	14.8	33.7	51.5	3.4	.89	656
The Netherlands	4.6	6.4	89.0	4.3	.82	282
United States	25.1	9.6	65.3	3.4	1.03	804

Note: In the first three columns each row totals to 100%, except for rounding errors. The data were collected with a Likert scale, ranging from strongly agree to strongly disagree. For the percentage distributions in this table, strong and moderate responses are collapsed into "Supportive" or "Not Supportive." All items are scored so that a high number equals a supportive response. The nature of the supportive response is shown below in parentheses for each item.

[a]Court Jurisdiction: The right of the [HIGHEST COURT OF THE COUNTRY] to decide certain types of controversial issues should be reduced. (Disagree)

[b]Institutional Commitment: If the [HIGHEST COURT OF THE COUNTRY] started making a lot of decisions that most people disagreed with, it might be better to do away with the [HIGHEST COURT OF THE COUNTRY] altogether. (Disagree)

[c]Trust: The [HIGHEST COURT OF THE COUNTRY] can usually be trusted to make decisions that are right for the country as a whole. (Agree)

Connecting Specific and Diffuse Support

Table 7 reports the regression coefficients connecting specific and diffuse support. As we expected, the coefficients are generally positive—those more satisfied with the current performance of the institution are more likely to extend diffuse support to it—but, also as expected, the relationships differ considerably in strength. In Greece, West Germany, the Netherlands, and the former East Germany, diffuse support closely mirrors specific support. In Russia, the two types of support are unconnected, and in Luxembourg and Spain (1995), they are not strongly related. The average unstandardized coefficient is 11.5, producing a range of nearly 46 points on the 100-point diffuse support scale from the lowest to highest level of specific support. At least at the bivariate level, awareness of the high court is not nearly as strong a predictor of diffuse support as is specific support.

With cross-sectional data, it is not entirely clear how to understand the relationship between diffuse and specific support. Consider the case of the United States, where those who are satisfied with the outputs of the Supreme Court generally support the institution. But this does not necessarily mean that support would fall if satisfaction were to decline. Indeed, according to the theory of diffuse support, only prolonged dissatisfaction would erode levels of dif-

TABLE 5. Average Diffuse Support for National High Court among Attentive Publics

	Index of Support		
	Mean	Std. Dev.	*N*
Spain (1993)	46.3	22.3	258
Bulgaria	48.8	18.4	860
Germany (East)	49.4	22.4	301
Belgium	52.2	22.0	211
Spain (1995)	53.9	17.2	658
Ireland	54.5	18.3	291
France (1995)	55.0	19.6	660
France (1993)	55.2	20.8	278
Russia	56.6	14.4	360
Hungary	57.1	20.5	654
Italy	57.8	23.3	271
Great Britain	58.0	20.2	295
Luxembourg	58.8	21.5	145
Portugal	61.6	22.0	235
United States	62.2	19.7	804
Poland	62.5	20.5	696
Greece	65.0	23.0	281
Germany (West)	65.4	20.4	194
Denmark	66.6	20.3	295
The Netherlands	69.9	22.4	282

Note: The 100-point summated index is created from responses to the items reported in Table 4. The index is the mean of the responses to the three diffuse support items, minus 1 (to set the bottom of the scale at zero) * 25. The countries are rank ordered on the degree of support for the national high court (lowest to highest).

TABLE 6. Regression of Diffuse Support on Awareness among Attentive Publics

	Intercept	*b*	(s.e.)	Beta	*N*
Luxembourg	64.3	−3.7	(2.5)	−.12	145
Bulgaria	50.6	−1.3	(1.2)	−.04	860
Russia	55.2	1.0	(1.5)	.04	360
Ireland	48.8	2.9	(1.5)	.11*	291
Poland	57.4	3.5	(1.2)	.11**	696
Germany (East)	42.5	3.6	(1.9)	.11*	301
Hungary	51.4	3.8	(1.4)	.11**	654
Spain (1995)	42.3	4.0	(1.1)	.14***	658
United States	52.4	4.5	(1.0)	.16***	804
Greece	57.4	5.1	(2.2)	.14**	281
Denmark	56.2	5.7	(2.0)	.16**	295
Portugal	52.8	6.0	(2.5)	.16**	235
Germany (West)	47.6	7.5	(2.1)	.26***	194
Spain (1993)	31.4	7.7	(1.8)	.26***	258
Italy	45.6	8.4	(2.3)	.22***	271
Great Britain	39.3	8.5	(1.8)	.27***	295
France (1995)	43.9	8.5	(1.3)	.24***	660
France (1993)	41.3	10.9	(2.4)	.27***	278
The Netherlands	53.6	11.4	(2.3)	.28***	282
Belgium	35.5	12.0	(2.4)	.33***	211

Note: The countries are rank ordered on the magnitude of *b*, the unstandardized regression coefficient. Beta is the standardized regression coefficient; s.e. is the standard error of the unstandardized regression coefficient. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

TABLE 7. Regression of Diffuse Support on Specific Support among Attentive Publics

	Intercept	*b*	(s.e.)	Beta	*N*
Russia	55.7	.7	(1.5)	.03	358
Luxembourg	49.0	4.9	(2.7)	.15*	145
Spain (1995)	42.7	7.3	(1.1)	.26***	653
Bulgaria	37.9	8.9	(1.0)	.30***	854
Ireland	37.6	9.3	(1.6)	.33***	291
Spain (1993)	30.0	10.2	(1.8)	.33***	258
France (1993)	37.9	10.4	(3.0)	.20***	278
Belgium	34.2	10.6	(2.5)	.28***	211
Poland	47.3	10.9	(1.3)	.31***	690
Hungary	40.8	11.5	(1.2)	.34***	652
France (1995)	37.0	11.8	(1.2)	.35***	649
Portugal	40.8	12.6	(2.8)	.28***	235
Denmark	39.1	12.7	(1.8)	.38***	294
Great Britain	34.7	12.9	(1.8)	.38***	295
United States	38.4	13.6	(.9)	.46***	803
Italy	36.3	13.8	(2.2)	.35***	271
The Netherlands	38.2	16.0	(2.5)	.36***	282
Germany (East)	26.1	16.5	(1.9)	.45***	301
Germany (West)	32.3	16.8	(1.9)	.53***	194
Greece	36.3	17.6	(1.6)	.56***	281

Note: The countries are rank ordered on the magnitude of *b*, the unstandardized regression coefficient. Beta is the standardized regression coefficient; s.e. is the standard error of the unstandardized regression coefficient. $^{*}p < .05$, $^{***}p < .001$.

fuse support. At any given time, diffuse and specific support may be positively related, negatively related, or even unrelated.[10]

The intercepts reported in Table 7 represent the levels of diffuse support when dissatisfaction with the outputs of the institution is total. In some respects, this reflects the hardest core of support for the institution. Among East Germans with no satisfaction with the policy outputs of the high court, there is very little diffuse support. Contrariwise, there is a great deal of support even in the absence of satisfaction in Russia, Poland, Luxembourg, and Spain (1995). The U.S. Supreme Court has a fairly large core of support: Roughly two out of five Americans who are completely unhappy with the Court's policy outputs nonetheless express support for the institution.

The multivariate results, reported in Table 8, are virtually identical to the bivariate results in terms of the relationship between diffuse support and satisfaction. Comparing Table 8 with Table 7, we observe that in none of the cases in which there is a significant relationship between diffuse support and satisfaction did the significance of the relationship attenuate. Therefore, satisfaction maintains an independent effect on diffuse support, even when controlling for awareness. Yet, among the seventeen countries that exhibited a significant bivariate relationship between diffuse support and awareness, in only five (Spain 1993, France 1993 and 1995, Belgium, and Great Britain) does the effect of awareness remain significant when controlling for satisfaction. Thus, we can conclude that awareness is correlated with diffuse support but is generally mediated by the effect of satisfaction, which has the primary independent effect. People become satisfied with the court's policy outputs by being aware of its policies. This lends support to the positivity bias hypothesis, which is well known in the political psychology literature.

[10] Due to selective perception and cognitive dissonance reduction, those who support an institution are more likely to perceive its policy outputs favorably. Thus, in general, we hypothesize some degree of positive correlation between diffuse and specific support. Perhaps the most worrisome relationship is a very strong correlation between the two forms of support, since this may mean that the measures are not capturing the difference between the two concepts. Still, a highly legitimate court whose policy outputs please most of its constituents could produce a reasonably strong correlation between the two attitudes.

Summary

We have discovered considerable cross-national variability in mass attitudes toward national high courts. These institutions are in general relatively salient, and most mass publics are satisfied with the outputs of the institutions. Moreover, awareness of the high court generally creates diffuse support, and specific support leads to diffuse support as well. These data are not dispositive on the processes leading to the cross-sectional results, but they are entirely compatible with our understanding of how institutional legitimacy becomes established. The macro analysis will shed some light on the dynamic nature of this process.

MACRO ANALYSIS

Perhaps the most interesting and unusual questions we can address with these data are those framed at the macro level, using the court and/or political system as the unit of analysis. In moving to the macro level, we

TABLE 8. Regression of Diffuse Support on Specific Support and Awareness among Attentive Publics

	Intercept	*b*	(s.e.)	Beta	*N*		Intercept	*b*	(s.e.)	Beta	*N*
Russia					358	France (1995)					649
Satisfaction	54.18	.79	1.47	.03		Satisfaction	3.06	10.71	1.22	.32***	
Awareness		1.06	1.49	.04		Awareness		6.57	1.29	.19***	
Luxembourg					145	Portugal					235
Satisfaction	54.07	5.94	2.70	.18*		Satisfaction	36.41	11.68	2.83	.26***	
Awareness		−4.81	2.54	−.16		Awareness		4.02	2.45	.11	
Spain (1995)					653	Denmark					294
Satisfaction	38.18	6.87	1.05	.25***		Satisfaction	34.66	12.13	1.82	.37***	
Awareness		3.05	1.06	.11**		Awareness		3.12	1.90	.09	
Bulgaria					854	Great Britain					295
Satisfaction	39.50	8.97	.97	.30***		Satisfaction	25.35	11.30	1.86	.34***	
Awareness		−1.18	1.11	−.04		Awareness		5.54	1.77	.17***	
Ireland					291	United States					803
Satisfaction	34.94	9.00	1.56	.32***		Satisfaction	34.79	13.16	.94	.45***	
Awareness		1.58	1.44	.06		Awareness		1.98	.92	.07*	
Spain (1993)					258	Italy					271
Satisfaction	19.54	9.07	1.81	.29***		Satisfaction	29.45	12.60	2.25	.32***	
Awareness		6.25	1.70	.22***		Awareness		5.98	2.24	.15**	
France (1993)					278	The Netherlands					282
Satisfaction	33.34	6.24	3.18	.12*		Satisfaction	33.55	13.27	2.59	.30***	
Awareness		9.01	2.54	.22***		Awareness		6.99	2.39	.17**	
Belgium					211	Germany (East)					301
Satisfaction	23.53	8.48	2.47	.22***		Satisfaction	21.13	16.33	1.89	.45***	
Awareness		10.30	2.41	.28***		Awareness		2.74	1.72	.08	
Poland					690	Germany (West)					194
Satisfaction	43.92	10.60	1.29	.30***		Satisfaction	27.84	15.87	2.04	.50***	
Awareness		2.52	1.17	.08*		Awareness		2.68	1.90	.09	
Hungary					652	Greece					281
Satisfaction	38.25	11.18	1.25	.33***		Satisfaction	36.27	17.56	1.61	.56***	
Awareness		1.93	1.32	.06		Awareness		.05	1.86	.00	

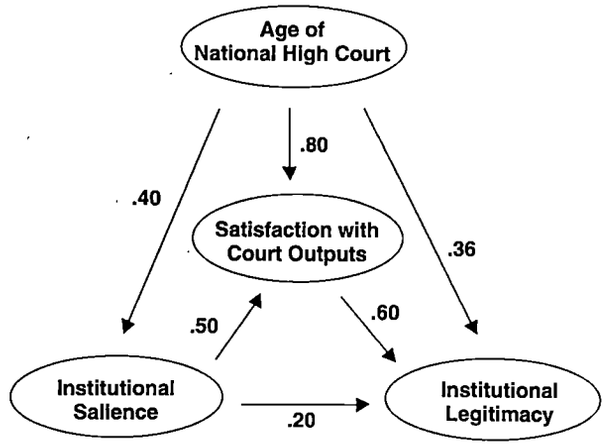
Note: The countries are rank ordered on the magnitude of *b*, the unstandardized regression coefficient. Beta is the standardized regression coefficient; s.e. is the standard error of the unstandardized regression coefficient. *$p < .05$, **$p < .01$, ***$p < .001$.

use the micro-level survey data to derive indicators of the attributes of the national high courts. Thus, average scores on diffuse support represent institutional legitimacy, awareness reflects the degree to which the court has made decisions of interest to ordinary people (the salience of the institution), and so forth. Though the number of cases is relatively small, perhaps something can be learned from careful and restrained analysis at the macro level.

Figure 1 reports the bivariate relationships among the three principal systemic variables—institutional legitimacy, satisfaction with court outputs (specific support), and institutional salience.[11] Several relationships are noteworthy. First, the salience of the national high court is moderately related to satisfaction with its outputs ($r = .50$). Generally, courts become visible not through controversial decisions that most people oppose but by ruling in a direction pleasing to the majority.

But salience does not necessarily directly increase institutional legitimacy ($r = .20$). To know a court is to be pleased with its policy outputs but not necessarily to love the institution (in the sense of granting it legiti-

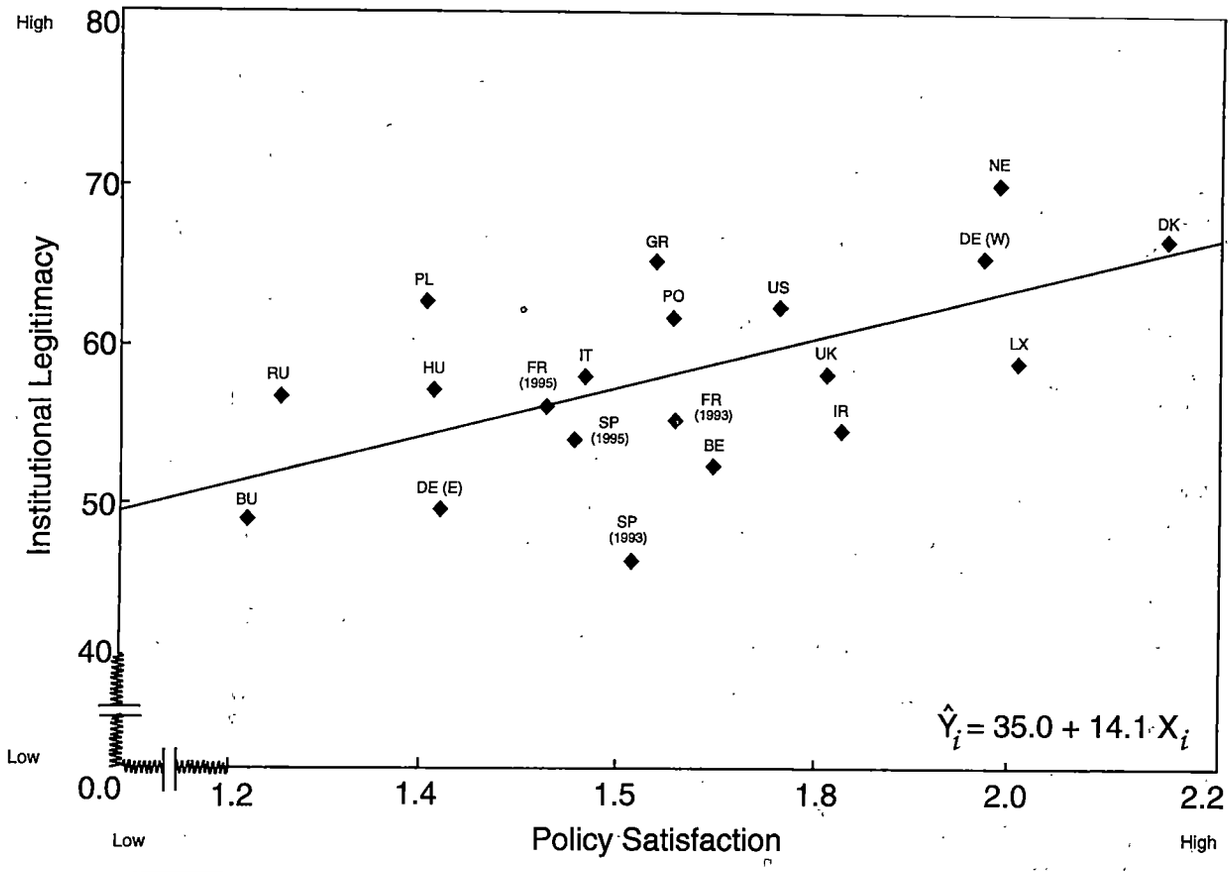
FIGURE 1. Macro-Level Relationships among Institutional Age, Salience, Satisfaction, and Legitimacy



Note: Entries are bivariate correlation coefficients. Weighted $N = 18$.

[11] The duplication of France and Spain in the macro-level data analysis creates certain statistical problems, since the observations are not strictly independent. We have not implemented the typical corrections for such dependence because we are little concerned with inferential statistics here. In no sense are these 20 observations representative of any larger group of courts, and we are mindful, indeed, of the dangers of generalizing from this particular sample.

FIGURE 2. Relationship between Institutional Legitimacy of and Policy Satisfaction with National High Courts among Attentive Publics



Note: BE = Belgium, BU = Bulgaria, DE (E) = East Germany, DE (W) = West Germany, DK = Denmark, FR = France, GR = Greece, HU = Hungary, IR = Ireland, IT = Italy, LX = Luxembourg, NE = The Netherlands, PL = Poland, PO = Portugal, RU = Russia, SP = Spain, UK = United Kingdom, US = United States.

macy). The primary importance of salience lies in its contribution to satisfaction with institutional outputs.

At the level of the individual citizen, diffuse and specific support are only moderately related in most nations; at the level of the political system, satisfaction with court outputs is strongly related to institutional legitimacy ($r = .60$). Figure 2 displays this relationship in more detail. Courts in the nations above the regression line have a reservoir of legitimacy in excess of what we would expect, based upon the aggregate level of specific support for the institution's policies; in this category are Poland, Greece, and the Netherlands. Courts below the line have less legitimacy than anticipated as a result of the effect of satisfaction; Spain in 1993 is the most striking instance. Generally, to the extent these institutions generate policy outputs pleasing to most people, on average, they have a greater store of institutional legitimacy.

The coefficients in Figure 1 may well reflect the cross-sectional residue of a longitudinal process. Over time, courts may gain visibility by making a series of decisions pleasing to various constituencies. Contemporary approval may therefore reflect the summation of approval over the years, and it may indicate that a succession of constituents has been satisfied over time. Why are court decisions generally pleasing rather than displeasing? Courts, in contrast to parliaments, are able to frame decisions as being required by a constitution or laws, and thus place their policies on a procedural rather than a substantive plane. This framing creates an asymmetry in the relationship between diffuse and specific support: Courts garner legitimacy from pleasing decisions but lose little or nothing from displeasing decisions, since they can transfer responsibility for unpopular decisions to the "law." In this sense, courts do not suffer from negativity bias, the common tendency for negative policy outputs to be more readily noticed than positive outputs.[12] As satisfaction with the policy outputs accumulates, institutional legitimacy grows. Institutional salience is not necessarily related to legitimacy at any given time; instead, salience is associated with knowing about and approving of judicial outputs, which is different from, even though connected to, extending legitimacy to the institution.

To what degree are any of these relationships a function of the age of the institution? Some of these

[12] This is contrary to some earlier work in other subfields. See, for example, Skowronski and Carlston 1989 and especially Lau 1985.

courts came into existence two centuries ago (e.g., the U.S. Supreme Court); others were established only very shortly before the surveys (e.g., Russia). It is reasonable to hypothesize that, ceteris paribus, the longevity of the institution has something to do with how ordinary people feel about it.

As also shown in Figure 1, the salience of the national high court is only moderately related to its age ($r = .40$), probably because chronological age is not equivalent to political age.[13] As we noted above, the important factor may be how long the institution has been in the public eye. Longevity may contribute only moderately to visibility, since even very old courts have only recently moved into the public spotlight. Moreover, salience may not contribute to a running tally in the same way as it generates satisfaction. It is possible that simply becoming salient to the mass public does not necessarily leave a residue on mass consciousness. We suspect that people may remember that they approve of the actions of the institution, and their running tally may reflect an increment or a decrement due to some earlier decision, but people do not keep track of how salient institutions are or have been (Lodge and Taber n.d.).

There is a very strong relationship ($r = .80$) between the age of the court and the level of satisfaction with its outputs. Apparently, courts that function for some time develop satisfied constituencies. This may be due to learning how to please constituents, or satisfaction may reflect not only contemporary evaluations but also the accumulation of satisfaction over time.[14] We speculate that although courts may be minoritarian institutions, if they are able to please a series of minorities, then they may gradually draw the support of a majority by assembling what amounts to a "coalition of minorities."

The age of a national high court is not very strongly related to legitimacy ($r = .36$). Consonant with the expectation that time alone does not directly enhance support, controlling for satisfaction completely reduces the relationship to statistical and substantive insignificance. What is important for the legitimacy of national high courts is the accumulation of satisfied constituents, not the accumulation of years. As satisfaction accumulates, a more enduring allegiance to the institution develops.

One implication of this hypothesized process is that the degree of connection between satisfaction with policy outputs and institutional legitimacy should increase over time. *Within a system*, the accumulation of specific support, rather than the absolute level of support at any given moment, shapes legitimacy. *Across systems*, the degree of connection between output satisfaction and legitimacy should therefore be contingent upon the age of the institution, especially in light of the strong relationship between longevity and satisfaction with policy.

To test this hypothesis, we use the unstandardized micro-level coefficients that resulted from regressing diffuse support on specific support (Table 7) as the macro measure of the rate at which policy satisfaction translates into institutional legitimacy. The correlation between these coefficients and the age of the national high court is only .21; although specific support and institutional legitimacy are more closely connected among older courts, the relationship is modest.[15] Older courts, on balance, tend to be more legitimate, and they tend to have more accumulated specific support, which more readily translates into institutional legitimacy.

Not much of this relationship is linear; instead, we find a large threshold effect. Specifically, there is a remarkable difference in the strength of the correlation between specific support and legitimacy depending on the institution's age. The courts are easily divided into two groups: nine younger than thirty years and eleven older than that. Among the former, the correlation between specific and diffuse support is .29; among the latter it is .72. This is a stunning difference in the magnitude of the relationship. Policy satisfaction and institutional legitimacy are more successfully linked in older institutions than in younger courts.

The data also reveal a very strong relationship between the intercepts and the slopes of the diffuse support and specific support regressions ($r = -.74$): As the magnitude of the intercept increases, the slope decreases. These intercepts indicate the level of support for the high court when satisfaction with its performance is at its nadir. In some sense, then, these coefficients represent the depth of the reservoir of goodwill at its shallowest. As the reservoir fills, specific and diffuse support become more weakly connected, suggesting that the marginal benefit of satisfying more people declines. The larger the hardcore support, the more difficult it is for the court to enhance its supply of legitimacy within the mass public.

There is also a powerful relationship between the age of the institution and the degree to which awareness translates into satisfaction. As age increases, the connection between the salience of the institution and specific support increases ($r = .71$). For older courts, this suggests that becoming salient strongly enhances satisfaction. Since we have seen that increased satisfaction more readily translates into legitimacy among older courts, the stronger connection between salience and satisfaction means that salience is a strong but indirect contributor to institutional legitimacy (indeed, the correlation between court age and the rate at which awareness translates into diffuse support is .45).[16]

[13] For the statistical analysis, we recoded the birth year of all the older courts to 1945. This not only eliminates statistical skewness but also recognizes the distinction between chronological and political age.

[14] Hoekstra and Segal (1996) make a similar argument based on the aggregation of support across various local communities in the United States.

[15] There is some curvilinearity in the relationship associated with skewness in the age of the courts. A simple quadratic equation results in explaining 12% of the variance.

[16] We have investigated whether these findings are in part a function of the particular legal cultures represented in our sample of countries. Relying upon our earlier work on the legal cultures of the nations of the European Union (Gibson and Caldeira 1996), we discovered that legal culture is related to the age of the institution,

DISCUSSION AND CONCLUSION

With only twenty cases available for analysis, it is prudent not to push the statistical analysis much farther. We have already made some important discoveries, and we are full of ideas for additional research. Our principal findings are recapped below.

- Even within our fairly restricted sample, national high courts vary enormously in the degree to which they have achieved institutional legitimacy.
- The U.S. Supreme Court is not as unusual as typically depicted. Compared to European high courts, it is among the more salient and more legitimate institutions, but it is by no means sui generis.
- Generally, to be aware of a court is to be supportive of it. This positivity bias may be associated with exposure to the legitimizing symbols that all courts so assiduously promulgate.
- The acquisition of support is a dynamic process. Our data suggest that courts generate specific support by becoming salient, by making their policymaking activity known to the mass public.
- It appears that satisfaction slowly evolves into institutional legitimacy, and the degree of connection between specific and diffuse support is contingent upon the institution's age. At the micro level, this finding is compatible with the notion of a running tally; at the macro level, it suggests that young courts can only acquire legitimacy by making their decisions known to the mass public and waiting.
- It may not be necessary for a court to satisfy the majority of its constituents with any given decision. Courts may be able to build legitimacy by developing support among a succession of minorities. To the extent that a national high court appeals to and satisfies only a single constituency, it is unlikely to acquire legitimacy. But if different areas of policymaking please different constituents, then legitimacy is attainable through the cumulation of satisfied minorities.
- This strategy is possible due to the strong positivity bias in popular reaction to these courts. We argue that the bias may be a function of the ability of judges to frame issues in a light favorable to the maintenance of institutional legitimacy, thereby blunting the effect of unpopular decisions.
- Finally, though we found some cross-national variability, these empirical results are remarkably consistent across the countries sampled.

Yet, the data also give rise to some important puzzles that may require fundamental rethinking of the processes involved in acquiring and maintaining institutional legitimacy. For instance, as we have shown, satisfaction with court outputs and institutional legitimacy are closely connected among established courts but weakly connected among new ones. The age of a court is more strongly related to the level of specific support than to institutional legitimacy. Conventional theory about the origins of legitimacy (e.g., Easton 1965) predicts that, with time, diffuse and specific support will become more divorced rather than more closely connected, since diffuse support is so little affected by short-term quibbles with the institution's performance. Our results require reconsideration of the relationship between these two forms of support.

Three possibilities occur to us. First, our measure of specific support may be systematically contaminated with traces of diffuse support. We neither asked respondents whether they approved of specific court decisions nor screened the question by awareness of specific decisions. Our measure is performance based, but it clearly asks the respondents for a summary judgment of how well the institution has been performing—to report something akin to a running tally of their feelings about the high court. Thus, our measure may come close to encroaching on diffuse support, or it may tap an orientation somewhere between diffuse and specific support.

Second, perhaps attitudes toward institutions are more complicated than we originally assumed. We have resisted making a distinction finer than that between specific and diffuse support, especially since we are mindful that not all scholars recognize the utility of differentiating these two attitudes in the first place. But perhaps many people hold one of three types of views toward institutions. (1) They may evaluate specific institutional outputs—after the fashion of presidential or ministerial popularity. Such attitudes are obviously highly fluid. (2) They may also develop a running tally, a summary of past satisfactions. This attitude is a judgment about how well the institution has been performing its job in general. (3) They may hold views about the institution itself. These we refer to as "institutional commitments," or diffuse support. Thus, people evaluate the day-to-day outputs of institutions, summarizing those evaluations in a generalized sense of the quality of institutional performance. After enjoying sustained satisfaction, people develop commitments that transcend short-term, or perhaps even relatively mid-term, dissatisfaction with the outputs of the institution. Though the interrelationship among these three attitudes is assuredly complicated (and changes over time), we suspect that at least some people have differentiated views of institutions and the policies they produce.[17]

We would like to add one more heresy to this revised framework. Perhaps diffuse support is a cause and not a consequence of specific support, at least in political systems within which attitudes toward courts have crystallized. That is, the level of commitment to an institution may color the views one holds of its performance. Through processes of framing and selective

but no relationship in Figure 1 is seriously altered by adding legal culture as a control variable. Obviously, this does not greatly enhance our confidence in generalizing the findings from this analysis, but at least one obvious threat to their universality can be discounted.

[17] Hibbing and Theiss-Morse (1995) are not reticent about breaking apart attitudes toward the U.S. Congress. They show that people have separable views toward (a) their own representative in the House, (b) members as a collective, and (c) the institution. Our proposal is not much more complicated than theirs.

perception, those with allegiance to a court may be likely to discount objectionable decisions and give excessive credit for favorable decisions. Part of this process may involve selective perception—one pays more attention to decisions with which one agrees. But another element may have to do with processes of assigning blame for bad decisions. Those with a strong commitment to a court are most likely to be those who have been continuously exposed to the legitimizing symbols propagated by courts (reinforcing the effects of preadult socialization), and thus they are likely to have an alternative explanation for a particular decision (or even a particular set of decisions)—for example, it was compelled by "the law" or "the Constitution." If courts frame unpopular decisions to avoid responsibility for policy, then a positivity bias would result: Favored decisions would result in an increment in the running tally, but disfavored decisions would not result in decrements. This would also mean that diffuse support causes specific support by providing a framework for interpreting and evaluating the outputs of courts. If so, then the research agenda for the field needs a profound reorientation.

Much of our analysis is admittedly speculative. Though we have amassed the broadest collection of data ever assembled on mass attitudes toward national high courts, our findings are nonetheless limited to the United States and Europe. And our efforts to tease longitudinal processes out of cross-sectional data will surely not please everyone. Still, we are convinced of three things: Additional case studies of the U.S. Supreme Court (at least using conventional survey methods) can do little to advance our understanding of the processes by which courts acquire legitimacy; comparative politics must begin to include courts and law in its research agenda; and the worldwide trend toward the "judicialization of politics" provides enormous opportunity for using cross-national research on courts to test theories of institutional support heretofore thought untestable.

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Attitudes toward Economic Reform in Mexico: The Role of Political Orientations

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Since the debt crisis of 1982, Mexico has experienced more than a decade of market-oriented economic reform, but research on public opinion toward reform is limited. Drawing on general findings from opinion research in the United States, this study examines how policy preferences of Mexicans are shaped by social background, judgments about the economy, and political loyalties. The effect of these variables is examined across three national surveys, conducted in 1992, 1994, and 1995. We found that favorable orientations toward the president and the ruling party were consistently the strongest predictors of preferences about reform. Furthermore, as in the United States, sociotropic evaluations of the economy outweigh "pocketbook" concerns; despite many years of reform, both expectations and retrospective judgments are important in shaping preferences, particularly since the 1994 crisis; and social background variables have limited direct influence.

Since the debt crisis of 1982, Mexico has experienced a series of stabilization efforts, trade liberalization, privatization, and other market-oriented reforms, but economic performance has fluctuated markedly.[1] In the early 1990s, after a long recession, the reforms appeared to be leading toward a significant recovery. In 1995, however, the economy again turned sharply downward as a consequence of the peso crisis and the uncertainties stemming from the 1994 presidential transition. This study examines public opinion about economic reform within this changing macroeconomic and political context, drawing on data from national opinion surveys conducted in 1992, 1994, and 1995.

The analysis focuses primarily on the extent to which opinions about reform are influenced by orientations toward the president and the ruling party, which for almost seven decades have been the core institutions of the Mexican political regime. How important were these orientations, as distinguished from judgments about the effect of the reforms on personal well-being or on overall performance of the economy? To what extent did political loyalties continue to shape opinion during the severe crisis of 1995, a period in which virtually all Mexican households experienced a sharp decline in their economic situation?

Two other issues discussed in the survey research literature also receive attention. One is the extent to which policy preferences are influenced by "sociotropic" judgments about their effect on collective well-being. This question has been discussed extensively in studies of public opinion in the United States (Kiewiet 1983; Kiewiet and Rivers 1984; Kinder 1981; Kinder and Kiewiet 1979, 1981; Kinder and Mebane 1983; Lau and Sears 1981; Rosentone, Hansen, and Kinder 1986). It is also relevant to countries in which market-oriented adjustments improve economic performance but have limited or negative effects on the distribution of income. The second issue highly relevant to the politics of reform is the extent to which the public may be willing to accept short-term sacrifices in the expectation of future benefits. The acceptance of such tradeoffs has been recently explored in works on Peru (Stokes 1996), Poland (Przeworski 1996), and Mexico (Buendia 1996).

Our main concern, however, is how such judgments are mediated by orientations toward the president and the ruling party. The weight of political mediations in the formation of policy preferences is a function of both the salience of the issue to the individual and the costs of acquiring information about it. Stabilization and structural adjustment initiatives are highly salient to most Mexicans, and we expect that they will be inclined to evaluate directly the way such measures are likely to affect their economic well-being. But it is costly to acquire information about distributive consequences, sustainable alternatives, and future prospects. Political cues and loyalties can be important for reducing those costs (Popkin 1994).

In Mexico, both the president and the ruling party (Partido Revolucionario Institucional, or PRI) have been major focal points of political loyalties throughout most of the twentieth century. Presidents traditionally have towered over the political landscape, exercising extensive powers during their six-year term. As heads of state and of the ruling party, they controlled access to all major bureaucratic and elective offices, dominated the policymaking process, and named their successor. They have also exercised significant influence over public opinion, both through the quasimonopolistic and proregime television networks and through private newspapers that depend on the government for advertising and newsprint. In response to

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The authors are very grateful to the following people for their criticisms and advice in the preparation of this manuscript: Ulises Beltrán, Jorge Buendia, Bruce Chadwick, Blanca Elena del Pozo, Richard Lau, Oscar Torres, and especially Jane Junn and Robert Shapiro. Research was financed by the Institute of Latin American and Iberian Studies of Columbia University with a grant from the Hewlett Foundation. The Center for International Business Education of Columbia University provided additional funds.

[1] For discussions of the economics of this period, see Lustig (1992) and Bazdresch (1993).

the 1982 debt crisis, presidents De la Madrid, Salinas, and Zedillo deployed their powers to push through wide-ranging stabilization and adjustment initiatives, and Salinas in particular used his extensive influence over the mass media to cultivate public support (Beltrán and Moreno 1996; Centeno 1994).

The PRI has been a much more reluctant partner in this process. Its main support historically has come from social sectors with a major stake in government patronage and protection, and important factions have resisted change in the policy status quo. Nevertheless, until at least the mid-1990s, the PRI remained a pivotal instrument through which reform-oriented presidents controlled opposition and gained electoral legitimation. Although the party's electoral hegemony declined markedly during the 1980s and the 1990s, a large percentage of voters continued to support PRI candidates. Their support arguably stemmed from a mixture of motives—habit, clientelistic ties, or perhaps aversion to the risks of political change. Nevertheless, many PRI loyalists may be expected to accept reform simply because it is associated with the ruling party.

A number of surveys in Mexico provide useful insights into the bases of support for the president and ruling party. These studies found that one factor is generalized judgments about the stability of the dominant-party regime and the viability of the political alternatives, rather than specific policy preferences or ideological orientations (see Davis and Coleman 1994, 358; Dominguez and McCann 1995, 1996). Davis and Langley (1995) suggest that as Mexico grows more competitive electorally, party identification may become increasingly important in evaluations of presidential performance. These findings are generally consistent with our arguments regarding political mediations.

We are concerned, however, with explaining issue preferences rather than political choice. We want to understand the sources of individual attitudes toward market reform, in particular, how they are formed, and how they relate to other characteristics and beliefs of individuals. In raising these questions, we address assumptions common in influential works on the politics of economic reform (e.g., Frieden 1991; Haggard and Webb 1994; Przeworski 1991). These attach considerable importance to socioeconomic background, distributive interests, and judgments about economic conditions, but they generally pay less attention to the role of political mediations.

Orientations toward the president and the PRI, of course, can be expected to affect policy preferences in combination with these other influences. As we shall discuss below, the interaction among such factors is likely to be quite complex. The underlying principle of our analysis, however, is relatively simple: The direction of causality goes from sociodemographic background, to judgments about the economy and political loyalties, to preferences about reform.

The article is organized into six sections. The first provides a description of the data. The second discusses the measurement of opinions about reform—the dependent variable. The third shows the bivariate associations with variables related to social background, economic judgments, and political loyalties. The fourth and fifth provide a multivariate analysis and a more complex causal model of opinion formation based on these findings. Finally, there is a brief conclusion.

THE SURVEYS

The three national surveys on which this study is based were organized by the Mexican Office of the Presidency. Questionnaires were designed within the office; the survey was conducted by Opinion Profesional S.A. de C.V., a private company specializing in policy polling and focus groups. Field workers were not informed of the client's identity; they identified themselves to respondents only as employees of Opinion Profesional. Because it was expected that many respondents would be reluctant to express their voting intentions and political preferences, a simulated secret ballot procedure was employed for these questions. Respondents were asked to mark their preference on a sheet of paper, seal it, and place it in a box.

The surveys in September 1992 and November 1994 contained 4,960 and 4,998 household interviews, respectively; in March 1995, there were 2,816 household interviews.[2] Households were located for interviews through a multistage, stratified probability sampling procedure. At each household, an interview of about 45–60 minutes was conducted with one respondent, selected at random, with about an equal proportion of men and women. The response rate is estimated at 75%; interviews were obtained in three of every four households contacted. For each survey, the margin of error attributable to sampling error was estimated at +/−2%. Further details on sampling and questions are provided in appendices A and B.

Government-sponsored surveys can sometimes be criticized for loading questions in ways that elicit responses favorable to the political objectives of the president. In these surveys, however, respondents were offered explicit "pro and con" choices on most issues, including the performance of the president, electoral preferences, general support for the government's economic program, and opinions on seven of the ten policy items used in our analysis (see the appendices for precise wording of each item). Exceptions were the items on NAFTA and bank privatization in the 1992 survey and the question on the stabilization pact in 1994, which asked respondents only if they "approved." The potential affirmative biases contained in such wordings, however, are more likely to constitute a problem for estimates of aggregate distributions of opinion than for analyses of the association among variables. The risks, moreover, are reduced still further by the use of multiple measures of the dependent variable, which produce similar results.

[2] Since 1995, the data from these surveys have been available to scholars at the Roper Center in Connecticut and at the Centro de Investigacion y Docencia Economica in Mexico.

TABLE 1. Association between General Support and Policy Index and Specific Policies

	1992	1994	1995
1. Correlation analysis (Pearson's *r*)			
General Support with			
Policy index	0.34	0.41	0.59
Agreement with NAFTA	0.37	0.33	—
Bank privatization	0.31	0.33	—
Approval of wage-price pacts	—	0.42	—
Agreement with foreign investment	—	0.27	—
Agreement with inflation control	—	—	0.43
Approval of wage-price inflation control	—	—	0.31
Agreement with stabilization program	—	—	0.58
2. Reliability analysis of policy index			
Cronbach's Alpha	0.54	0.62	0.54
Number of items in the index	2	4	3

Note: Coefficients are significant at the $p < .001$ level. Values not available are marked with a dash (—).

MEASURING OPINIONS ABOUT ECONOMIC REFORM

Two different measures of opinions about reform were used, each with some distinct advantages and disadvantages. One was *General Support*, based on a question that asked respondents to evaluate the "economic measures" taken by the government. Respondents who viewed those measures as "adequate for resolving the economic situation" were regarded as strong supporters of reform; those who thought only "some" measures were adequate were considered moderate supporters; those who answered "inadequate" were labelled opponents. The same general support item was included in all three surveys and is thus especially useful for gauging changes in opinion over time.

The other measure was an additive *Policy Index*, based on responses to questions about more specific stabilization and structural adjustment policies that were major components of the government's market-oriented strategy: NAFTA, privatization, foreign investment, and wage-price agreements (see Appendix A for precise questions and coding process). The policy items used for the index differed across the surveys, making the indexes less useful than the general support measure for comparisons over time. Yet, the specific policy content of the indexes provides confidence that responses do not simply reflect more general political predispositions.

Table 1 shows that opinions about specific policies were consistently associated with support/opposition for the general policy measures taken by the government. This provides a good indication that both sets of measures are valid—that they tap actual opinions about salient issues of economic reform. The 1992 respondents who agreed with the general economic policies of the government also tended to support NAFTA and, to a somewhat lesser extent, the privatization of banks. In 1994, general support was closely associated with foreign investment and the antiinflation program. In 1995, those who favored the government's overall economic management were also inclined to support the stabilization program announced by President Zedillo, to express faith in pacted wage-price agreements, and to express confidence that the program would contain inflation.[3]

Before analyzing these preferences at the individual level, it is important to consider briefly the broader political and economic context in which the surveys were conducted and how aggregate opinion toward reform changed over time. The 1992 survey was conducted during the third consecutive year of recovery from almost a decade of economic stagnation and price instability (Banco de México 1993), and general support was predictably high: 57% wholly approved of government policies, 33% approved in part, and only 10% opposed.

During the next two years, the situation was less promising. In 1993 the growth rate flattened, and in 1994, the year of the next national survey, the turbulent process of presidential succession entered into full swing. Strong and moderate supporters fell to 44% and 38%, respectively, while opponents rose to 18%. By the March 1995 survey, the peso crisis had occurred, and the Mexican economy was entering its third month of resurgent inflation, recession, and growing political uncertainty. Almost half the population (47%) expressed disagreement with the reforms, while strong and moderate support dropped to 31% and 22%, respectively.

This sharp decline in support is consistent with Jorge Buendia's (1996) finding that evaluations of reform in Mexico are based on retrospective judgments about economic performance rather than on expectations of improvement. Surveys conducted in Peru (Stokes 1996) and Poland (Przeworski 1991, 1996) indicate that people in those countries are willing to accept "temporary" hardships during the early phases of reform in the expectation of long-term benefits. After so many years of adjustment in Mexico, it is not surprising that people have begun to respond to economic fluctuations in a manner more typical of "normal" market economies.

A somewhat different puzzle is posed by the fact that, despite deteriorating economic conditions and growing public dissatisfaction, a large sector of the population (53%) continues to express complete or partial agreement with government policies. This raises the question of how opinion is distributed among different population sectors. Are some groups more likely than others to support or oppose reform, and

[3] An additional check on the coherence of the policy index was provided by a principal-component factor analyses of the individual items. In each year, this yielded only a single factor with an eigenvalue greater than 1.0. Factor loadings for all but one of the individual items ranged from more than 0.6 to more than 0.8. The lowest loading was .58, for approval of the wage-price pact in 1994. Principal-component analyses which included both general support and policy items produced similar results. For each year, the analysis produced a single factor, with individual loadings ranging from .65 to .78 in 1992, .58 to .75 in 1994, and .54 to .82 in 1995. For reasons discussed in the text, however, we analyze the policy index and general support measures separately.

what factors influence their opinions? In the following sections, we address this question with individual-level data from the three surveys.

SUPPORT FOR REFORM: BIVARIATE RELATIONSHIPS

We begin by examining the bivariate associations between attitudes toward reform and the three sets of independent variables mentioned in the introduction: sociodemographic background, judgments about current and future economic performance, and political attachments to leaders and parties. From what we know generally about the effects of economic reform across socioeconomic groups and geographic regions (Lustig 1994; Morely 1994), we can hypothesize that several demographic variables may reflect respondents' "objective" interests with respect to reform. Public sector employees may be more inclined to opposition than those in the private sector, since the former are more threatened by privatization and the downsizing of the state. Support for reform may also be relatively low among residents of greater Mexico City. The urbanized and relatively cosmopolitan capital and its surrounding territories have long been an important center of opposition to the ruling PRI and the established authority of the regime (Beltrán and Moreno 1996). In addition, this region contains a high concentration of import-substituting firms and government workers.

The possible influence of income, education, and gender is much more ambiguous, but we can suggest a number of ways in which they may affect opinion about reform. Wealthier groups would be in the best position to tolerate the risks associated with large-scale transformation.[4] Education would increase access to information about actual and potential policy effects. In Eastern Europe, there is some evidence that people with more schooling expect to benefit from reforms and therefore are more inclined to support them (Evans 1995). In Mexico, however, prolonged efforts at economic adjustment have yielded mediocre or poor results, so people with more education may be skeptical about the government claims. Finally, reforms have opened up new employment opportunities for women, especially in the *maquiladora* (export-processing) sector of the economy. Despite their low wages and poor working conditions, they may be more inclined than men to favor reforms.

The surveys also asked for judgments about economic conditions. Respondents were asked to evaluate both their personal economic situation and that of the country—indicators of "pocketbook" and more "collective" concerns. Another set of questions measured intertemporal judgments—whether they expected their personal situation to improve in the future.[5] The 1995 survey included an item on fear of unemployment. We examined these responses, because such fears have been shown to be a very important source of opposition to reform in Eastern Europe (Przeworski 1993, 1996) and Peru (Stokes 1996) as well as Mexico (Buendia 1996).

For reasons discussed above, support for reform also can be expected to be associated with favorable orientations toward the president and the PRI. The direction of causality, to be sure, can be questioned; policy preferences can determine as well as reflect political attachments, and it is beyond the scope of this article to model the potential two-way effects. In this instance, however, government policies on stabilization and market-oriented reforms challenged principles of state-led development long supported by virtually all sectors of Mexican society. Thus, it is plausible to assume that Mexican presidents and their allies within the ruling party have led rather than followed public opinion on policy (Aguilar Camin 1989).

These impressions are supported by findings in other studies of Mexican public opinion. In surveys conducted in 1988 and 1991, Dominguez and McCann (1995, 1996, 9) found that in initial decisions to support the PRI or the opposition, views on policy issues were "only marginally related to voter intentions." Davis and Coleman (1994, 358) argue similarly that voters' "overall impression of the performance of the Salinas administration drives their views about performance in more specific policy areas." The widespread popularity enjoyed by President Salinas during his term thus seems far more plausibly attributable to his mastery of public relations, or perhaps to the prestige of the presidency itself, rather than to his privatization and trade policies.

As indicators of orientation toward the president and the PRI, we relied on responses to two items. The first asked whether respondents agreed or disagreed with the way the incumbent president (Salinas or Zedillo) was "handling his job." The second asked respondents to indicate their preferences among the major contenders in the most recent presidential election, each of whom was explicitly identified as a candidate of the PRI, the Partido Acción Nacional (PAN), or the Partido de la Revolución Democrática (PRD). Unfortunately, more direct questions about party identification were not included in any of the three surveys, and we do not claim that the item we use is a fully valid proxy. Nevertheless, the question introduces party into the respondent's frame of reference, and the answers can be presumed to provide a reasonable indication of partisan orientation.[6]

Bivariate relationships are shown in tables 2 and 3. Table 2 reveals how general support for the economic program is distributed among subgroups of the sample.

[4] It is also possible, however, that the very poor would be less inclined to oppose reform than the lower middle class, which historically has been highly dependent on public employment and services (Nelson 1992).

[5] In 1994 and 1995, this question did not make explicit reference to economic well-being, but it is reasonably safe to assume that the item measured *economic* expectations. Evidence from focus groups showed that economic well-being was the dominant issue in discussions of personal concerns.

[6] Preelection polls show strong correlations (between .7 and .8) between responses to the standard question on party identification and the items used in this study. There is a strong association between party identification and presidential vote (Mercado Gasca 1997, 316).

TABLE 2. General Support for Economic Reform, by Economic Judgment, Political Orientation, and Social Background

Grouping	1992				1994				1995			
Total Sample	Agree	Agree in Part	Disagree	*N*	Agree	Agree in Part	Disagree	*N*	Agree	Agree in Part	Disagree	*N*
General Support	57.4%	33.2	9.4	4,472	43.8%	37.9	18.3	4,694	30.7%	22.2	47.1	2,491
N				4,960				4,998				2,816
Peception of country's economic situation												
Very good and good	69.5%	28.2	2.4	1,574	57.7%	38.1	4.2	1,604	67.2%	20.2	12.7	55
Fair	58.4%	35.1	6.5	2,116	43.3%	37.1	19.6	2,102	45.0%	30.6	24.5	186
Bad and very bad	29.5%	37.4	33.1	730	19.1%	40.6	40.3	903	28.5%	21.5	50.1	2,233
N		***		4,420		***		4,609		***		2,474
Perception of personal economic situation												
Very good and good	66.8%	29.8	3.4	1,556	54.1%	37.8	8.1	1,673	54.2%	21.2	24.7	93
Fair	57.6%	34.5	8.0	2,190	42.3%	38.3	19.4	2,169	42.0%	25.0	33.0	441
Bad and very bad	36.3%	36.9	26.8	694	24.9%	37.3	37.8	782	26.6%	21.7	51.7	1,929
N		***		4,440		***		4,624		***		2,463
Personal expectations for the next year												
Will be better off	70.6%	25.7	3.7	2,264	52.3%	35.3	12.4	2,087	47.4%	17.6	35.0	498
Will be the same	45.3%	45.5	9.2	1,065	32.8%	49.4	17.8	863	35.7%	29.5	34.8	320
Will be worse off	30.3%	32.5	37.2	445	23.4%	31.1	45.4	563	21.7%	20.0	58.3	1,216
N		***		3,774		***		3,513		***		2,034
Agreement with the president												
Agree	67.0%	29.7	3.3	3,675	53.7%	36.7	9.7	3,627	46.6%	25.4	27.9	1,113
Disagree	11.1%	43.5	45.5	597	8.8%	37.4	53.7	859	14.5%	20.2	65.3	1,205
N		***		4,272		***		4,486		***		2,318
Electoral preference for presidential election												
Salinas/Zedillo (PRI)	69.1%	27.7	3.2	2,867	59.6%	33.1	7.3	2,632	50.8%	22.8	26.4	978
Clouthier/Fernandez (PAN)	47.8%	37.4	14.8	591	27.4%	40.6	32.0	871	17.9%	23.7	58.4	729
Cardenas (PRD)	29.5%	44.9	25.5	456	19.9%	39.7	40.4	517	13.6%	18.0	68.4	329
N		***		3,914		***		4,020		***		2,036
Fear of unemployment												
Yes fear	na	na	na	na	na	na	na	na	27.3%	22.1	50.6	1,813
No fear	na	na	na	na	na	na	na	na	40.5%	21.7	37.8	629
N										***		2,442
Sector												
Public	54.0%	34.8	11.2	653	35.3%	43.6	21.0	511	33.9%	22.5	43.5	225
Private	59.8%	31.6	8.6	2,063	45.5%	35.7	18.8	2,141	28.4%	22.3	49.2	1,136
N		*		2,716		***		2,652		ns		1,361
Region												
Mexico City	38.0%	49.5	12.5	858	27.0%	47.9	25.1	1,150	22.2%	26.5	51.3	527
Other	62.0%	29.3	8.7	3,619	49.2%	34.7	16.1	3,544	33.0%	21.1	46.0	1,964
N		***		4,477		***		4,694				2,491
Income												
Poor	55.4%	33.4	11.2	2,662	47.2%	34.8	18.0	2,440	30.5%	21.4	48.1	1,408
Lower middle class	58.9%	34.0	7.1	1,044	37.5%	42.4	20.1	1,141	31.7%	27.4	40.9	420
Middle class	61.1%	33.2	5.8	645	41.9%	41.5	16.6	906	28.1%	22.2	49.7	426
Upper middle class & rich	69.1%	22.3	8.7	127	46.6%	33.8	19.5	206	34.1%	18.0	47.8	237
N		***		4,478		***		4,693				2,491
Education												
None	60.2%	28.1	11.7	177	50.3%	30.9	18.9	179	30.0%	11.6	58.3	126
Elementary	58.2%	31.6	10.2	1,377	52.2%	32.3	15.5	1,463	33.4%	17.2	49.3	753
Junior high school	57.0%	34.0	9.0	1,113	42.4%	39.8	17.8	1,096	29.4%	24.1	46.5	570
Senior high school	57.7%	35.3	7.1	984	38.9%	41.9	19.2	1,097	30.6%	30.1	39.2	562
University or more	55.8%	33.5	10.6	826	36.1%	41.4	22.5	859	28.1%	21.3	50.6	481
N		ns		4,477		***		4,694		ns		2,492
Gender												
Male	57.4%	32.9	9.7	2,280	42.6%	39.6	17.9	2,443	27.8%	23.8	48.3	1,259
Female	57.4%	33.6	9.1	2,198	45.2%	36.1	18.8	2,251	33.6%	20.6	45.9	1,232
N		ns		4,478		ns		4,694		ns		2,491

na = not available.
ns = not significant.
Note: $^{*}p < .05$, $^{***}p < .001$. Significance levels are based on χ^2 estimates.

TABLE 3. Bivariate Associations between General Support/Policy Index and Economic Judgment, Political Orientation, and Social Background

	1992		1994		1995	
Variable	General Support	Policy Index	General Support	Policy Index	General Support	Policy Index
Perception of country's economic situation	0.33***	0.36***	0.35***	0.34***	0.18***	0.17***
	(4,420)	(3,300)	(4,610)	(3,734)	(2,474)	(2,062)
Perception of personal economic situation	0.25***	0.26***	0.27***	0.34***	0.18***	0.18***
	(4,440)	(3,319)	(4,625)	(3,733)	(2,463)	(2,049)
Personal expectations for the next year	0.37***	0.32***	0.30***	0.36***	0.25***	0.24***
	(3,774)	(2,880)	(3,512)	(2,951)	(2,034)	(1,737)
Fear of unemployment	—	—	—	—	−0.13***	−0.20***
	—	—	—	—	(2,442)	(2,034)
Agreement with the president	0.52***	0.36***	0.47***	0.41***	0.40***	0.36***
	(4,271)	(3,212)	(4,482)	(3,631)	(2,318)	(1,963)
Voting intention for presidential election	0.34***	0.21***	0.41***	0.36***	0.39***	0.44***
	(3,952)	(2,992)	(4,694)	(3,762)	(2,405)	(2,010)
Employed in public sector	0.05*	0.03	0.07**	−0.00	−0.05	0.02
	(2,715)	(2,124)	(2,652)	(2,163)	(1,361)	(1,151)
Reside in Mexico City	−0.17***	−0.02	−0.18***	−0.14***	−0.08**	−0.06*
	(4,478)	(3,344)	(4,694)	(3,762)	(2,491)	(2,069)
Income	0.07***	0.10***	−0.03	0.14***	0.00	0.03
	(4,478)	(3,344)	(4,694)	(3,762)	(2,491)	(2,069)
Education	−0.01	0.07**	−0.11***	0.08***	0.00	−0.05*
	(4,477)	(3,344)	(4,694)	(3,762)	(2,491)	(2,069)
Gender (M = 0; F = 1)	0.00	−0.01	0.01	−0.03	0.05*	0.01
	(4,478)	(3,344)	(4,694)	(3,762)	(2,491)	(2,069)
Sample total	4,960		4,998		2,816	

Note: Values are Pearson *R*s, significant at the *$p < .05$; **$p < .01$; ***$p < .001$ levels. The number in parentheses refers to valid cases in each bivariate association. The unemployment question was not asked in 1992 and 1994.

Table 3 shows the strength of the associations between the independent variables and the two measures of attitudes toward reform. Both general support and the policy indexes are strongly and consistently linked to economic perceptions and political orientation, but the associations with the demographic variables are far weaker.

Among the demographic variables, region is most strongly associated with preferences about reform. In both tables 2 and 3, residents of Mexico City are generally less likely to express favorable opinions than those in other regions of the country. As already suggested, this may reflect the effect of economic liberalization on the import-substituting economy of central Mexico or relatively more skepticism toward the government.

The relationship between opinions about reform and the other demographic variables is weak or inconsistent. Table 3, for example, shows that respondents with higher incomes and those who worked in the private sector were more likely to express favorable opinions in 1992 and 1994, but in 1995 these associations disappeared or went in the opposite direction. The association with education is positive in some cases and negative in others, and gender differences are insignificant across the three surveys.

One possible explanation for these findings may lie in the weakness of Mexico's civil society; interest associations were generally dependent on the government or ruling party and were not strongly inclined to articulate and contest the distributive implications of reform. It is also possible that sociodemographic background influences preferences indirectly, through effects on economic perceptions and political loyalties. This point is addressed in the causal models presented below.

In contrast to the sociodemographic variables, perceptions of economic well-being are closely linked to support for reform. The only surprise—and it is a very mild one—concerns fear of unemployment. This variable correlates significantly with both general support and the policy index; but a somewhat stronger association could have been expected, given the importance attached to fear of unemployment in the comparative literature. A possible explanation is that the substantial layoffs following Zedillo's stabilization initiative did not begin until after the March 1995 survey, and earlier Mexican reforms were not accompanied by very high rates of unemployment, unlike the situation in either Poland or Peru.

On the whole, however, preferences about reform are strongly associated with evaluations of the economy. People who judge the economy favorably are very likely to back both the overall government program and the specific policies. This holds for both retrospective and prospective judgments, for "sociotropic" and "pocketbook" concerns, and for fear of unemployment.

The multivariate analysis will provide a fuller discussion of the relative importance of these different types of judgment. It should be noted, however, that during the 1995 crisis, people expressing hope for the future made a much larger contribution to overall support for

reform than did the tiny minority of Mexicans who judged the current situation favorably. "Optimists" in 1995 constituted 17.6% of the total sample but more than 30% of those who supported the government's economic program.

Finally, there is a strong and consistent association between political orientation and support for reform, a pattern which persists through the 1995 crisis. Despite the severe economic deterioration, a large group of respondents expressed support for the president (47%) and the party (40%). Furthermore, the reform program of the government received approval from about half of these political loyalists (47% in the presidential group and 50.8% in the PRI group), compared to only 31% for the total sample.

Of course, the relatively strong support for the government's program among presidential and PRI backers could stem from their inclination to affirm their political loyalties rather than from their policy preferences. If that were true, then the distributions shown in Table 2 would simply reflect at autological relation between independent and dependent variables. For reasons discussed above, however, the policy indexes are less vulnerable to this criticism, and Table 3 shows a strong association between these indexes and political orientation. In most cases, in fact, it equals or exceeds the strength of association between the policy indexes and judgments about the economy.

SUPPORT FOR REFORM: MULTIVARIATE ANALYSIS

The central question raised by the bivariate associations is the extent to which political orientation plays an independent role in shaping opinion and the extent to which it simply reflects economic well-being. The collapse of the economy clearly undermined support for the president and the PRI candidates in 1995. Does this mean that loyalist respondents are backing the reform because of their positive views of the economy, or does political orientation have an effect that is not simply a reflection of economic perceptions?

These questions were examined through regressions of the measures of support against the independent variables discussed in the preceding section. The generic model is as follows:

$$\text{SUPPORT or POLICY INDEX} = \text{Constant} + b_1 \text{Economy} + b_2 \text{Personal} + b_3 \text{Expectations} + b_4 \text{President} + b_5 \text{Party} + b_6 \text{Income} + b_7 \text{Education} + b_8 \text{Mexcity} + b_9 \text{Gender} + b_{10} \text{Sector} + \text{Error},$$

where:

SUPPORT = general support for reform;
POLICY INDEX = policy index;
Economy = perception of country's economic situation;
Personal = perception of personal economic situation;
Expectations = personal expectations;
President = approval of president;
Party = political-electoral sympathies;
Income = income;
Education = education;
MexCity = Mexico City;
Gender = gender; and
Sector = public or private employment.

Tables 4 and 5 show the ordinary least squares (OLS) estimates of these models.[7] Taken together, the demographic, perceptual, and political orientation variables explain a substantial percentage of the variation in both the policy index and general support for reform. Adjusted R^2 in the two sets of regressions ranges between .34 and .27 for general support and between .21 and .26 for the policy index.

The weights of the individual variables within each equation—as indicated by the standardized betas—are also quite consistent across the surveys and correspond closely with what we found in the bivariate statistics. Both models are thus robust across time. Although residence in Mexico City had some effect on general support in 1992 and 1994, the demographic variables are generally among the weakest predictors of reform preferences. Gender crosses significance thresholds in the estimates of the 1994 and 1995 political index, but its contribution is limited, and contrary to our hypotheses, women are somewhat less likely than men to have favorable opinions about reform. As in the bivariate statistics, the strongest contributions come from judgments about economic conditions and political orientation.

Looking more closely at the way perceptions of well-being affect support for reform, two points can be noted. First, as in the United States, views of general economic conditions are consistently stronger predictors of support for reform than are perceptions of personal well-being. Standardized betas for the former are significant and relatively high in all six regressions, whereas personal well-being is significant only for the 1994 estimate of the policy index.

The second point of interest is the strength and consistency of expectations as a predictor of support for reform. In 1992 and 1994, this variable made substantial contributions to both general support and the policy index. During the crisis year 1995, as judgments about the economy turned uniformly negative, the standardized betas for expectations were surpassed only by the variables measuring political orientation. The importance of expectations indicates the need to modify somewhat the conclusions drawn at the aggregate level about retrospective judgments (Buendia 1996). These did contribute to a broad decline in backing for reform, but respondents continued to differ in their judgments about future prospects. As just noted, this helped to account for the persistence of support for government policy during 1995.

Finally, the multivariate models show quite strong support for our hypothesis about political mediations.

[7] In the case of SUPPORT, an ordinal measure, we used ordered probit regressions as well as OLS statistics. The ordered probit estimates are more suitable for the analysis of ordinal data. The results are very consistent with those obtained through OLS regressions. We present the OLS findings in order to facilitate comparisons with the policy index.

TABLE 4. OLS Regression of Economic Judgment, Political Orientation, and Social Background on General Support for Economic Reforms: 1992, 1994, 1995 (Dependent Variable: SUPPORT)

	B Coefficient			Standardized Beta		
Independent Variables	1992	1994	1995	1992	1994	1995
Economic situation	0.08***	0.19***	0.19**	0.09***	0.18***	0.08**
	(0.02)	(0.03)	(0.07)	(0.02)	(0.03)	(0.03)
Personal situation	−0.00	0.00	0.03	−0.00	0.00	0.02
	(0.02)	(0.03)	(0.05)	(0.02)	(0.03)	(0.03)
Personal expectations	0.17***	0.09***	0.11***	0.18***	0.09***	0.11***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Fear of unemployment	—	—	−0.06	—	—	−0.03
	(—)	(—)	(0.06)	(—)	(—)	(0.03)
Approval of president	0.74***	0.59***	0.45***	0.37***	0.31***	0.26***
	(0.04)	(0.04)	(0.05)	(0.02)	(0.02)	(0.03)
Intend to vote PRI	0.15***	0.23***	0.52***	0.11***	0.15***	0.29***
	(0.03)	(0.04)	(0.06)	(0.02)	(0.02)	(0.03)
Income	0.02	0.03	−0.02	0.02	0.04	−0.03
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)
Education	−0.03*	−0.01	0.03	−0.06*	−0.02	0.04
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Mexico City	−0.22***	−0.18***	−0.08	−0.14***	−0.10***	−0.04
	(0.03)	(0.03)	(0.06)	(0.02)	(0.02)	(0.03)
Public sector	0.03	0.08*	−0.02	0.01	0.04*	−0.01
	(0.03)	(0.04)	(0.07)	(0.02)	(0.02)	(0.03)
Gender	0.03	−0.03	−0.00	0.02	−0.01	−0.00
	(0.03)	(0.03)	(0.05)	(0.02)	(0.02)	(0.03)
(Constant)	0.50***	0.27***	0.31**	—	—	—
	(0.05)	(0.07)	(0.11)	(—)	(—)	(—)
Adjusted R^2	0.343	0.333	0.279	0.34	0.333	0.279
N	2,005	1,861	1,012	2,005	1,861	1,012
F	105.57***	93.71***	36.65***	105.57***	93.71***	36.65***
Sample total	4,960	4,998	2,816	—	—	—

Note: Standard errors are in parentheses. *$p < .05$; **$p < .01$; ***$p < .001$. The fear of unemployment question was only asked in 1995.

Even after controlling for perceptions of economic well-being and sociodemographic variables, positive assessments of the president and of PRI electoral candidates were consistently among the strongest predictors of support for reform. In both 1992 and 1994, a respondent's orientation toward the president was the best single predictor of general support and preferences on the policy index, and partisan preferences also made a substantial contribution relative to other variables in the model.

Political orientation continued to make the strongest contribution in the 1995 regressions. Unlike 1992 and 1994, however, the preference for PRI candidates tended to be a somewhat stronger predictor than was approval of the president. In all the estimates, moreover, there was an increase over time in the *b* coefficients of the partisanship variable. This pattern may reflect in part the contrast between the flamboyant Salinas and the more tentative style of Zedillo. Or, as Davis and Langley (1995) suggest, it may reflect the growing importance of partisan cues for making economic judgments, as Mexican party politics becomes more competitive. In either event, despite deep internal divisions among PRI activists, the party remained an important mechanism for building public consent (or at least acquiescence) to reform policies.

CAUSAL MODELS

The estimates in our multivariate analyses show the extent to which political orientation, economic perceptions, and demographic variables affect support for reform but not how they combine to do so. In this section, we model possible paths through which these variables influence opinion, building on the results shown above. The hypothesized causal relations and the path coefficients for 1994 are displayed in Figure 1. To simplify the presentation, we do not show the coefficients for 1992 and 1995, but these parallel closely the findings for 1994, and we indicate in the text the points at which they diverge.[8]

The sociodemographic variables in the model are region and income, both of which had at least a modest effect on support in our earlier analysis. Numerous studies have suggested that opposition to the regime is strongest in metropolitan areas (Mercado Gasca and Zuckermann 1994), and on the basis of this literature, we posit that region influences preferences about reform through its effects on political orientation. In-

[8] To illustrate our broader findings, path coefficients from 1994 were selected because that year was closest to "normal" in terms of the economic recovery and support for the president. In contrast, 1992 marked a high point, and 1995 was a year of profound crisis.

TABLE 5. OLS Regression of Economic Judgment, Political Orientation, and Social Background on Policy Index: 1992, 1994, 1995 (Dependent Variable: POLICY INDEX)

	B Coefficient			Standardized Beta		
Independent Variables	1992	1994	1995	1992	1994	1995
Economic situation	0.19***	0.09***	0.14*	0.19***	0.11***	0.07*
	(0.03)	(0.02)	(0.06)	(0.03)	(0.03)	(0.03)
Personal situation	0.01	0.04*	0.07	0.01	0.05*	0.05
	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
Personal expectations	0.15***	0.14***	0.06*	0.14***	0.17***	0.08*
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
Fear of unemployment	—	—	−0.12*	—	—	0.08*
	(—)	(—)	(0.05)	(—)	(—)	(0.03)
Approval of president	0.44***	0.36***	0.24***	0.20***	0.24***	0.17***
	(0.06)	(0.04)	(0.05)	(0.03)	(0.03)	(0.03)
Intend to vote PRI	0.13***	0.19***	0.51***	0.09***	0.16***	0.35***
	(0.04)	(0.03)	(0.05)	(0.02)	(0.02)	(0.03)
Income	−0.01	0.07***	0.02	−0.02	0.10***	0.04
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)
Education	0.03	0.06***	−0.03	0.05	0.12***	−0.05
	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.04)
Mexico City	0.01	0.08**	0.06	0.01	−0.06**	0.03
	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)	(0.03)
Public sector	0.05	0.05	0.06	0.03	0.04	0.04
	(0.04)	(0.03)	(0.06)	(0.02)	(0.02)	(0.03)
Gender	0.04	−0.10***	−0.09*	0.03	−0.07***	−0.06
	(0.03)	(0.03)	(0.05)	(0.02)	(0.02)	(0.03)
(Constant	0.48***	0.30***	0.53***	—	—	—
	(0.07)	(0.06)	(0.09)	—	—	—
Adjusted R^2	0.202	0.33	0.27	0.202	0.33	0.27
N	1,616	1,588	896	1,616	1,588	896
F	41.84***	77.86***	31.58***	41.84***	77.86***	31.58***
Sample total	4,960	4,998	2,816	—	—	—

Note: Standard errors are in parentheses. *$p < .05$; **$p < .01$; ***$p < .001$. The fear of unemployment question was only asked in 1995.

come, we suggest, affects policy preferences through a different path: Wealthier individuals may be expected to have more positive views of the economy and more positive expectations about the future, which in turn would encourage positive views of reform.

Specification of the sociodemographic antecedents of policy preferences are of less concern in this paper than are economic perceptions and political orientation. Nevertheless, the two factors that we highlight are not implausible points of departure for the analysis. Income and region consistently and significantly affect economic judgments, political orientation, and expectations in the directions hypothesized. The direct effects of both income and Mexico City on attitudes toward reform in 1994 are stronger than we expected, but they are still weak relative to the other factors presumed to have the most direct influence, and they disappear entirely in the specifications of the model for 1992 and 1995.

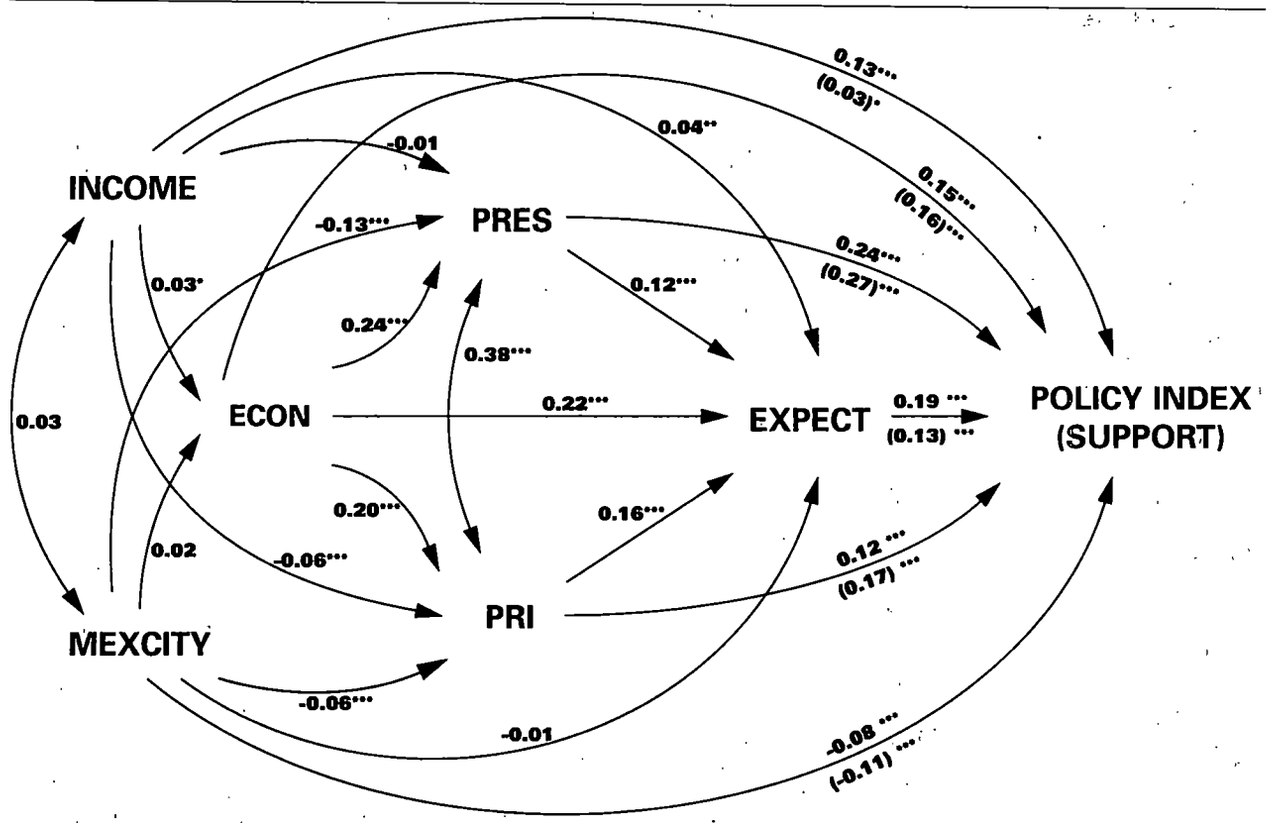
The most proximate influences on policy preferences come from three sets of variables: judgments about the economy, orientation toward the president and the PRI candidates, and expectations about future improvement. Yet, these perceptions affect one another, and support for reform, in several different ways.

Judgments about economic performance can have both a direct and an indirect (or mediated) effect on popular assessment of economic reforms. Given Mexico's long experience with adjustment, people can be expected to judge reforms in terms of how they think the economy is doing, regardless of their political orientation. Yet, judgments about economic performance also have an important effect on support for the president and the ruling party. People who feel positively about them are more likely to have faith in their policies, even if they are not sure how such policies have affected economic outcomes.

Our models allow us to disaggregate the total nonspurious effects of economic judgment on policy preferences and to compare the relative contributions made by the direct path and by paths passing through support for the president and the PRI. As expected, the direct path from economic judgment does indeed make a strong contribution to the policy index and general support—about 50% of the total nonspurious effects in 1994. But the paths leading through support for the president and the PRI also make important contributions to the nonspurious effects of economic judgments—about 33% of the effect on the policy index and about 36% on general support. In the other two years as well, about one-third of the effects of economic judgments on policy preferences passed through political orientation.

Expectations play a somewhat different role in our model. We suggest that they will affect policy preferences relatively directly but will be shaped by a number

FIGURE 1. Causal Model for Policy Index/Support, 1994



Note: Coefficients for support are in parentheses. $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$. INCOME = Family income; MEXCITY = residence in Mexico City; ECON = perception of country's economic situation; PRES = approval of president; PRI = party sympathies; EXPECT = personal expectations.

	Policy Index	Support
Adjusted R^2	0.30	0.31
N	2835	3339
F	201.04***	252.01***

of different influences. One path to expectations goes through judgment about economic performance; people who think things are going well are more likely to feel their own situation will improve over time. Expectations also may be shaped by political orientation. People who support the president or party in power are also inclined to trust them to bring a brighter future. These positive expectations, in turn, would lead to support for policy reforms. The path coefficients in Figure 1 show that judgment about the economy has the strongest single influence on expectations (.22) but is outweighed by the combined effect of support for the president (.12) and the PRI (.16). Expectations, finally, are consistently and fairly strongly related to support for reform in all years and all specifications of the model.

Finally, our model posits a number of different mechanisms through which positive views about the president and the ruling party can lead to support for reform. One, just discussed, is through shaping expectations about improvement in economic well-being. Other and more direct paths may reflect the way respondents evaluate the competence of the incumbents relative to opponents. People who support the economic policies of the regime do so because they believe it can produce better results than its challengers.

Our path analysis shows that the direct effect of presidential support outweighs both judgments about the economy and expectations, a finding that is generally true for 1992 and 1995 as well.[9] The effect of political orientation on policy preferences, it may be noted, is strongest relative to perceptions about the economy in 1995; in that year, the path coefficient leading from support for the president to the policy index reached .36, the highest in any of the estimates. At a time of economic crisis, people may support the policies of the regime not because of retrospective judgments or hopes for the future but because they perceive a lack of political alternatives.

[9] The single exception is for the 1992 policy index, in which the coefficient for economic judgment is .21 and that for the president is .19.

CONCLUSION

In all countries undergoing economic adjustment, public support is potentially an important factor in sustaining reforms. Survey research on the sources of support is still at a relatively early phase, and there is much to be learned. One conclusion is suggested rather clearly, however, both by existing theory and our own findings: Immediate "pocketbook" concerns are neither the only nor the most important basis on which people evaluate policy. Both socioeconomic interests and "subjective" perceptions of well-being may influence the formation of policy preferences, but this influence depends on how people link their personal situation to broader patterns of economic performance, prospects for the future, and the government in power. In these respects, Popkin's (1994, 31) characterization of American voters appears to apply to Mexicans as well: They "are not self-centered and reflexive in evaluating their leaders and making voting choices. Their evaluations and voting decisions depend on whether their reasoning connects their situations to the national situation and to the actions of their leaders."

When people give precedence to "national conditions" over "pocketbook concerns," they are not necessarily motivated by altruism; they may believe that reform will have a stronger effect on overall economic performance than on their own personal condition. Individual economic interests, to be sure, may influence the way people view the economy—a point we have suggested in our causal model. As in the United States, however, judgments about the economy as a whole are likely to have the most direct effect on the way people evaluate reform (Fiorina 1981, 90; Kinder, Adams, and Gronke 1989, 511; Lau and Sears 1981).

Although preferences for reform are influenced by judgments about how it will affect the future, we must qualify our findings about the role of expectations in several ways. Expectations may be less important in Mexico than in countries where adjustment initiatives are more recent. The sharp rise in the percentage of Mexicans opposing reform since the crisis of 1995 indicates increasing disillusionment with promises of the future prosperity; they are understandably inclined to judge reform in terms of the existing economic record. Yet, given the extraordinary hardships most Mexicans have endured since 1995, it is somewhat surprising that support for reform has remained so strong. While this may partially reflect a belief that there are no better alternatives, our data suggest that hopes for improvement continue to play an important part.

The most striking finding to emerge from our analysis concerns the role of political orientation in shaping policy preferences. Its importance is well documented in the American setting; in situations of high uncertainty and limited information, political cues and group loyalties can provide crucial short-cuts in the evaluation of both candidates and policies (Popkin 1994). In most countries undergoing economic adjustment, the quantitative effect of these mechanisms has not been analyzed, but impressionistic evidence suggests that they are important. Spanish workers were more inclined to accept painful market reforms in the 1980s, for example, because they were undertaken by the Socialist Party (Bermeo and Garcia-Duran 1994, 112–6; Haggard and Webb 1994, 16–8). In countries such as Peru and Argentina, policies pursued under Fujimori and Menem gained broad acceptance in part because they were backed by presidents who appeared to be "in charge" (Haggard and Kaufman 1995, 198–202). Our data from Mexico show a comparable effect, notwithstanding the long-term erosion of the regime's legitimacy. During his term, Salinas successfully used his authority and control of the PRI to build personal prestige and sell his reform program. People who were positively inclined toward the president and the PRI tended to support reform regardless of their views of the economy.

After Salinas, the "imperial" presidency declined, due to economic crisis and Zedillo's different style of political leadership. In his effort to respond to the crisis, however, Zedillo can still draw on considerable political capital. Acceptance of his program is still substantial, and a large segment of the population continues to approve of the president and to support the PRI.

In few societies are political loyalties unconditional; they are continuously subject to reevaluation as circumstances change and new information becomes available. Our path analysis indicates that while approval of the president and the ruling party provides a guide for determining policy preferences in Mexico, it is strongly influenced by evaluations of economic well-being. The president is, in effect, a salesman who must persuade the public that he can bring a brighter future, and his success depends to a significant extent on what has been delivered in the past.

During the mid-1990s, the evident failure to deliver on promises has had two important implications for the capacity of the president and the PRI to mediate preferences about reform. First and most directly, support for the regime has weakened, and belief in the capacity of its leadership to resolve the problems facing Mexican society has diminished. Put somewhat differently, the "new information" coming from the collapse of the economy is undermining the political predispositions that underlay beliefs about reform (Zaller 1992).

Second, the crisis has accelerated the trend toward a more competitive electoral system, which fundamentally alters the political context in which Mexican public opinion is formed. On the center-left, the PRD has appealed to voters dissatisfied with the government's market-oriented reforms. On the right, although PAN leaders tend to share the policy views of the president, support for reform is lower among their followers than among the backers of the PRI (refer to Table 2).

Over time, increasing multiparty competition in Mexico is likely to expose that society to a wider debate about alternative solutions to current economic dilemmas, and opposition parties will have a stronger role in shaping perceptions (Davis and Langley 1995). It remains to be seen whether the result will be an unsus-

tainable backlash against the reforms or more constructive forms of political learning. Either way, Mexicans are likely to be presented with new ways to interpret the experiences derived from the economic crisis, and as never before, they will have opportunities to act on their opinions within the electoral arena.

APPENDIX A: DEPENDENT VARIABLES

GENERAL SUPPORT: Agreement with Economic Reform
[1992:Q24; 1994:Q31; 1995:Q28]

¿Cree usted que las medidas económicas tomadas por el gobierno son adecuadas o inadecuadas para resolver la situación económica del país? (1) Adecuadas (2) Algunas adecuadas (3) Inadecuadas

Do you think that the economic measures taken by the government are adequate or inadequate for resolving the country's economic problems? (1) Adequate (2) Some adequate (3) Inadequate

Support for Economic Reform	1992	1994	1995
Adequate	57.4%	43.8%	30.7%
Some adequate	33.2	37.9	22.2
Inadequate	9.4	18.3	47.1
Total valid responses	4,477	4,694	2,492
Don't know/No response	483	304	324
N	4,960	4,998	2,816

POLICY INDEX: Policy Index Composed of Several Components

1992 Components

Agreement with NAFTA [1992:Q4C]

¿Está usted de acuerdo con que se haya terminado la negociación de un tratado de libre comercio con los Estados Unidos y Canadá? (1) Si (2) No

Do you agree with what has been concluded in the free trade treaty with the United States and Canada? (1) Yes (2) No

Agreement with Privatization of Banks [1992:Q4A]

¿Está usted de acuerdo con que se hayan privatizado casi la totalidad de los bancos? (1) Si (2) No

Are you in favor with the privatization of almost all of the banks? (1) Yes (2) No

1992 Responses Components	NAFTA	Bank Privatization
Yes, agree	76.1	74.5%
No, disagree	23.9	25.5
Total valid responses	4,021	3,644
Don't know/No response	939	1,316
N	4,960	4,960

1994 Components

Agreement with NAFTA [1994:Q20]

Como usted sabe, desde enero de este año empezó a funcionar el Tratado de Libre Comercio con Estados Unidos y Canadá. ¿Está usted a favor o en contra de este tratado? (1) A favor (2) Da igual (3) En contra

As you know, the free trade treaty with the United States and Canada began to function in January of this year. Are you in favor or against this treaty? (1) In favor (2) Neutral (3) Against

Agreement with Privatization of Banks [1994:Q28A]

Dígame si usted está de acuerdo o en desacuerdo con las siguientes medidas realizadas por el gobierno este sexenio: Venta de bancos y de Teléfonos de México a empresarios privados. (1) Acuerdo (2) Acuerdo en parte (3) Desacuerdo

Tell me if you agree or disagree with the following steps taken by the government this presidential term: Sale of the banks and Teléfonos de México to private entrepreneurs. (1) Agree (2) Agree in part (3) Disagree

Perception that PECE (wage-price pact) is Controlling Inflation [1994:Q33]

¿Cree usted que el Pacto está ayudando a controlar la inflación? (1) Si (2) No

Do you believe that the Pact is helping to control inflation? (1) Yes (2) No

Agreement with Foreign Investment [1994:Q28C]

Dígame si usted está de acuerdo o en desacuerdo con las siguientes medidas realizadas por el gobierno este sexenio: Mayor entrada de empresas extranjeras. (1) Acuerdo (2) Acuerdo en parte (3) Desacuerdo

Tell me if you agree or disagree with the following measures taken by the government this presidential term: More access for foreign enterprises. (1) Agree (2) Agree in part (3) Disagree

1994 Responses Components	NAFTA	Privatization	Inflation Pacts	Foreign Invest.
Yes, agree	54.5%	39.3%	70.7%	42.4%
Neutral/agree in part	21.9%	18.5%	—	25.6%
No, disagree	23.6%	42.2%	29.3%	31.9%
Total valid responses	4,498	4,481	4,384	4,656
Don't know/No response	500	517	614	342
N	4,998	4,998	4,998	4,998

1995 Components

Agreement with Inflation Control [1995:Q21]

El gobierno afirma que con las medidas para enfrentar la emergencia económica se logrará evitar que la inflación se desboque, es decir, que se salga de control. ¿Usted cree que esto es cierto o falso? (1) Cierto (2) Cierto en parte (3) Falso

The government says that the measures taken to confront the economic emergency will succeed in preventing inflation from surging out of control. Do you believe this is true or false? (1) True (2) Partly true (3) False

Agreement that the PECE (wage-price pact) Controls Inflation [1995:Q32]

En el pasado gobierno, obreros, campesinos y empresarios firmaron acuerdos para controlar que no subieran mucho los precios. ¿Usted cree que estos pactos sirven para algo o no sirven para nada? (1) Si sirven (2) No sirven

In the past, the government, workers, peasants, and businessmen signed agreements to limit the rise of prices. Do you agree that those pacts were useful or useless? (1) Yes, they were useful (2) No, they were not useful

Agreement with Stabilization Program [1995:Q24]

En general, ¿está usted de acuerdo o en desacuerdo con las medidas anunciadas por el gobierno para enfrentar la crisis económica? (1) Acuerdo (2) Desacuerdo

In general, do you agree or disagree with the measures announced by the government to confront the economic crisis? (1) Agree (2) Disagree

	Components		
1995 Responses	Gov. Control Infl.	PECE Control Infl.	Stabilization Prog.
True, yes, agree	33.2%	51.7%	39.6%
Partly true	15.9	—	—
False, no, disagree	50.8	48.3	60.4
Total valid responses	2,395	2,518	2,470
Don't know/No response	421	298	346
N	2,816	2,816	2,816

The Policy Index was computed as the average of all components for that year wherein each Yes/No response was coded Yes = 2 and No = 0 and each Yes/Somewhat/No response was coded Yes = 2, Somewhat = 1, No = 0. Thus, the policy index ends up as a variable ranging between 0 and 2 (the same range as the support variable), but able to take on noninteger intermediate values depending on the set of responses. Descriptive statistics for each year follow:

Policy Index Frequency Distribution	1992	1994	1995
Respondents with:			
Low scores*	424	766	634
Medium scores*	774	1,191	999
High scores*	2,145	1,805	437
Mean	1.52	1.22	0.88
Standard deviation	0.71	0.60	0.71
Total valid responses	3,344	3,762	2,069
Missing	1,616	1,236	747
N	4,960	4,998	2,816

*"Low" scores are all those which fall in the bottom third of the index in a given year. "Medium" scores are those in the middle third. "High" scores are those in the upper third.

Missing Data and Biases in the Policy Indexes

The high percentage of missing data in the policy indexes is attributable in part to the fact that the indexes are composites which eliminate any respondent who answers "don't know" on any of the individual policy items. A comparison of valid responses and "don't knows" for the indexes showed that persons with limited education and low incomes were underrepresented in the valid responses. In 1994, for example, respondents with no more than an elementary school education comprised 37% of the total sample, 30.2% of the valid responses, and 59.0% of the nonresponses. People classified as "poor" constituted 53.4% of the sample, 48.9% of the valid responses, and 67.0% of the nonresponses. Women and private-sector workers were also underrepresented, but to a lesser extent. Biases were also either more limited or non-existent on questions concerning judgments about the economy and in our measures of political orientation. People making negative judgments about the economy or expressing opposition to the PRI or president were slightly less inclined to answer, but the differences between valid responses and nonresponses were generally between 1 and 3 percentage points.

To assess the impact of these biases, we regressed the individual policy items used to construct the indexes against the independent variables in the general model. With much lower percentages of missing data, the results of these regressions paralleled those for the policy indexes: limited impact for the demographic variables, a larger impact for economic judgments, and political orientations with the greatest effect. It should be emphasized as well that estimates in the policy index models were very similar to those for general support, which had much lower percentages of missing data.

APPENDIX B: INDEPENDENT VARIABLES

Sociodemographic

Sector: *[1992, 1994, 1995: QC]*

¿En cuál tipo de institución, organismo o empresa trabaja usted? (1) Gobierno federal, estatal o municipal (2) Empresa paraestatal u organismo descentralizado (3) Empresa privada (4) Negocio propio (5) Ejercicio o práctica independiente

In what kind of institution, agency, or enterprise do you work? (1) Federal, state, or municipal government (2) Public enterprise or decentralized agency (3) Private firm (4) Own business (5) Independent profession

[Replies are recoded: 1 + 2 = Public; 3, 4, 5 = Private]

Occupational Sector	1992	1994	1995
1. Federal, state, municipal government	19.1%	16.0%	13.4%
2. Public enterprise/ decentralized agency	4.1	2.7	2.3%
3. Private firm	38.0	37.4	32.3
4. Own business	16.8	18.2	25.6
5. Independent profession	22.0	25.6	26.4
Recoded public [1 + 2]	23.2%	18.7%	15.7%
Recoded private [3 + 4 + 5]	76.8	81.3	84.3
Total valid responses	2,931	2,808	1,490
Don't know/No response	2,029	2,190	1,326
N	4,960	4,998	2,816

Missing data. The very high percentages of respondents who could not be classified as either public or private sector employees accounts for much of the missing data in the OLS regressions in tables 4 and 5. Models estimated without inclusion of "sector" showed similar results for both general support and the policy index, however. As a further check, we ran the regressions with the nonresponses as a dummy variable (DK = 1, Response = 0). The dummy was not significant and had no impact on the other results.

Region: Region of the Country

Mexcity: Living in Mexico City?

The interviewers filled this section with the following categories: *Northeast* (Baja California, Baja California Sur, Sinaloa, Sonora): *West* (Aguascalientes, Colima, Jalisco, Michoacán, Nayarit); *North* (San Luis Potosí Zacatecas, Nuevo León, Durango, Coahuila, Chihuahua); *Gulf* (Tabasco, Tamaulipas, Veracruz); *Central* (Guanajuato, Hidalgo, México, Puebla, Querétaro, Tlaxcala); *Mexico City* (Federal District and metropolitan suburbs); *South* (Chiapas, Guerrero, Morelos, and Oaxaca); *Southeast* (Campeche, Quintana Roo, Yucatán.

Regions were in turn divided into urban, semi-urban, and rural zones, weighted according to the proportion of such zones in the region, and were divided again into neighborhoods weighted in terms of socioeconomic conditions. Blocks within neighborhoods were chosen for sampling with an equal probability of selection, and households visited were selected with systematic sampling and a random start. Urban strata were those with more than 15,000 inhabitants: semi-urban contained between 2,500 and 15,000; rural were less than 2,500. The coverage of rural and semi-rural areas distinguishes these national surveys from many partial ones that sample only urban populations.

[Categories are recoded: Mexico City—Other]

Region	1992	1994	1995
Other	81.1%	76.3%	80.0%
Mexico City	18.9	23.7	20.0
Don't know/No response	0.0	0.0	0.0
N	4,960	4,998	2,816

1990 Census (percentage of population: Mexico City, 22.2%; Other, 77.8%).

Income: *[1992, 1994, 1995: QH]*

¿Cual es su rango de ingreso familiar? (1) 0 a 1 Salarios Mínimos (SM) (2) 1 a 3 SM (3) 3 a 5 SM (4) 5 a 7 SM (5) 7 a 10 SM (6) 10 o más SM

What is the range of your family income? (1) 0 to 1 Minimum Salaries (MS) (2) 1 to 3 MS (3) 3 to 5 MS (4) 5 to 7 MS (5) 7 to 10 MS (6) 10+ SM

[Responses are recoded: 1-2 Poor: 3-4 Lower middle class: 5 Middle class: 6 Upper middle class and rich]

Salary Range	1992	1994	1995
Minimum salaries (MS)			
0 to 1	16.8%	15.7%	22.0%
1 to 3	44.9	37.7	37.2
3 to 5	22.2	23.8	16.0
5 to 7	9.8	13.4	9.2
7 to 10	3.6	5.2	6.8
10 or more	2.7	4.2	8.9
Recoded:			
Poor (0-3 MS)	61.7%	53.4%	59.2%
Lower middle class (3-5 MS)	22.2	23.8	16.0
Middle class (5-10 MS)	13.5	18.6	16.0%
Upper middle class/Rich (10+ MS)	2.7	4.2	8.9
Don't know/No response	0.0	0.0	0.0
N	4,960	4,998	2,816

Incomes in Mexico are typically calculated as multiples of "minimum salaries" which are adjusted on an annual basis. Comparable percentages from the 1990 Census are: (1) Poor, 56.3%; (2) Lower middle class, 12.8%; (3) Middle class, 12.8%; (4) Upper middle class/rich, 6.3%.

Education *[1992, 1994, 1995: QG]*

¿Hasta qué año escolar estudió usted? (1) Ninguno (2) Primaria incompleta (3) Primaria completa (4) Secundaria incompleta (5) Secundaria completa (6) Preparatoria incompleta (7) Preparatoria completa (8) Universidad incompleta (9) Universidad completa y más

What year of school did you reach? (1) None (2) Primary incomplete (3) Completed primary (4) Secondary incomplete (5) Completed secondary (6) Preparatory incomplete (7) Preparatory complete (8) Incomplete university (9) Complete university

[Recoded as: 1 None; 2 + 3 Primary; 4 + 5 Secondary; 6 + 7 Preparatory; 8 + 9 University or More]

Education	1992	1994	1995
1. None	5.1%	4.8%	6.2%
2. Primary incomplete	14.7	15.5	16.8
3. Completed primary	17.4	17.0	16.1
4. Secondary incomplete	8.2	7.8	5.8
5. Completed secondary	16.3	14.9	16.4
6. Preparatory incomplete	9.4	10.1	9.6
7. Completed preparatory	11.7	12.3	11.5
8. Incomplete university	9.5	10.0	9.0
9. Complete university	7.6	7.6	8.5
Recoded:			
None (1)	5.1%	4.8%	6.2%
Primary (2 + 3)	32.1	32.5	32.9
Secondary (4 + 5)	24.5	22.7	22.2
Preparatory (6 + 7)	21.1	22.4	21.1
University or more (8 + 9)	17.1	17.6	17.6
Don't know/No response	0	0	0
N	4,960	4,998	2,816

Distribution in 1990 Census: None: 14.3%; Elementary: 51.1%; Junior high school: 17.8%; Senior high school: 10.6%; University or more: 6.1%.

Gender

The interviewers supplied information in this category.
(1) Male (2) Female

Gender	1992	1994	1995
Male (0)	50.0%	51.4	48.9
Female (1)	50.0	48.6	51.1
Don't know/No response	0.0	0.0	0.0
N	4,960	4,998	2,816

Distribution in 1990 Census: Male: 49.1%; Female: 50.9%

Economic Opinion

Perception of Country's Economic Situation
[1992:Q7; 1994:Q15]

¿Como describiría la situación económica del pais hoy en día? (1) Muy buena (2) Buena (3) Regular (4) Mala (5) Muy mala

How would you describe the economic situation today in the country? (1) Very good (2) Good (3) So-so (4) Bad (5) Very bad

[Recoded as: 1 + 2 Good and very good: 3 So-so: 4 + 5 Bad and very bad]

Description of Economic Situation	1992	1994
Very good	5.3%	5.4%
Good	29.6	28.8
So-so	47.7	46.0
Bad	14.8	16.6
Very bad	2.5	3.3
Recoded:		
Good/very good	34.9%	34.2%
So-so	47.7	46.0
Bad/very bad	17.4	19.8
Total valid responses	4,827	4,856
Don't know/No response	133	142
N	4,960	4,998

[1995:Q4]

Comparada con el año anterior (marzo de 1994) ¿cómo cree usted que está la situación económica del país actualmente, mejor o peor? (1) Mejor (2) Igual (3) Peor

Compared with last year (March of 1994), do you think that the current economic situation of the country is better or worse? (1) Better (2) Same (3) Worse

In the following table, the recoded 1992 and 1993 results are presented for comparison with 1995.

Evaluation of Economic Situation	1992	1994	1995
Better (1995)/recoded good/very good	34.9%	34.2%	2.7%
Same (1995)/recoded so-so	47.7	46.0	7.8
Worse (1995)/recoded bad/very bad	17.4	19.8	89.6
Total valid responses	4,827	4,856	2,781
Don't know/No response	133	142	35
N	4,960	4,998	2,816

Perception of Personal Economic Situation
[1992:Q8; 1994:Q16]

¿Cómo describiría su situación económica [o la de su familia] hoy en día? (1) Muy buena (2) Buena (3) Regular (4) Mala (5) Muy mala

How would you describe your economic situation [or your family's] these days? (1) Very good (2) Good (3) So-so (4) Bad (5) Very bad

[Recoded as: 1 + 2 Very good and good; 3 So-so; 4 + 5 Bad and very bad]

Description of Personal Economic Situation	1992	1994
Very good	4.7%	5.7%
Good	29.1	30.0
So-so	49.3	46.5
Bad	14.9	15.4
Very bad	2.0	2.3
Recoded:		
Good/very good	33.7%	35.8%
So-so	49.3	46.5
Bad/very bad	16.9	17.7
Total valid responses	4,894	4,901
Don't know/No response	66	97
N	4,960	4,998

[1995:Q5]

¿Cree usted que su situación económica personal (o de su familia) es mejor o peor que hace un año (marzo 94–95)? (1) Mejor (2) Igual (3) Peor

Do you think your personal economic situation (or your family's) is better or worse than a year ago (March 94–95)? (1) Better (2) Same (3) Worse

In the following table, the recoded 1992 and 1993 results are presented for comparison with 1995.

Evaluation of Personal Situation in 1995	(1992)	(1994)	1995
Better (1995)/recoded good/very good	(33.7%)	(35.8%)	4.1%
Same (1995)/recoded so-so	(49.3)	(46.5)	17.7
Worse (1995)/recoded bad/very bad	(16.9)	(17.7)	78.2
Total valid responses	4,894	4,901	2,779
Don't know/No response	66	97	37
N	4,960	4,998	2,816

Personal Expectations for the Next Year *[1992:Q16; 1994: Q10; 1995:Q6]*

¿Cómo cree usted que será su situación personal el año que entra? (1) Mejor (2) Igual (3) Peor (4) No sabe (5) No contestó

What do you think your personal situation will be in the coming year? (1) Better (2) The same (3) Worse (4) Don't know (5) No answer

[Recoded as 1 = Better; 2 = The same; 3 = Worse; 4 + 5 = Don't know or didn't answer]

Personal Expectations for Next Year	1992	1994	1995
Better	58.2%	59.0%	24.2%
The same	29.4	24.8	15.0
Worse	12.4	16.2	60.8
Total valid responses	4,454	3,637	2,233
Don't know/No response	876	1,361	583
N	4,960	4,998	2,816

Fear of Unemployment *[1995:Q26]*

¿Tomando en consideración la situación económica actual del país, teme o no que usted o alguien de su familia pierda su empleo o trabajo este año? (1) Si (2) No

Taking the current economic situation of the country into account, are you afraid or not that you or someone in your family might be out of work this year? (1) Yes (2) No

Fear Unemployment?	1992	1994	1995
Yes, fear	—	—	73.6%
No, don't fear	—	—	26.4
Total valid responses			2,722
Don't know/No response	—	—	94
N			2,816

Political Opinion

Agreement with the President *[1992:Q29; 1994:Q41; 1995: PACU]*

En general, ¿está usted de acuerdo o en desacuerdo con la manera como está gobernando el presidente Salinas? (1) Acuerdo (2) Desacuerdo

On the whole do you agree or disagree with the way President Salinas is handling his job? (1) Agree (2) Disagree

Approval of President	1992	1994	1995
Approve	85.5%	80.9%	48.8
Disapprove	14.5	19.1	51.2
Total valid responses	4,657	4,738	2,558
Don't know/No response	303	260	258
N	4,960	4,998	2,816

Party: Electoral Preference for Presidential Election

[1992:Q3]

De los candidatos a la presidencia en las pasadas elecciones de 1988, Cuauhtémoc Cárdenas del PRD, Manuel J. Clouthier del PAN y Carlos Salinas del PRI, ¿con cuál simpatiza usted más? (1) Manuel J. Clouthier-PAN (2) Carlos Salinas-PRI (3) Cuauhtémoc Cárdenas-PRD

Of the presidential candidates in the past 1988 election. Cuauhtémoc Cárdens of the PRD, Manuel Clouthier of the PAN, and Carlos Salinas of the PRI, whom do you identify with the most? (1) Manuel J. Clouthier-PAN (2) Carlos Salinas-PRI (3) Cuauhtémoc Cárdenas-PRD

[Although not offered as a possible response, interviewers noted when respondent answered with (4) another name or specifically stated "none of these candidates."]

[Recoded as: 2 = PRI; 1 + 3 + 4 = Opposition]

[1994:Q36]

Si hoy se repitieran las elecciones para Presidente de la República y los únicos candidatos fueran el candidato del PRI, Ernesto Zedillo, el candidato del PRD, Cuauhtémoc Cárdenas y el candidato del PAN, Diego Fernández de Cevallos, por cual candidato votaría usted?

(1) Zedillo-PRI (2) Cárdenas-PRD (3) Fernández de Cevallos-PAN

If the election for president were repeated today, and the only candidates were the candidate of the PRI, Ernesto Zedillo, the candidate of the PRD, Cuauhtémoc Cárdenas, and the candidate of the PAN, Diego Fernández de Cevallos, which candidate would you vote for?

(1) Zedillo-PRI (2) Cárdenas-PRD (3) Fernández de Cevallos-PAN

[Although not offered as a possible response, interviewers noted when respondent answered with (4) another name or specifically stated "none of these candidates."]

[Recoded as: 1 = PRI; 2 + 3 + 4 = Opposition]

[1995:Q38]

De los candidatos a la presidencia en las pasadas elecciones del 21 de agosto. Diego Fernández de Cevallos del PAN, Ernesto Zedillo del PRI y Cuauhtémoc Cárdenas del PRD. ¿con cuál simpatiza usted más?

(1) Fernández-PAN (2) Zedillo-PRI (3) Cárdenas-PRD

Of the candidates for the presidency in the past elections of August 21, Diego Fernández de Cevallos of the PAN, Ernesto Zedillo of the PRI, and Cuauhtémoc Cárdenas of the PRD, with whom do you identify the most?

(1) Fernández - PAN (2) Zedillo - PRI (3) Cárdenas - PRD

[Although not offered as a possible response, interviewers noted when respondent answered with (4) another name or specifically stated "none of these candidates"]

[Recoded as: 2 = PRI; 1 + 3 + 4 = Opposition]

Favored Party/Candidate	1992	1994	1995
PAN	11.2%	19.2%	28.9%
PRI	72.5	55.9	41.8
PRD	15.2	11.7	13.5
Other or "No candidate"	1.0	13.3	15.8
Recoded: Vote PRI	72.5%	55.9%	41.8%
Recoded: Vote opposition (not PRI)	27.5	44.1	58.2
Total valid responses	4,294	4,766	2,699
Don't know/No response	666	232	117
N	4,960	4,998	2,816

Results of the August 1994 presidential elections: Zedillo (PRI): 48.8%; Fernández (PAN) 25.9%; Cárdenas (PRD) 16.6% and Cecilia Soto (PT) 2.7%.

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Democracy, War Initiation, and Victory

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How do nation-states' political institutions affect the relations between states? This article addresses that question by testing the predictions of different theories linking political institutions to war outcomes. Specifically, rent-seeking and regime legitimacy theories predict that all democratic belligerents are more likely to win wars because they fight more effectively. Alternatively, other theories focusing on the domestic political vulnerability of leaders and the marketplace of ideas predict that democracies are likely to be more careful about choosing when to start war. This would mean that only democratic initiators are more likely to win. Analyzing all interstate wars from 1816 to 1982 with a multivariate probit model, we find that democratic initiators are significantly more likely to win wars; democratic targets are also more likely to win, though the relationship is not as strong. We also find empirical support for several control variables, including strategy, terrain, and capability.

Recent empirical scholarship has noted that democracies are significantly more likely to win the wars they fight than are other types of regimes. Though this finding refutes the general realist claim that regime type is unrelated to foreign policy behavior, it also begs the important theoretical question of why democracies are more likely to win wars. Since different theories of foreign policy behavior provide different answers to this question, discerning the real reasons would advance our theoretical understanding of foreign policy behavior.

Scholars offer two different general arguments to explain democracies' winning ways. First, they are perceived as intrinsically more effective at waging war than are nondemocracies (hereafter termed the "war-fighting" explanation), primarily because it is easier for them to rally their society behind a war effort. Second, as derived from several theories, democracies win more often because they are more careful about deciding to initiate war (hereafter termed the "selection-effects" explanation). Significantly, the theoretical assumptions of the latter argument are consistent with the structural explanation of the tendency for democracies not to fight one another, popularly known as the democratic peace.

This article explores in detail why democracies are more likely to win wars, specifically comparing the war-fighting and selection-effects explanations. Our empirical tests discriminate between these explanations by exploring whether democracies are more likely to win all wars in which they participate (predicted by the war-fighting explanation) or just those they initiate (predicted by the selection-effects explanation). Our examination of all interstate wars from 1816 to 1982 finds that initiating democracies are significantly more likely to win wars than any other kind of belligerent. We also find limited support for the proposition that democracies which are attacked are more likely to win wars than are other kinds of targets. These results are in noteworthy contrast to previous empirical studies for three reasons: We use a more complete sample, account for possible interactions between democracy and initiation, and include a more complete set of control variables.

The article has four parts. The first presents the theoretical arguments relating domestic regime type to conflict behavior. The second discusses hypotheses, operationalization, and the data set. The third presents empirical results. The fourth offers conclusions.

REGIME TYPE AND CONFLICT BEHAVIOR

Are democracies more likely to win wars? Mainstream thinking during the twentieth century has occasionally doubted the ability of democracies to defend themselves, especially during the 1930s and the Cold War. Recently, however, scholars have examined the entirety of the post-Napoleonic historical record with quantitative tests and have found that when democracies fight, they tend to win, and their wars are of shorter duration (Bennett and Stam 1996; Lake 1992; Stam 1996). What we call the war-fighting and selection-effects arguments have been advanced to explain this phenomenon.

The war-fighting explanation argues that democracies simply fight wars more effectively than do other kinds of states. Lake (1992) developed a model of the state as rent-seeker and posited three reasons for democracies' superior war-fighting abilities. Democracies extract fewer rents from society, hence are wealthier and devote more resources to security. They form overwhelming countercoalitions against aggressors. Democratic governments can extract higher levels of support from society for a war effort. We discuss each of these arguments in turn; we believe the last is the most persuasive.

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The authors' names are listed alphabetically; the order does not connote principal authorship. The authors thank the following for their invaluable assistance: Scott Bennett, Bruce Bueno de Mesquita, David Davis, Christopher Gelpi, Ali Ghobadi, Joshua Goldstein, Paul Huth, Gary King, David Lake, David Leblang, Shoon Murray, James Ray, Patrick Regan, Bruce Russett, Randall Siverson, Scott Tarry, Alexander Wendt, attendees at Emory and Yale seminars on international security, and several anonymous reviewers. The authors are solely responsible for any remaining errors.

The rent-seeking argument has logical shortcomings. The model assumes that democracies are wealthier because they extract fewer rents from their society than do autocratic states, leading to greater wealth, which in turn means more resources for the military. Yet, one important reason states extract rents is to increase military spending, so fewer rents also would mean a smaller percentage of GDP devoted to military spending. Hence, the model does not necessarily predict that democracies devote more resources to military spending than do autocracies. Though the economic pie may be larger for the former, military spending is a relatively smaller slice. Indeed, according to the model, democracies prosper for that reason. In order for the rent-seeking argument to predict that democracies can generate a strong military, additional assumptions need to be made about an economic multiplier effect of reduced rent-seeking, which means that proportionately more rents are available in the long run. Logical shortcomings aside, empirical studies have found that democracy does not cause prosperity, though higher levels of economic development may make democracy more stable (Przeworski and Limongi 1997).

Although the argument that democracies form overwhelming countercoalitions contains compelling logic, even anecdotal evidence in support of this claim is scarce. Lake's (1992, 33) data included 30 wars from 1816 to 1982 in which democracies participated. In only six of these was there more than one democracy on any side, meaning that in 80% of the cases a lone democracy could attract no support from a sister regime. Of the six wars that involved a democratic coalition, four were won by the coalition (the Boxer Rebellion, the Sinai War, and the two world wars; the 1991 Gulf War also might be included), which fought for imperialist or realist motivations; democracies did not declare war primarily to come to one another's rescue.

Finally, the argument that democratic governments enjoy widespread support and therefore can extract more contributions for the war effort seems more tenable. In a study of major battles since 1800, Reiter and Stam (1998; see also Reiter and Stam 1997) found that soldiers fight better under democratic governance than under other regime types. Specifically, democratic armies fight with greater initiative and better leadership than do the armies of other kinds of states, which the authors attribute to the emphasis on individual prerogative in democratic political systems. Note that we are not arguing that democratic governments are more effective at extracting economic resources from society during wartime; empirical studies cast doubt on that particular claim (Kugler and Domke 1986).

Distinct from the war-fighting explanation is the selection-effects argument. It maintains that democracies tend to win more often because they are more careful than autocratic states about deciding whether to initiate war. Significantly, this risk aversion may account for the empirical observation that democracies are more likely to win wars (see Achen 1986). Testing for the selection effect may lead to new and significantly different conclusions about the nature of state power, as was the case in studies of deterrence (Fearon 1994) and the democratic peace (Rousseau et al. 1996).[1]

Scholars make two distinct arguments in favor of the selection-effects explanation. The first centers on democratic electoral institutions. When faced with a displeased public, democracies are generally more likely than other governments to lose power, since the latter need not hold regular, competitive elections. Moreover, autocracies are less constrained in their use of state power to suppress dissent. Therefore, democratic leaderships are more likely to lose power following a military defeat—an event quite likely to generate popular dissatisfaction—than are nondemocratic leaderships (Bueno de Mesquita and Siverson 1995; Downs and Rocke 1994). An important implication is that fear of postdefeat political consequences motivates democracies to initiate war only when they are relatively certain of victory. Consider that democratic Israel has won all the wars it has initiated (the Suez, Six-Day, and Lebanon wars). Conversely, in the 1898 Fashoda Crisis, democratic France backed down when its leadership realized the considerable military inferiority it faced against the British.

Following this logic, nondemocratic leaders are less likely to lose power after a defeat and hence are more likely to undertake risky military ventures. Saddam Hussein initiated costly wars against Iran and Kuwait but retained power. This, despite the fact that one ended in an extremely bloody stalemate, and the other conflict ended in perhaps the most decisive military defeat in modern history. The expectation, then, is that because democracies are cautious initiators, they have a higher probability of victory than nondemocratic initiators. This argument shares common theoretical assumptions with the structural explanation of the democratic peace. That is, democracies tend to be more peaceful than other kinds of states (at least in their relations with one another) because their political institutions constrain leaders from pursuing belligerent foreign policies (see Bueno de Mesquita and Lalman 1992; Russett 1993).

A second argument in support of the selection-effects explanation posits that since democratic governments benefit from more and higher quality information, they are more likely to make better policy and therefore only initiate winnable wars. The proposition that the vigorous discussion of alternatives and open dissemination of information in democratic systems produce better decisions is an idea at the core of political liberalism, traceable to thinkers such as John Milton, Thomas Jefferson, and John Stuart Mill. Specifically, a free press with limited government control or censorship is more likely to expose the flaws in

[1] One possible selection bias that may remain in our model stems from limiting our dependent variable to war and the concomitant exclusion of lower levels of violence. One may argue that in some cases the real initiation decision comes at lower levels (such as the decision to show force), so that focusing solely on the change from peace to large-scale war obscures important dynamics. We doubt that this is a problem with our model, given that the move to large-scale war is the most important and salient threshold in the escalation ladder of violence.

policy and improve government stewardship. In the modern international relations literature, scholars have posited that the relatively open marketplace of ideas engendered by a free press decreases the chances that democratic leaders will engage in foolhardy wars (Snyder 1991; Van Evera 1994, 32–3). In addition, the less politicized bureaucracies of democratic governments are more likely to generate higher quality, less biased information (see Reiter 1995b, 47–9). Specifically, in authoritarian regimes, the military is likely to provide lower quality information to the leadership. Following this logic, authoritarian governments are more likely to promote military officers based on political loyalty, as they fear potential threats to their hold on power from a disgruntled military. This toadyist phenomenon has been called "commissarism," one side effect of which is biased military intelligence, as officers are more interested in maintaining the approval of civilian leaders than in presenting sound military or strategic analysis (Pollack 1996, chapter 3). The bottom line is that since democratic leaders get better information from both society and their own bureaucracy, they are more likely to make better policy and therefore avoid starting losing wars.

Empirical work supports various aspects of these theories. As noted, both Stam (1996) and Lake (1992), while failing to control for selection effects, found that democracies are more likely to win. Siverson (1995) found that democratic initiators suffer significantly fewer casualties than other kinds of war participants. This strengthens the selection-effects argument, since higher casualties are likely to increase the domestic political threat to democratic leaders, and concerns about those repercussions may guide their war initiation decisions (see also Gartner and Segura 1998). Bennett and Stam (1996) found that democracies tend to fight shorter wars, which also is consistent with the selection-effects explanation; democratic leaders should prefer shorter wars, as they are likely to fear that public support will dissipate as a war drags on. Finally, research on the tenure of regimes following war confirms important links between the outcome and domestic politics (Bueno de Mesquita and Siverson 1995).

Empirical research, however, has not provided a test that discriminates between the war-fighting and selection-effects explanations. This is unfortunate, as these stem from very different assumptions, and more careful validation of variables could prove useful in theoretical debates. The empirical tests presented here endeavor to determine whether one, both, or neither explanation has validity.

HYPOTHESES AND DATA

Conceptual Framework and Hypotheses

Our unit of analysis is the single nation participating in war, and the dependent variable is whether that nation experienced military victory or defeat. To understand who wins wars, our conceptual framework integrates war initiation and outcomes (our ideas are closely related to those of Gartner and Siverson 1996). Assume that when a state's leadership decides whether to initiate war, it makes a subjective probability assessment that it will win (W; a number ranging from 0 to 1) and that states prefer winning to losing. Assume further that a state chooses to initiate war if W surpasses some threshold (T) value of acceptable risk. Also assume that W reflects, at least to some extent, a state's real, objective probability for victory (V). Drawing on the cost-benefit approach laid out by Stam (1996), we argue that a war ends when one side concedes defeat because it has absorbed more costs than it is willing to bear to achieve the foreseen benefits of victory. Therefore, V rises when a state's threshold of cost acceptability rises, when a state can inflict greater costs at a greater rate, and/or when it can reduce the rate at which it suffers costs.

From this set of assumptions, we deduce a number of specific hypotheses. These are presented next, along with the operationalization of independent variables.

HYPOTHESIS 1: *War initiators are more likely to win than targets.*

Since W (subjective belief that the state will win) is related to V (the objective probability of victory), we should observe that initiators are more likely to win than targets. States with a greater objective chance of victory will select themselves into the population of war participants by launching wars, whereas states with a lower objective probability of victory are selected into the population of targets for attack. The proposition that all initiators should be more likely to win wars is central to the literature on conflict behavior (Bueno de Mesquita 1981; Wang and Ray 1994). We closely equate "initiating war" with the stronger phrase "choosing war." There are very few examples of states being forced to initiate war because they feared imminent attack (the only democratic example is Israel in 1967 [Reiter 1995a]).[2] *Initiation* is a dichotomous variable, 1 for initiators and 0 for noninitiators.[3]

Our conceptual framework proposes that cautious decision making by democracies relates only to their role as initiator, not target; that is, the selection effects apply only to those which start wars, not to those which are attacked. We assume that targets must accept war when attacked. It may be argued that they can choose

[2] Some scholars disagree with the notion that states choose war, positing instead that states are usually forced into it by circumstances beyond their control. Of course, if war does not reflect a real choice, then in our data analysis the initiation terms will not be significant.

[3] Our initiation codings differ slightly from those of the Correlates of War (COW) project (Small and Singer 1982). Aside from three cases that we divide (the two world wars and the Vietnam War), we follow the arguments in Dupuy and Dupuy (1986) and Clodfelter (1992). We made two changes from the COW initiation codings. In the Crimean War, we code Russia as the initiator, not Turkey. In the First Balkan War, we code the three members of the Balkan League as coinitiators.

One also may argue that states which join wars are conceptually similar to those which initiate wars. We tested for this possibility. When we recoded the initiation variable to include states that join because of a defense pact with a belligerent or because an ongoing war threatens their national interests, the results were largely the same, though substantive and statistical significance were weaker.

not to fight, but if this were the case, then perhaps the selection-effects explanation would imply that democratic targets are also more likely to win than are nondemocratic targets. Weak democratic targets would be more likely to accept peaceful surrender over military defeat, as Denmark did in 1940 when Nazi troops crossed the border.

The assumption that targets must fight does not undermine our findings. Our empirical tests will allow us to discern whether targets have the range of choice available to initiators. If so, then we should find that initiators—democratic or otherwise—have no greater chance of winning than targets. Regardless, there are almost no cases of peaceful surrender aside from Denmark, though there are several cases of small targets electing to fight when attacked, even against overwhelming odds, such as all the other small European neutrals in World War II. Leaders tend to place a higher utility on defeat in battle than on peaceful surrender for three reasons. First, abject surrender probably would end their personal political career, while choosing to fight vainly would permit the possibility of postwar leadership. Second, honor supports power, as a country that allows itself to be conquered peacefully brands itself for decades to come as a feckless weakling to be bullied (Kagan 1995, 8–9). Third, leaders treat the choice to defend one's own land as an insubvertible good, and honor in defeat is intrinsically preferable to ignominious surrender. Consider Belgium's choice at the outbreak of World War I. In the first days of the war, Germany presented an ultimatum: Allow us to move peacefully through your territory to reach France, and we will honor Belgian sovereignty after the war and recompense any damages; oppose such movement, and face destruction. Faced with a relatively attractive offer and the prospect of military annihilation, the Belgians still fought (Tuchman 1962, 124).

HYPOTHESIS 2: *Democratic targets are more likely than other kinds of targets to win wars.*

HYPOTHESIS 3: *Democratic initiators are more likely than other kinds of initiators to win wars.*

The war-fighting explanation posits that all democratic war participants, initiators and targets, win more frequently because they fight more effectively. In other words, within the context of our framework, V is likely to be higher for democracies. The war-fighting explanation makes no assumptions about W or T varying among states. The straightforward implication is that all belligerents—both initiators and targets—are more likely to win if they are democratic. The war-fighting explanation implies both hypotheses 2 and 3.

As discussed previously, the selection-effects explanation proposes that, because democratic leaderships fear being turned out if a war is lost, they are likely to initiate only those wars which they are confident of winning. Within the context of our framework, this implies that T will be systematically higher for them than for leaders in nondemocracies. The selection-effects theory predicts that democracies will only initiate war when W is high. We also expect that their estimates of W will closely approximate V. If democracies grossly overestimate their chances of victory ($W >> V$), then we would fail to observe the hypothesized relationship between democratic war initiation and victory. The selection-effects explanation implies only Hypothesis 3, not Hypothesis 2.

HYPOTHESIS 3a: *Among initiators, democracies are the most likely to win, followed by dictatorships, followed by mixed regimes.*

Hypothesis 3 implies a monotonic relationship between regime type and outcomes for initiators: The more democratic they are, the more likely they are to win. An alternative approach is to relax the monotonicity assumption. The study of imperial overexpansion by Snyder (1991, 21–65; for a different theory relaxing the monotonicity assumption, see Goemans 1997) suggests that there may be a more complex relationship. Following Snyder's theory, democracies are still most likely to initiate only wars they believe winnable, owing to the prospects for postdefeat political punishment and the benefits of the marketplace of ideas. Dictatorships should be more risk acceptant, since dictators need not fear the political fallout from losing and do not benefit from a marketplace of ideas, but some dictators may have the ability to make clear-sighted foreign policy, and thus avoid starting unwinnable wars. Therefore, it is mixed regimes that are especially likely to start misguided military ventures, because their corporate or oligarchic political structure engenders logrolling and militarist mythmaking.

To test hypotheses 2, 3, and 3a, we use democracy and autocracy scores from the Polity III data set (Jaggers and Gurr 1995). These scores are aggregations of the degree of system openness, the degree of participation, and the degree of competitive candidate selection. The democracy scale ranges from 0 to 10 (0 = absence of democratic institutions; 10 = very strong democratic institutions). The autocracy scale also ranges from 0 to 10 (10 = most autocratic). We combine the two into a single *Politics* index by subtracting the autocracy scale from the democracy scale, which results in a 20-point scale, with +10 representing the most democratic states and −10 the least. Some countries that were invaded and conquered in the same year (such as Norway in 1940) are coded by Polity III as having missing democracy and autocracy scores. For these cases, we used the scores in the previous year, as the "missing" scores reflect the lack of a normal political system during occupation; these countries were functioning polities prior to invasion and during the war, which means that our hypotheses can make predictions about how their political system affected war outcomes.

To test our various hypotheses linking regime type and war outcomes, we used two functional forms, each with a different set of interaction terms. In one functional form, we added two interaction terms: (1) a state's Politics score times the dichotomous initiation variable and (2) a state's Politics score times a dichotomous target variable, which is coded 1 if the state is a

target (not an initiator) and 0 if it is not a target (an initiator). Hypothesis 2 predicts that the *Politics*Target* term will be positive, and Hypothesis 3 predicts that the *Politics*Initiation* term will be positive. The war-fighting explanation predicts that both terms will be significant, while the simpler, monotonic selection-effects explanation predicts that only the Politics*Initiation term will be significant. Inclusion of an interaction term is a new approach, since Lake (1992) considered initiation and democracy as independent variables.[4]

Testing Hypothesis 3a, which envisions a nonmonotonic selection-effects relationship, requires a different functional form for the independent variables. To test for the expected curvilinear relationship in the Politics*Initiation term, we used a pair of fractional polynomials (see Royston and Altman 1994). To compare the curvilinear selection-effects explanation to the war-fighting explanation we then need three terms. The first is a state's politics score multiplied by a dichotomous target variable, which is coded 1 if a state is a target (meaning it is *not* an initiator) and 0 if a state is not a target (meaning it *is* an initiator). There are also two terms of a transformed independent variable: The transformation for the first term is $x^{-1/2}$ and for the second term is $x^{-1/2}(\ln(x))$, where x = (Politics*Initiation + 11)/10. This transformation is a relatively simple fractional polynomial (e.g., we could have squared or cubed x, but doing so gains little, either substantively or statistically). The polynomial specification allows for a curve that starts at a medium level for a politics score of −10, dips down to a lower level as the scores increases, but then rises to a higher level as it approaches +10. With this alternate functional form, the democratic selection effects are supported in the curvilinear manner predicted by Hypothesis 3a if the transformed independent variables are statistically significant. Due to the complex nature of the transformation, we will leave interpretation of the signs of the coefficients to the results section. Among targets, democracies have an advantage (an effect predicted by Hypothesis 2) if the Politics*Target interaction term is positive and significant.

In contrast, realists are generally skeptical about a relationship between domestic political structure and international conflict, a point often made in critiques of the democratic peace proposition (see Mearsheimer 1990). In our framework, there would be no relationship between *W, T,* or *V* and a state's level of democracy. This implies that regime type should be unrelated to war outcomes (see Wright 1965, 163). Morgenthau (1978, 143–6) did argue that the quality of government and national character or morale are important factors in state power, though he did not believe that these are systematically related to whether the state is democratic. Realism predicts that we will not find either substantive or statistical support for hypotheses 2, 3, and 3a.

[4] Lake (1992, 36n) seems to have checked for a possible democracy-initiation interaction effect, but he only mentions briefly that he found no significant relationship.

Control Variables

A variety of factors unrelated to regime type or initiation should systematically influence the objective probability of victory in war *(V)*. Realism proposes that these variables provide a virtually complete explanation of war outcomes. Though our main theoretical interest is to trace the effect of political structure on war outcomes, excluding these factors from the analysis would risk biasing our results, which in turn could lead to incorrect inferences about the relationship between political structure and war outcomes. Our choice of control variables is driven by theory and empirical research on the determinants of war outcomes (e.g., Stam 1996).

HYPOTHESIS 4: *The greater a state's relative advantage in material capabilities, the greater are its chances of winning a war.*

As a state's advantage in military-industrial capabilities rises, its chances for victory rise, since it will be able to inflict greater costs upon its opponent relative to the costs it will suffer. We operationalize this variable as each actor's *Capabilities* as a proportion of all participants' capabilities. We use the COW composite capabilities index as our indicator. It is composed of industrial production, military troops, military spending, energy consumption, and proportion of urban to total population.[5]

HYPOTHESIS 5: *States with a military of higher quality will have a greater chance of winning.*

Since militaries are the tools of war, higher quality militaries ought to increase the chances of victory (Van Creveld 1991). Specifically, superior military technology and training should translate into fighting forces that can more efficiently inflict costs on the opponent (Arquilla 1992, 81–5). We used an indicator that can be measured by ex ante information. We assume that states with a higher *Quality Ratio* (spending rate per soldier, relative to their opponents) will have a correspondingly higher level of technology, equipment, and training (Huth, Bennett, and Gelpi 1992; Stam 1996). The military spending and troop numbers come from the COW data set.

HYPOTHESIS 6: *The better a state's choice of military strategy, the greater are its chances of winning a war.*

Strategy can have a major effect on a state's ability to absorb and inflict costs and hence on the prospects for victory. We categorize military strategies as maneuver, attrition, and punishment (see Mearsheimer 1983; Posen 1984; Stam 1996). We then match each with either offensive or defensive political goals, or doctrine. It is important to note that the effectiveness of a particular strategy also depends on the adversary's choice of strategy. We predict the effectiveness of various combinations based on two assumptions: Min-

[5] We do not discount nations' material contributions based on when they joined the war. The inevitable imprecision and inaccuracies of any discount formula would outweigh any reduction in bias it might achieve.

imizing one's own military losses is better, and the side trying to change the status quo prefers quicker outcomes (see Stam 1996).

We coded military strategy as a set of dummy variables following Stam's (1996) procedures. The first step is to determine whether a state has offensive (*O*) or defensive (*D*) political goals, based on Dupuy and Dupuy (1986) and Holsti (1991). Next, we coded the choice of maneuver (*M*), attrition (*A*), or punishment (*P*) strategies, following the analysis in Dupuy and Dupuy (1986), Dupuy (1983), and Clodfelter (1992). We coded blitzkrieg strategy, or encircling and dividing the opponent's forces, as maneuver. A focus on annihilation of the enemy and/or seeking decisive battles was coded attrition. Cases in which Maoist guerrilla tactics were used or in which civilians were the principal military targets were coded punishment. If multiple strategies were employed, then we chose the one that absorbed the majority of the state's military assets. If there was more than one country on a side, then we coded the strategy of the largest state (in terms of capabilities). In the three cases of no clear distinction between maneuver and attrition, we coded the modal strategy of attrition (see Bennett and Stam 1996, 247n).

For each belligerent, we code its goals and strategy, and those of its opponent with a four-letter code. For example, OADP indicates that one party has offensive goals and an attrition strategy, and the other has a defensive doctrine and punishment strategy. We also grouped all possible strategy codings into pairs and ranked the effectiveness of each pair. Not all possible strategies are observed, however. Among those that are observed, we rank the strategy pairs as follows starting with the most effective: OMDA/DPOA; OPDA/DMOA; OADA/DAOA; OADM/DAOP; OADP/DAOM. Five dummy variables are used to account for the observed strategy combinations. To avoid perfect multicolinearity among the strategy categories, we omitted from the regression models the dummy category representing OADP/DAOM (for more discussion, see Stam 1996).

Our strategy variables are certainly simplifications of reality, but the general aim of most military theorists is to propose concepts about the execution of combat that transcend a particular military environment. The essential issue is whether our conceptualizations explain more than they obscure. We believe so, if for no other reason than that this set of strategy variables has been found to be significantly correlated with both war outcomes and duration (Bennett and Stam 1998).

Our model also simplifies reality by coding each belligerent as having a single strategy throughout the war (see Gartner 1997). Fortunately, there are very few cases in which belligerents made strategy changes from one of our categories to another. We reduce the potential problem of intrawar strategy change by breaking the two world wars and the Vietnam War into shorter components (discussed in detail below).

HYPOTHESIS 7: *Appropriate terrain will increase a state's chances of victory.*

Terrain significantly enhances or diminishes the effectiveness of a strategy choice. For example, the maneuver strategy of one side can better exploit the attrition strategy of the other side on flat plains than in mountainous jungle. Terrain codings come from *New York Times Atlas of the World* (1983) and correspond to the location of the majority of battles fought during a war (Dupuy and Dupuy 1986). We then scaled the terrain types to match the predicted movement times, using data from Dupuy (1979, 1983) that estimate movement speed over various terrains. In cases involving more than two actors on one side, we used the average of terrain scores weighted by the size of the forces fighting in particular terrain. The final terrain index ranges from 0.3 to 1.2; 1.0 corresponds to the speed at which vehicles and troops can move on open rolling terrain, similar to the plains of Eastern Europe. Higher scores correspond to desert areas with flat, hard-packed surfaces. A score close to 0.3 indicates very difficult movement for vehicles, such as rugged mountains and dense jungles.

Our model includes two terrain variables, a *Strategy-Terrain* interaction variable and, as a control, the terrain variable alone. To measure the interaction term, we would normally multiply the two variables. Unfortunately, with dummy variables to control for strategy choice, the inclusion of each terrain interaction dummy would create multicolinearity problems with the strategy dummy variables. Instead, we used a scaled strategy index with terrain, which reduces multicolinearity (as revealed by partial correlations) by creating a single variable with a larger range of values rather than several variables with a more limited range. This scaled strategy variable is coded 1 through 5, a ranking of the observed strategies listed above.[6]

HYPOTHESIS 8: *The greater the distance between the war participant and the site of combat, the lower are the chances of victory.*

Several authors hypothesize a correlation between *Distance* and the chances of victory (Stam 1996). Bueno de Mesquita (1981, 41) argues that a state's ability to inflict costs or exert power declines over distance, which may affect a state's ability to raise the costs of war for an opponent or minimize its own costs. We measure distances in miles from capital to capital, using Fitzpatrick and Modlin (1986) and *New York Times Atlas of the World* (1983).

HYPOTHESIS 9: *The more powerful a war participant's allies, the greater are its chances of winning.*

Alliance Contributions are at the core of the realist vision of international relations and waging war (Morgenthau 1978). A state's allies increase the costs its opponents must bear and reduce its own costs. Yet, the contributions to a state's power from its allies must be

[6] We score the most effective offense and defense strategies (OMDA and DPOA) 1, the next most effective 2, and so forth, to the least effective strategies (OADP and DAOM), which we code as 5. Including the scale or individually multiplied dummy variables makes little difference in the overall results of the model, as the fit remains nearly identical.

discounted compared to the state's own military and industrial capabilities. At the operational level, it is likely that differences in factors such as command structure, technology, training, and communication equipment may hinder effective coordination. Allies also have to defend themselves and will reserve some of their forces for that purpose. We thus separate out alliance contributions from the resources of the belligerents. Alliance contributions are measured in the same fashion as the state's military-industrial capability, using the COW data, that is, as a fraction of the total capabilities of all states involved in the war. Because we have no discount formula for allies, we include these contributions as a separate term to permit measurement of their independent effect.

Data Set

Our population for analysis is all participants in interstate wars between 1816 and 1982.[7] They were identified by the COW set of interstate wars (Small and Singer 1982) and Dupuy and Dupuy (1986). The hypotheses we test concerning polity type relate only to the likelihood of victory. Accordingly, we simplified the analysis by dropping draws from the data set, leaving 197 cases.[8]

Unlike the COW data set, ours disaggregates World War I and II and the Vietnam War into multiple wars. Our goals were to distinguish between separate military campaigns (for example, the German campaign against France in 1940 and against the Soviet Union) and to err on the side of conservatism, that is, to create fewer rather than more new wars. World War I we divide into three campaigns: Germany versus Belgium, Germany/Austria/Turkey versus Russia, and the Allied Powers (minus Russia and Belgium) versus the Central Powers (see Edmonds 1951). World War II we divide into several campaigns: Germany versus individual European countries as separate wars, Germany versus the Soviet Union, the Pacific War, and the United States and Great Britain versus Germany and Italy. The Vietnam War we divide into the United States and South Vietnam versus North Vietnam, a phase lasting until 1972, and North versus South Vietnam.

This separation makes our data set more historically accurate; these long coalition wars can be thought of as aggregations of distinct conflicts. Decision makers rarely anticipate or think in terms of large systems of wars, but usually in terms of sequences of opponents. In the case of World War II, Dupuy and Dupuy (1986), among many others, identify separate and distinct negotiated settlements among the various participants as well as fundamentally independent war-fighting efforts.[9] Disaggregation also affords two important advantages for testing the hypotheses. First, it permits more accurate distinctions between winners and losers. In long coalition wars, members of the losing coalition may score distinct campaign victories against members of the ultimately victorious coalition. For example, our method permits us to classify Russia and Belgium as losers in World War I and Poland and Norway as losers in World War II, whereas the COW data set views these four countries as winners. Conversely, our method recognizes the execution of successful military campaigns by countries that went on to lose, such as Germany's campaign against the Netherlands in World War II. Second, disaggregation allows us to code the independent variables more accurately. Dividing long wars of global reach into distinct campaigns greatly improves coding accuracy for such variables as strategy and terrain.[10] This advantage may not be trivial; one scholar found that how one treats cases from the two world wars determines whether one finds significant correlations between military buildups and the outbreak of war (Diehl 1983).

EMPIRICAL RESULTS

We first present a simple contingency table listing victory-defeat records for states classified by the politics index and initiation. A war participant was coded democratic if its politics score was +7 or higher, mixed regime or "anocratic" if the score was −6 to +6, and autocratic if the score was −7 or lower. In Table 1,

[7] Some may argue that it would be better to focus on the core dyad in each war, but we include all belligerents for two reasons. First, if the dependent variable is war outcomes, then there is no theoretical justification for excluding them. For war initiation, it may be more important to think about the primary disputants, but membership in the core dyad is less relevant to performance during the war. Second, including only the core dyad would leave out relevant information. We enhance confidence in the results if we broaden the empirical sample. Furthermore, note that model 6 in Table 2 includes only core dyads, and the results are largely the same.

[8] Analyzing the data with draws included does not substantially change the results. One variable that we eventually dropped from the analysis, geographic distance, is statistically significant if we include draws. States fighting in distant wars are more likely to settle for draws than states fighting near their borders.

In addition, we note that our data set differs from Lake's. From the COW list, Lake (1992, 31; 33, 35n) used only wars in which at least one democracy participated, minus the Spanish-American War and wars in which there was no clear victor. Lake also restricted the size of his sample by using a version of the COW data set that extends only to 1965 (20 wars). We believe our sample is richer and likely to contain fewer sources of bias; it covers up to 1982 (77 wars), includes the Spanish-American War, and does not exclude wars in which no democracy participated. The results do not change significantly if we only use cases up to 1965, though standard errors do increase somewhat. Finally, Lake did not disaggregate the two world wars and the Vietnam War, as we did.

[9] The results were quite similar even when we omitted our disaggregation; because that reduced seventeen wars to three, *all* the standard errors increased slightly. The coefficient estimates were stable, though the strategy and terrain variables could not be estimated because of insufficient variance among the independent variables.

[10] The COW codings are based, in part, on an attempt to generate a data set useful for testing propositions about systemic wars and the mechanism through which bilateral wars become multilateral. The COW outcome codings do not attempt necessarily to reflect the outcome of waging war. For example, while Germany clearly defeated Poland in World War II, the Polish government in exile remained allied to the U.S. and British war effort. Consequently, COW codes Poland as being on the winning side, although they were defeated in battle. We are interested in the process of waging war, so we treat each war separately; they are aggregated into multilateral wars only if that was the way the battles were actually waged. The 1973 Arab-Israeli conflict is an example of a multilateral war we did not disaggregate.

TABLE 1. War Outcomes by State Type and Initiation

Outcome	Democracy	Anocracy	Autocracy	Total
Initiators				
Win	14	21	21	56
Lose	1	15	14	30
Targets				
Win	12	18	16	46
Lose	7	27	31	65
Total	34	81	82	197

χ^2 (6) = 12.7. p = .048.

entries are victory-defeat records. The probit analysis, which follows, uses the −10 to +10 politics scores.

As a first and somewhat crude cut, Table 1 indicates preliminary support for hypotheses 1, 2, and 3—our baseline model. There is no apparent support for Hypothesis 3a, that is, a curvilinear relationship between regime type and victory. Initiators and democracies appear to be more likely to win, and democratic initiators appear to be especially likely to win. The general results are statistically significant at the $p < .05$ level.

While interesting, the results in Table 1 may also be an artifact of correlations with other determinants of victory. For example, democratic war initiators may have the most powerful armies. To test for this and other possible factors, we used a more fully specified, multivariate model. A probit model was chosen because of the dichotomous nature of the dependent variable. Applying a linear model would risk introducing specification error and biased coefficients. Table 2 presents the probit regression results of several models.[11]

In model 1, our baseline, we include terms for the interaction between the politics variable and both initiators and targets. We also include a variable marking whether the state was an initiator. Note that in this model we cannot test the nonmonotonic Hypothesis 3a, which is addressed below. Regardless, concerning the effects of democracy, the Politics*Initiation term is in the hypothesized direction and statistically significant.[12] We also see that democratic targets appear to perform somewhat better during war, and we find support for the selection-effects argument. Yet, with this simple model we cannot ascertain the individual validity of the rent-seeking, democratic countercoalitions, or state legitimacy arguments.

To test Hypothesis 3a (a nonmonotonic relationship among politics, initiation, and victory), we also fit a fractional polynomial model. In model 2, we find similar results for the initiation term. The parameter estimate for the Politics*Target term is relatively stable, although it is no longer statistically significant. In this arguably incomplete model of outcomes the fractional polynomial terms (interacted with initiation) are not supported—leading to preliminary rejection of Hypothesis 3a—but it is possible that including additional data may allow for less biased estimates of the polynomial terms. Recall Lake's claim that democracies should be more likely to win because they devote more resources to the military and assemble overwhelming countercoalitions, and it is these variables that should determine war outcomes. This implies that if we control for these factors, then the effects of democracy may drop out of the equation. First, we tested our model of military factors absent the domestic politics variables. Model 3 might best be termed a realist model of war outcomes. It is devoid of state polity characteristics and includes variables marking military-industrial capability, alliance contributions, and military quality as well as several markers for strategy and strategy-terrain interactions. In general, we find strong support for this realist approach.

Next, in model 4, we include the regime type variables from model 1, and all the politics terms from our base model are still statistically significant. This increases our confidence in the validity of the selection-effects explanation. It also implies that democratic targets tend to be more powerful, which is consistent with all three of the arguments underlying the war-fighting explanation. In model 4, however, we have controlled for alliances and industrial capability, two of the factors that Lake (1992) highlights in his presentation of the war-fighting explanation. We still observe a relationship between democracy and war outcomes; indeed, it appears both substantively and statistically stronger, not weaker, when we add the realist controls. This seems to support Lake's empirical generalization, but not for all the reasons he outlined. Though there is no direct evidence here, the results do not contradict the validity of the state legitimacy proposition. In addition, we ran tests to see whether adding political variables in model 4 makes a statistically significant improvement to our explanation of the dependent variable. A likelihood ratio test demonstrates that the gains in model 4 over model 3 are statistically significant at the $p < .01$ level.

In model 5 we substitute two fractional polynomial terms for the Politics*Initiation interaction. Unlike model 2, the inclusion of a more fully specified model of outcomes indicates some support for the nonmonotonic Hypothesis 3a. Note that although the interaction terms in both models 4 and 5 are statistically significant, model 5 is preferable to model 4. The improvement in the fit of the overall model from the inclusion of two fractional polynomial terms, compared to the linear term, is significant using a conservative test at the $p < .02$ level.[13] This encourages preference for the

[11] We also fit the same models to the logit distribution. In these models, the parameter estimates were essentially the same; the standard errors increased somewhat. In the baseline model with the logit estimates, the democratic target term did not meet standard levels of significance (p = .052).

[12] Models 1 and 2 do not include an estimate for a constant. When included, the absolute values of the standard errors exceeded the estimate of the constant itself. In runs with a constant estimated, the coefficients for all variables remained stable, with the p-value for all remaining below 0.05 (one-tailed tests).

[13] To compare the fit of two (or more) fractional polynomial models, we compared the deviance (D) of the two. The deviance is defined as −2 times its minimized log likelihood, using the formula $D=$

TABLE 2. Probit Models of War Outcomes: Dependent Variable Is Win/Lose

	Model 1		Model 2		Model 3	
Independent Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Poly-Pol 1*Initiation	—	—	−0.16	0.13	—	—
Poly-Pol 2*Initiation	—	—	0.013	0.075	—	—
Politics*Initiation	0.045*	0.021	—	—	—	—
Politics*Target	0.038*	0.017	0.029	0.019	—	—
Initiation	0.51***	0.15	0.64**	0.21	—	—
Capabilities	—	—	—	—	3.33***	0.70
Alliance Contributions	—	—	—	—	3.85***	0.95
Quality Ratio	—	—	—	—	0.082*	0.05
Terrain	—	—	—	—	−9.00***	3.36
Strategy*Terrain	—	—	—	—	2.86***	1.09
Strategy 1	—	—	—	—	5.14*	3.40
Strategy 2	—	—	—	—	2.72	2.39
Strategy 3	—	—	—	—	2.47*	1.70
Strategy 4	—	—	—	—	2.09*	1.27
Constant	—	—	—	—	−4.11***	1.86
	LL = −128.1		LL = −128.9		LL = −74.9	
	Pseudo R^2 = 0.07		Pseudo R^2 = 0.07		Pseudo R^2 = 0.45	
	N = 197		N = 197		N = 197	

	Model 4		Model 5		Model 6	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Poly-Pol 1*Initiation	—	—	−3.67*	1.88	−3.90*	1.98
Poly-Pol 2*Initiation	—	—	−1.07*	0.57	−1.14*	0.60
Politics*Initiation	0.068*	0.030	—	—	—	—
Politics*Target	0.064**	0.027	0.064*	0.03	0.059	0.039
Initiation	0.91**	0.34	0.96**	0.35	1.24**	0.41
Capabilities	3.73***	0.52	3.76***	0.53	3.85***	0.60
Alliance Contributions	4.72***	0.68	4.71***	0.68	4.48***	0.78
Quality Ratio	0.052	0.03	0.051	0.03	0.055*	0.03
Terrain	−10.93***	2.94	−11.31***	3.02	−14.1***	3.31
Strategy*Terrain	3.56***	0.97	3.68***	0.99	4.73***	1.10
Strategy 1	7.24**	2.89	7.62**	2.93	10.5**	3.13
Strategy 2	3.47*	1.99	3.76*	2.02	6.06**	2.15
Strategy 3	3.35**	1.43	3.57*	1.45	4.91**	1.55
Strategy 4	3.07**	1.25	3.21*	1.26	5.23**	1.59
Constant	−5.51***	1.70	−2.14	2.49	−3.76	2.77
	LL = −64.9		LL = −64.9		LL = −50.2	
	Pseudo R^2 = 0.52		Pseudo R^2 = 0.52		Pseudo R^2 = 0.57	
	N = 197		N = 197		N = 168	

Note: All estimates were generated using Stata 5.0. Reported standard errors are robust standard errors. Data are available currently from the authors at http://www.yale.edu/plsc151a/initiation.zip. Model 6 includes only core dyads. *$p < .05$. **$p < .01$, ***$p < .001$. All tests are one-tailed.

curvilinear selection effect described in Hypothesis 3a over the monotonic selection effect described in Hypothesis 3. We limit the remainder of our discussion to the estimates for model 5, which tests all hypotheses except Hypothesis 8, which we discuss below.

As shown in model 5, the coefficient estimate for initiation is statistically significant, indicating that Hypothesis 1 (initiators win more often) is supported. The coefficient estimates for the two Politics*Initiation interaction terms are also statistically significant.[14] This provides support for hypotheses 3 and 3a—democratic initiators win more often, and dictatorships are more likely to win than anocracies (we discuss the meaning and the interpretation of the signs and magnitudes of the interaction terms below). This is an interesting result; the conventional belief in the rational choice literature that initiators are more likely to win wars

$n\left(1 + log\frac{2\pi RSS}{n}\right)$, where n is the sample size and RSS is the residual sum of squares estimated fitting a linear model. The gain (G) is defined as the deviance estimated for a straight-line model minus the deviance estimated for the fractional polynomial model. The test statistic to calculate the p-value for the gain for the polynomial probit model is obtained using the formula $T = D_{k-1} - D_k$. We then compared T to the χ^2 distribution with 2 df. These p-values are approximate and conservative (for an extensive discussion of fractional polynomial models, see Royston and Altman 1994).

[14] In model 5, the constant's standard error is larger than the parameter estimate. When we suppress the constant, in the resulting model the other parameter estimates are stable, but the standard errors shrink substantially. In this model (not shown) the Politics*Initiation terms are significant at the $p < .001$, the Politics*Target and Initiation terms at $p < .005$. We used these more precise estimates for the deviance test outlined in footnote 13.

receives confirmation. But, even when we control for the general increase in the likelihood of victory for all initiators, some initiators do better than others. Democracies tend to have a higher likelihood of winning than all other kinds of regimes.

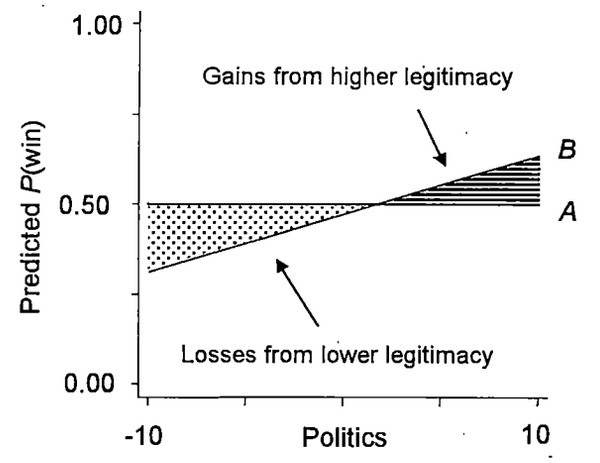
Among the control variables, the findings are mixed. The probit models in Table 2 are restricted models. They reflect our decision to drop two independent variables from models not presented in Table 2; one (military personnel) initially was included because Lake did so, and the other (distance) was included to test Hypothesis 8. The p-values for these variables fall far outside the standard levels of statistical significance. In addition, a likelihood ratio test indicated that we can safely exclude these two variables from the model. When including distance and military personnel, the results are consistent with models 4 and 5.

We find statistically significant relationships between winning and a better ratio of military-industrial capabilities and greater allied contributions, which supports hypotheses 4 and 5. The results for Hypothesis 9 (superior troops and training contribute to victory) are mixed. We leave it to future research using more sophisticated measures of technology and training to sort out this issue. Distance was not significantly related to outcomes, so we can reject Hypothesis 8.[15] The strategy dummy variables are statistically significant, indicating that strategy choices do have systematic effects on outcomes. In addition, both the terrain and strategy-terrain interaction are statistically significant ($p < .001$). Strategy dummy variables 2 and 3, while having essentially the same coefficients, still predict different outcomes because of the effects of the strategy-terrain interaction. Hence, we find strong support for hypotheses 6 and 7.

One possible objection is that our results are biased by the inclusion of war participants other than the core dyad of belligerents (see footnote 7). To be sure that our results are robust, we also estimated model 6, which includes all the variables from model 5 but on a smaller sample, that is, the core dyad in each war (n = 168 versus n = 197).[16] In this model, the results do not change substantially.

The issue of substantive explanatory power remains: How much of an effect do the statistically significant variables have on the dependent variable? We use figures to portray the effects of politics on the dependent variable.

FIGURE 1. Probability of Victory by Level of "Politics" among Target States



Note: A higher "Politics" score indicates a more democratic state. Null (*A*) is set at 0.50, 41% of all targets actually won. Line *B* represents *P*(Win).

In Figure 1, we demonstrate, ceteris paribus, the substantive effect of changing levels of democracy on war outcomes for targets.[17] Note that the darkest area, which represents the potential gains from democracy compared to the null, shows a positive but small effect. When we compare dictatorships to highly democratic states (the lightly shaded area), the apparent effect is quite large. Recall that there are several explanations of this effect, most notably Lake's (1992) rent-seeking theory and the legitimacy argument. Note that we still observe effects for democratic targets even when we control for industrial capability and alliance contributions, two factors emphasized by Lake's rents argument. Indirectly, then, these findings point toward the legitimacy argument made by Lake (1992) and Stam (1996) as the most likely explanation, in concurrence with the empirical findings of Reiter and Stam (1998). What of the selection-effects argument?

Figure 2 represents the changes in the expected chance of victory for an initiating state as its politics score varies from −10 to +10.[18] Here we see that the selection effect has substantive as well as statistical significance.[19] States appear to become increasingly

[15] We also checked for the possibility that the distance term would have an effect only as an interactive variable with military and industrial capability. This hypothesis was not supported. We also estimated a model using capability scores adjusted for distance according to the procedures in Bueno de Mesquita (1981); again, the hypothesis was not supported.

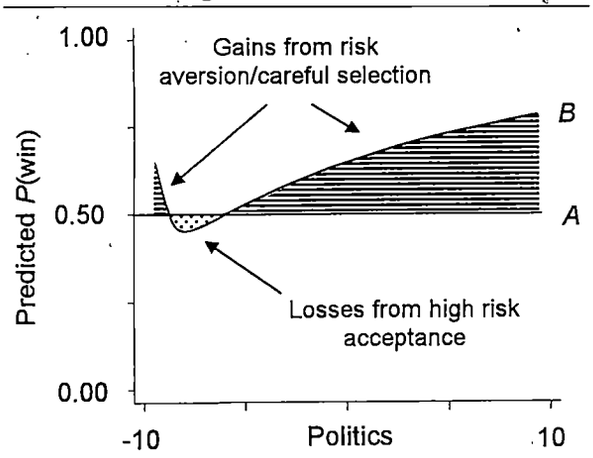
[16] To estimate the core dyad model we reran our basic model, but with certain exclusions: Austro-Sardinian War (Tuscany and Modena); Roman Republic (France); Crimean (France, Great Britain, and Italy); Italian Unification (France); Lopez (Argentina); Seven Weeks (Hanover); Franco-Prussian (Baden, Bavaria, and Wurtemberg); Pacific (Bolivia); Second Balkan (Romania and Turkey); World War II in Western Europe (Italy and United States); Yom Kippur II (Jordan and Saudi Arabia); Ethiopia-Somalia (Cuba); World War Ib (Turkey); and World War Ic (Austria, Bulgaria, Great Britain, Greece, Italy, Rumania, Turkey, and United States). We also estimated a model, not in the table, that dropped all cases but initiators. In this case, politics was still associated with greater chances of victory (coefficient 0.044, SE 0.022, $p < 0.05$).

[17] We used the parameter estimates from model 5 to estimate the predicted values for the plots. The vertical axis is the predicted probability of victory. Line A represents the null hypothesis: Democracy has no effect on outcomes. Line B represents the increase in the expected likelihood of victory as a state's Politics score rises from −10 to +10 while holding all other variables at their observed values.

[18] Line A again represents the null hypothesis of no democratic selection effect. Curve B represents the expected change in a state's probably of victory (*V*) as the Politics*Initiation term varies from minimum to maximum. We used the parameter estimates from model 5 to estimate the outcome probabilities.

[19] A simple interpretation of Table 2 might indicate that the political variables are not particularly important for a general understanding

FIGURE 2. Probability of Victory by Level of "Politics" among Initiator States



Note: A higher "Politics" score indicates a more democratic state. Null (*A*) is set at 0.50, 64% of all initiators actually won. Line *B* represents *P*(Win).

prudent, or risk averse, as they become more democratic; and the effect appears to be quite substantial. Highly democratic states appear to be quite unwilling to initiate war except under the most propitious conditions. Keep in mind that Figure 2 represents the political component of the selection-effects argument, as we have controlled for both the operational advantages of initiation and the other components of state power. Note also, at the far left of the graph, that the most autocratic states, while not doing as well as democracies, do substantially better than their slightly more moderate associates. This lends support to Snyder's argument that some totalitarian states may be able to gauge the chances for success more accurately than mixed regimes (anocracies).

Our results differ somewhat from those of Lake (1992) about democracies' war-fighting capabilities. We found that initiators who are democratic are more likely to win than all other types of states, and targets who are more democratic also tend to do better, but not for all the reasons Lake suggested. He argued that democracies should be stronger because lower rent-seeking means greater material capabilities, powerful alliances can be formed, and the society will be more supportive of the war effort. Note that our model uses separate control variables for material capability and alliances, and we find that democratic targets still fare better than their autocratic counterparts. This encourages the conclusion that democracies are more likely to win because they can more effectively rally the support of society for a war effort.

One might expect that democratic initiators win because they select themselves into wars in which they have specific advantages, such as much smaller opponents and more allies. If this were true, then we would expect the Politics*Initiation term to be strongly correlated with some of the control variables. Yet, difference-of-means tests reveal no substantively or statistically significant relationships between that term and capabilities, alliance contributions, or troop quality ratios. In addition, democratic initiators do not make different strategy choices; among the sixteen in our data set, all but two used the modal strategy OADA. In contrast, Reiter and Meek (1998) found, in a random sample of all states in peace and war, that democracies are significantly more likely to choose maneuver strategies. Their finding may be the basis for providing a more fully satisfying answer to the following question: On what basis do democracies first judge a potential war to be winnable and then choose whether to initiate fighting?

CONCLUSION

This article has presented several results about the determinants of war outcomes. Examining all interstate wars from 1816 to 1982, we discovered that the nature of the relationship between regime type and war outcomes is more complex than previously believed. By using a broad sample and a comprehensive array of control variables, and by accounting for important interaction effects, we are able to offer some confirmation of the conventional wisdom that democratic states are likely to win the wars they fight. Notably, however, we discovered important interaction effects between regime type and initiation. Specifically, we found that democratic initiators are statistically and substantively more likely to win than other kinds of states. Furthermore, the empirical results indicate that the effect is not monotonic among initiators: Democracies are most likely to win, highly autocratic regimes are less likely to win, and mixed regimes (anocracies) are least likely to win. These findings add an important qualification to the conventional view that all initiators have an advantage, as it appears that the degree of advantage varies with regime type. We also found that democratic targets are more likely to win than other targets, though this finding has more limited statistical and substantive significance. It appears that the tendency for democratic targets to win wars is most likely due to the greater legitimacy of the regime rather than to lower rent-seeking or bandwagoning by democratic allies.

These results have important implications for broader theoretical issues. The finding that democratic initiators are more likely to win is strong circumstantial evidence of their concern about how foreign policy outcomes will affect domestic political fortunes. The finding that they almost never lose the wars they start is evidence for the theoretical expectation that demo-

of war outcomes because their addition only raises the pseudo R^2 by .07 (model 3 to model 4). Two points are in order. First, the marginal effects tests illustrated in the figures are preferred, as they more precisely indicate the exact expected effects on the predictions of the dependent variable when comparing democratic targets and initiators to other belligerents. Second, R^2 and other summary statistics capture the overall or average contribution made by a particular set of independent variables in explaining the variance of the dependent variable. The vast majority of war participants are not very democratic (mean politics score = −2.6). We should not be terribly surprised that democracy makes, on average, less of a contribution to determining war outcomes than does strategy or military capability. For an extensive discussion of historical average effects versus potential influence, see Stam 1996, 197–8.

cratic governments wish to avoid foreign policy failures. These results fit nicely with Siverson's (1995) finding that democratic initiators suffer fewer war casualties, which suggests they start wars that they are likely to win with little bloodshed. The alert reader will recall that we laid out two theoretical explanations for the selection effect, only one of which was related to domestic political vulnerability. It is possible that democratic initiators win because their policies are based on better information, and we leave it to future research to explore this issue.

Our findings also have important ramifications for the debate on the origins of the democratic peace. There are two principal explanations of the democratic peace: The normative explanation argues that peaceful means of conflict resolution in domestic affairs drive a peaceful foreign policy, whereas the structural explanation maintains that democratic political institutions such as elections restrain militarist adventures (Russett 1993). Our results are consistent with the latter, as they support the view that democratic leaders are willing to take military action if they are confident it will not threaten their domestic political fortunes. Our findings are not consistent with the normative position that democracies intrinsically prefer a nonviolent foreign policy, as the data reveal they will launch wars under the right conditions.

Finally, the implications for realism are mixed. On the one hand, a number of factors that realists hypothesize to be related to outcomes (in particular, allies, military capabilities, and strategy) apparently do have a strong influence. On the other hand, realism simply cannot explain the powerful relationship between democracy and successful war outcomes. If it were the case that democracies simply fought better, then the explanation could be fit into realism by recognizing that institutional democracy is a component of state power, the key focus of realism. The findings, however, indicate that differences in outcomes are not merely the result of some heretofore-misunderstood power differential. Rather, they are the result of certain characteristics peculiar to democracy itself. These may include differences in decision making and/or vulnerability to public opinion, factors which realists are loath to acknowledge. The relationship among regime type, decisions to initiate, and war outcomes indicates that domestic political structure is an aspect that scholars should not omit from the analysis of international relations.

Our findings present three new empirical puzzles for future research. First, exactly what material or strategic advantage makes democratic initiators more likely to win? Reiter and Meek (1998) suggest that democracies choose maneuver strategies, but more study is needed. Second, why do democracies only initiate wars they will go on to win? Do democratic leaders fear being punished at the polls, or do democracies create better marketplaces of ideas? Third, why do democratic targets not perform even better? Since democracies do not fight one another, they are the targets of a group less likely to win (nondemocratic initiators) than the group that confronts nondemocratic targets (a mix of democratic and nondemocratic initiators). Continuing theoretical and empirical research can broaden our understanding of these and other issues.

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Explaining Divided U.S. Senate Delegations, 1788–1996: A Realignment Approach

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We maintain that the rise and fall in the number of states with divided Senate delegations can be explained primarily in terms of long-run forces of realignment/dealignment and staggered Senate elections. We test our model with election data from 1788–1996 rather than only the post–World War II period, which was common in previous research. We show that a large number of divided Senate delegations is not new; indeed, the highest percentage occurred in 1830. Exactly as predicted by our model, we find a cyclical pattern in divided Senate delegations that is tied to realigning epochs. Our analysis also calls attention to the recent decline in the number of such delegations, and we argue that this trend may well continue.

Divided government has become one of the most studied topics in political science in recent years, and it has even been suggested as an organizing principle of American politics research (Fiorina 1992, 3). Since World War II, from the state to the federal level, both parties have shared the channels of power the majority of the time (see, e.g., Fiorina 1992). In this article one particular aspect of divided government is closely examined—split Senate delegations, that is, states with two senators from different parties. An increase in their number after World War II has been observed. Perhaps the most widely known explanation for why split Senate delegations occur and for which types of voters are most likely to split their ticket in voting for senators is the policy balancing model of Fiorina (1992). Exposited and tested in Alesina, Fiorina, and Rosenthal (1991), it was further refined by Alesina and Rosenthal (1995). Fiorina (1992, 80–5) hypothesizes that sending a split Senate delegation to Washington may very well be a rational decision by some voters, namely, those (presumably moderate voters) whose policy position is closer to $(D + R)/2$ than to either $(D + D)/2$ or $(R + R)/2$. This dynamic creates what Fiorina calls an "in-party disadvantage."

While our focus is on our own model of realignment effects, rather than on critiquing/testing other explanations, whatever may be the power of the policy balancing model in accounting for House/president split voting patterns, we are skeptical of its underlying logic as applied to Senate delegations. That is, we doubt that moderate voters balance off senators from their state to arrive at a "ticket" closest to their own policy midpoint (see, e.g., Fiorina 1992, 82–5). If it is assumed that voters are concerned with policy outcomes and choose a ticket accordingly, then what should concern them is the potential effect of their vote on control of Congress or on the partisan differences between Congress and the president, as in the Fiorina (1992) attempt to account for divided government (president versus Congress) in terms of policy balancing. If, for example, the president is a Democrat and one senator is a Republican, then, given the limited influence of any single senator on policy, a moderate voter who wishes to shift policies more toward the center still may have more reason to vote for a second Republican senator than to vote to seek to create a divided Senate delegation. In the logic of the policy balancing model, it seems to us to make no sense to consider the two senatorial seats without considering the overall political context, including which party controls the presidency. We should note that this article does *not* directly test or refute the policy balancing model. We are explaining the rise and fall of mixed delegations at the aggregate level.[1]

Segura and Nicholson (1995) tested Fiorina's strategic choice hypothesis and found little support for it. They propose and test a model to account for differences in senatorial outcomes within a given state that uses such independent variables as candidate quality and campaign spending, along with controls for position of the election in the presidential electoral cycle. They found that the level of challenger finances has a significant effect on the ability of the challenger to unseat the incumbent. They also found that senators facing a stiff primary challenger are more likely to lose in the general election (Segura and Nicholson 1995, 99). Segura and Nicholson (1995) also link the increase in split Senate delegations to the recent rise in candidate-centered politics (Wattenberg 1991), to the increase in the number of voters who consider themselves independents (Wattenberg 1994), and to the growing ability of incumbents to insulate themselves from national electoral tides (Cox and Katz 1996;

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An earlier version of this paper was delivered at the annual meeting of the American Political Science Association in San Francisco, August 29–September 1, 1996. This article is an adaptation of a chapter from the first author's dissertation (Brunell 1997), and the data are available from him upon request. We would like to thank Russell Dalton, William Koetzle, David Mayhew, Barbara Sinclair, and Martin Wattenberg for their helpful comments.

[1] In related unpublished research, however, we do provide an alternative explanation for why one state may be more likely to elect two opposing senators than another state. We suggest that the state's primary type (i.e., open versus closed) has a significant effect on the likelihood of electing a mixed Senate delegation as well as on the general ideological similarity of a state's Senate representation (Grofman and Brunell 1997).

Gelman and King 1990; King and Gelman 1991; Mayhew 1974). They argue that split delegations can be explained by factors specific to the individual Senate contests, without the need to posit an in-party disadvantage.

Previous research also has attempted to account for which states can be expected to have split representation in the Senate. One explanation for divided government is the so-called two constituencies thesis, that is, the notion that some states are sharply polarized, with senators of opposite parties drawing their principal support from two different and generally opposing constituencies of roughly comparable size; the potential for senators of opposite parties being elected in successive elections comes as a result of shifts among the remaining voters. Jung, Kenny, and Lott (1994) found strong support for this thesis. In related work, Bullock and Brady (1983) found evidence that the less homogeneous a state, the more likely it is to elect a split Senate delegation.

One consistency among the research on divided Senate delegations is that no analysis has been done on elections prior to 1940. In reading the literature, one might think that a high number of split Senate delegations is strictly a post–World War II phenomenon. Yet, the percentage of states simultaneously represented by a senator from each of the two major parties has varied greatly over time since the founding of this country, and very high levels of split Senate delegations are found in earlier political eras.

Moreover, most of the models proposed to account for patterns of divided government simply do not apply to the period before passage of the 17th Amendment. It was ratified in 1913 and required that senators be popularly elected rather than chosen by the state legislature. Given the nature of mass politics in America today—many people express no strong partisan identification, elections are largely candidate centered, and turnout rates vary tremendously between on-year and off-year elections—there is good reason to expect that the likelihood of consistent partisan Senate elections should decrease, ceteris paribus.

THE REALIGNMENT MODEL

Our explanation looks to long-run factors and applies to both before and after 1913. Our analysis of data going back to the first Congress will show that the rise and fall in the number of split Senate delegations can be attributed in large part to periods of realignment in American politics.[2] Yet, our data analysis also contributes to analysis of contemporary trends. While authors such as Fiorina (1992, 42) focus on why there has been a general increase in the number of split delegations compared to earlier decades after World War II, our analysis calls attention to an important phenomenon that seems unnoticed, namely, that divided Senate delegations have been *decreasing since 1978*. Indeed, we argue that this trend is likely to continue in 1998, although it is unlikely we will ever return to the low number of divided Senate delegations observed in previous eras.

Our argument is quite simple. A realignment can be thought of as a sustained and durable change in the partisanship and voting behavior of the electorate overall.[3] But, given the staggered election cycle of senators, any radical transformation of the upper chamber is simply not possible in any *single* election. Unlike the House of Representatives, in which every member must face reelection every two years, senators are elected to six-year terms, and no two senators from the same state face the electorate the same year (barring a resignation or death).

A realignment is a multistage process. First, the number of divided Senate delegations increases as the party in whose direction the realignment is moving makes gains in states previously under the unified control of the opposition. Second, the number of unified delegations grows as the realignment eventually sweeps away incumbents of the opposite party and/or finally captures the second Senate seat in the state when it becomes open. Third, as the force of realignment becomes exhausted, idiosyncratic factors and short-run forces regain importance, and there is the slow building of momentum toward a new realignment. Once again, the number of divided Senate delegations rises, with a new inflection point (local maximum) occurring at (or near) the next critical realigning election. Over the course of each realignment era, there should be a clear cyclic shift in the extent to which Senate delegations are unified.

This process occurs relatively slowly. Even when the realigning trend is strong, unseating a well-known senator is not easy. Today, for example, one must raise millions of dollars and generate extensive media coverage and interest before a serious challenge can be mounted (see, e.g., Jacobson and Kernell 1983, 44). Thus, some time elapses before the full ramifications of a realignment are realized in the upper chamber of Congress in the form of unified party control of state delegations.

Table 1 presents an idealized version of the dynamic we propose. A hypothetical realignment is taking place over six states, each with two senators with differing election classes. The Republicans initially control a majority of the seats. As Democratic support grows, the percentage of split states rises. Then, as the remain-

[2] In terms of the debate about whether divided government should replace realignments as the organizing principle of American politics research (Fiorina 1992, 3), we argue that divided government cannot be understood except in the context of realignment, and that this is true for the contemporary period as well as for earlier periods. Of course, the fact that there are long-term trends in the number of split delegations does not mean that outcomes in individual Senate contests cannot be modeled primarily in terms of factors specific to those elections, as was done by Segura and Nicholson (1995), nor does the existence of long-term trends vitiate the accuracy of the Bullock and Brady (1983) findings about which states were most likely to have split delegations. We must always remember the need for multiple levels of analysis and for care about exactly what question we are trying to answer.

[3] While we do not discount the importance of approaching realignments in terms of critical elections, we prefer to think of them as multiple-election phenomena involving a clear direction of change rather than as a single critical election (see Burnham 1970).

TABLE 1. Changes in Senate Delegations during a Hypothetical Realignment

State	Classes	Senators	Divided?
A	1,2	RR	No
B	2,3	RR	No
C	1,3	DR	Yes
D	1,3	DD	No
E	1,2	RR	No
F	2,3	RR	No
Percentage divided: 16.6			
Class 1 up for election			
A	1,2	DR	Yes
B	2,3	RR	No
C	1,3	DR	Yes
D	1,3	DD	No
E	1,2	DR	Yes
F	2,3	RR	No
Percentage divided: 50			
Class 2 up for election			
A	1,2	DD	No
B	2,3	DR	Yes
C	1,3	DD	No
D	1,3	DR	Yes
E	1,2	DR	Yes
F	2,3	DR	Yes
Percentage divided: 66.7			
Class 3 up for election			
A	1,2	DD	No
B	2,3	DD	No
C	1,3	DD	No
D	1,3	DD	No
E	1,2	DR	Yes
F	2,3	DD	No
Percentage divided: 16.6			

ing senator from these states comes up for reelection, there is a reunification of the senators, only this time with two members of the opposite party.

Because our model emphasizes the cyclic nature of split Senate delegations, it suggests that what goes up will come down. In the next section we provide strong historical evidence that cyclical patterns exist, with critical inflection points in the number of divided Senate delegations corresponding to critical (realigning) elections. Moreover, the evidence suggests that 1980 was a critical election in the South, the culmination of more than fifty years of declining Democratic hegemony and a critical point in what we propose should be called "glacial, top-down" realignment in the South.[4]

[4] The realignment is "glacial" in that only now are the remnants of the Civil War realignment waning in the South, and no one "critical election" can define the change. The change is "top-down" insofar as we agree with Aistrup (1996, particularly chapter 9) that the pattern of Republican success in the South begins with the highest office (the presidency), moves downward to Senate and House elections, and then to state and local elections.

We emphasize that the classification of 1980 in realignment terms is not crucial for our more general argument about the importance of cyclical trends in the number of split Senate delegations.

DATA ANALYSIS

Longitudinal Patterns in the Percentage of Split Senate Delegations

To understand why and when split Senate delegations occur, we look at the makeup of the Senate by state for all 105 Congresses from 1788–1996.[5] Figure 1 is a time series depicting the percentage of states with split Senate delegations from 1788 to 1996. It shows that divided delegations are not a new phenomenon in the United States. Contrary to our expectations before examining the data, even when state legislatures were electing senators, divided delegations were common.[6] While the percentage of states with split Senate delegations was at an extremely high mark in 1978, it was not the highest in history. The all-time high was 60% of states in 1830.[7]

Regional and Temporal Variations in the Percentage of Split Senate Delegations

There are interesting differences in the pattern of split Senate delegations not only over time but also among regions. Table 2 shows some clear long-run differences among the West, Midwest, South, and Northeast.

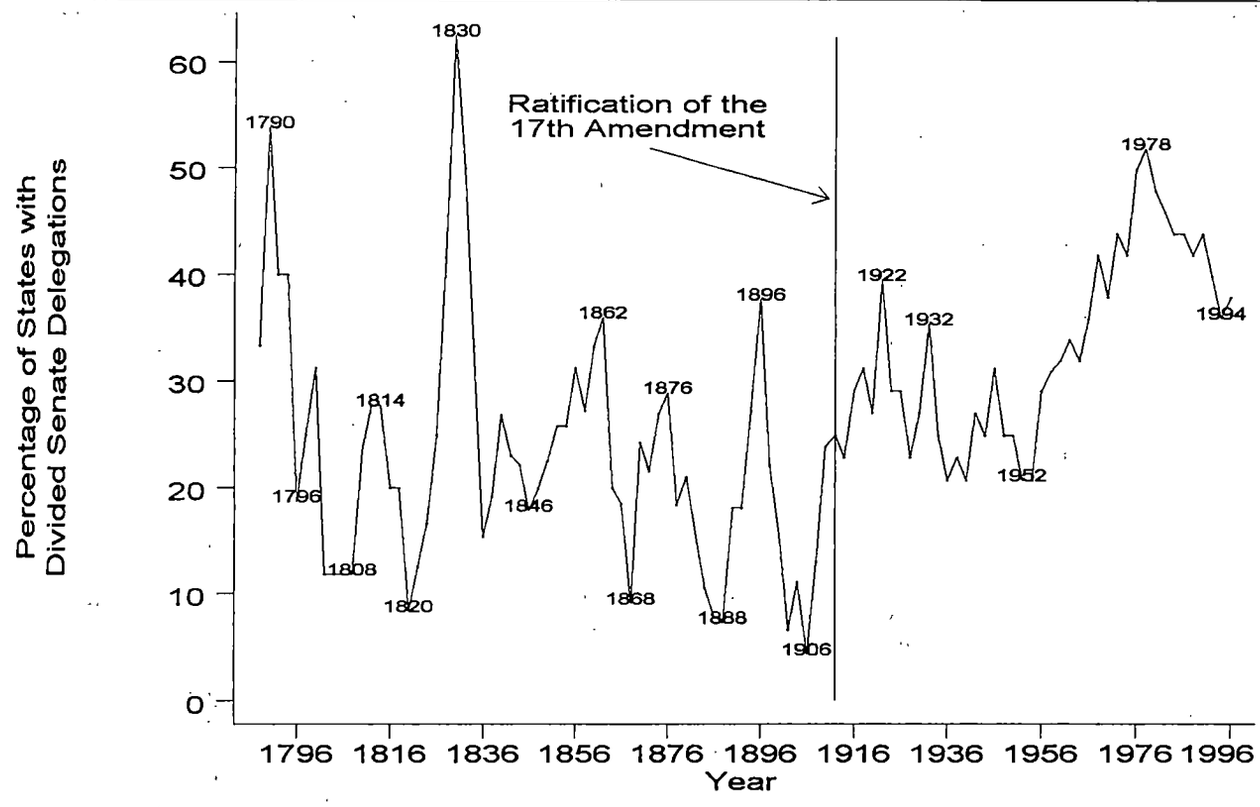
Two particular details are evident from Table 2. First, beginning early in this century, there is a long-run generally upward trend in the percentage of divided delegations as compared to earlier periods. In 1906, the percentage was at an all-time low—only 4% of states had a nonunified Senate delegation. From 1906 to 1924 the trend increased nearly monotonically each year (refer to Figure 1). After that, the overall proportion never falls below 20%. One plausible explanation for this pattern is ratification of the 17th Amendment in 1913, which changed how senators are elected. Indeed, we find that the percentage of divided delegations is negatively correlated with year for the period 1788–1912 (r = minus .32, $p < .01$) but positively correlated with year from 1914 to 1996 ($r = .70$, $p < .01$). The second detail is that the South has experienced by far

[5] These data were gathered from *Congressional Quarterly's Guide to U.S. Elections*, 3d ed. (1994), supplemented with data on the 104th and 105th Congress.

[6] Obviously, if a state legislature remained in the same hands, then two senators from the same party could be expected. Yet, legislative choices were not always this straightforward. For example, some legislatures were so deadlocked as to whom the next senator should be that they left the seat vacant. Between 1891 and 1905, this occurred no less than fourteen times, including California 1899; Delaware 1895, 1899, both seats in 1901 and 1905; Kentucky 1896; Louisiana 1892; Montana 1893; Oregon 1897; Utah 1899; Washington 1893; and Wyoming 1893 (Haynes 1960, 92). On at least two occasions, states controlled by one party elected a senator from the other party. In 1893, Kansas and North Dakota, both dominated by the Republicans, sent Democrats to the Senate (although, at the time, party lines in both states were "badly blurred"; see Haynes 1960, 92).

[7] A divided delegation has two senators from different parties—any two parties. While we are very confident about partisan identification of members in the modern era, there are inconsistencies among reference books regarding the "true" party of members of Congress in the early periods of the Republic. We use the partisan identification from *Congressional Quarterly* (1994).

FIGURE 1. Divided Senate Delegations, 1788–1996



Note: A divided Senate delegation has senators from different parties.

the lowest amount of split delegations compared to the other three regions, which comes as no surprise given the Democratic domination there. The West has had the highest incidence of divided representation in the Senate.

TABLE 2. Split Senate Delegations: Regional and Longitudinal Differences

Region	Average Percentage
West	38.9
Midwest	28.2
South	15.4
Northeast	29.5
Period	
1788–1828	23.4
1830–70	26.3
1872–1912	18.2
1914–54	26.5
1956–96	40.2
Overall	28.0

Note: Average percentage is the number of states with nonunified Senate delegations among the total number of states in that region or time period. *West*: Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, North Dakota, Oregon, South Dakota, Utah, Washington, and Wyoming. *Midwest*: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, Ohio, Oklahoma, and Wisconsin. *South*: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, and Virginia. *Northeast*: Connecticut, Delaware, Kentucky, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Tennessee, Vermont, and West Virginia.

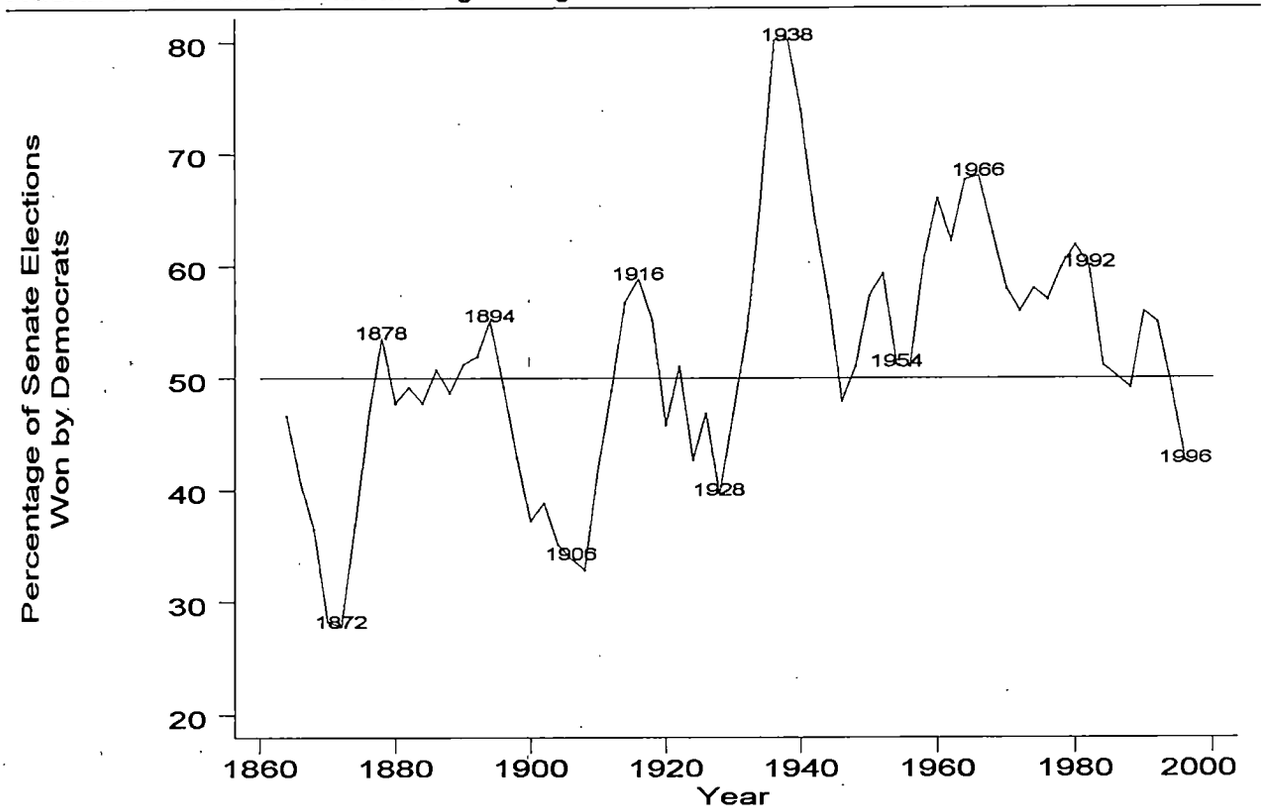
Realignment Periods in U.S. Electoral History and Trend Directions

Another very important point can be teased out of Figure 1. The upward and downward movement in the percentage of divided Senate delegations exhibits a greater number of runs (i.e., continuously upward or downward movement) than would be expected by chance from a normal distribution with the same mean and variance.[8] As we shall see, these runs can be linked to realigning periods.

The standard analyses of U.S. party systems posit five historical epochs (Key 1955; Sundquist 1983). (1) The first party system ended in 1828–32 with the victory of Jacksonian democracy. (2) The second party system lasted until 1858–60 and the coming of the Civil War. (3) The third party system lasted from the end of the Civil War to the election of 1896, in which the power of western mining and agrarian interests in the Democratic Party strengthened, but the Republican Party remained dominant. (4) The fourth party system, also a period of Republican strength, lasted from 1896 until 1932 and the rise of the New Deal coalition. (5) The fifth party system, forged by Franklin D. Roosevelt and the Republican opposition to his policies, arguably persists. Many scholars claim that a long period of

[8] With a mean of .27 and a standard deviation of .11, our random simulation exhibited no runs of length greater than 4 and only one run of that length. In contrast, the actual data have two runs of length 4, three runs of length 5, and one run of length 6.

FIGURE 2. Democratic Tide Moving Average



Note: The graph is a three-election moving average of the percentage of Senate elections won by the Democrats. Every point then is the percentage of elections in which the Democrats win for that year averaged with the same data for the two prior elections.

dealignment began in 1964 or so, perhaps even earlier. Our analyses will bear on this controversy.

Realignment-Related Historical Inflection Points

For present purposes, there are two important aspects of Figure 1. One is the cyclical nature of change in the number of divided Senate delegations that it reveals. The other is that, even though the key long-term patterns are somewhat obscured by features of the data not directly related to realignments (e.g., women's suffrage in 1920), with a little care one can discern the starting date of the five realignments as inflection points in the data. That is, divided delegations peak around the same time that a major realignment brings about massive transformations of the American electorate (1830, 1858, 1896, and 1932) and at the beginning of the first era of American electoral politics (1790).

The first period of interest is around 1828. The percentage of mixed Senate delegations rises from a low of .083 in 1820, peaks at .625 in 1830, and returns to a low of .192 in 1838. During this realignment we see states deunifying their delegation as they elect one Democratic senator; only later do they reunify their delegation with two Democrats.

Next, during the Civil War realignment around 1860, we see a rise in split delegations beginning in the late 1840s, with the number increasing throughout the next decade until it peaks in 1862. It then begins to fall as more and more states reunify their senators, only this time it is the Republicans in control. To establish that the partisanship is moving in the hypothesized direction, we plot the Democratic tide in Figure 2, which is the percentage of the Senate controlled by the Democrats since the beginning of the modern party system in 1860. Thus, around 1860 and shortly thereafter, we expect a low percentage of Democrats to be in office, and this expectation is met. The number of Democrats falls from 1860 until the early 1870s, when it reaches a local minimum.

There is another local peak in the number of divided Senate delegations, shown in Figure 1 for 1896, as the Republicans reassert their electoral dominance over the Democrats. Again, we would expect the Democrats to do very poorly in Senate elections near the realignment of 1896. In 1894 the Democrats held a majority of the seats in the Senate, but in 1896 and continuing until 1906 the Republicans regained control of the Senate, and the Democratic tide ebbed.

Figure 1 also shows mixed delegations rising before the 1930s, peaking in 1932, and then receding for most of the elections shortly thereafter. Yet, the local maximum around the New Deal realignment occurred in the early 1920s. While we cannot explain with certainty why this peak occurred then, one possible reason is the

TABLE 3. Modeling Longitudinal Changes in the Percentage of Divided Senate Delegations, 1788–1996

Intercept	9.52*** (5.00)
Percent divided ($t - 4$)	.521*** (7.18)
Percent divided ($t - 2$)-($t - 8$)	.266*** (5.10)
Post-1912 dummy	6.16*** (4.03)
Realignment 1830	12.49*** (5.50)
Realignment 1862	8.31*** (3.90)
Realignment 1896	7.49** (3.43)
Realignment 1932	2.67 (1.26)
R^2	.69
Adjusted R^2	.67
N	101.00

Note: Entries are unstandardized regression coefficients with t-statistics in parentheses. The dependent variable is the percentage of divided Senate delegations for each Congress from 1788 to 1996. **$p < .01$; ***$p < .001$.

introduction of women into the electorate.[9] The direction of partisan change in Senate elections given in Figure 2 and with the pattern of divided delegations in Figure 1 support our theory about divided delegations and partisan realignments.

While our model rests on a shift in electoral outcomes from one party to another, we are not saying that partisan realignments (past or present) are strictly unidirectional. Indeed, much of the literature has argued that these shifts, while massive, are still local affairs. Nardulli (1995, 10) states "that critical realignments are subnational phenomena that vary considerably in form, [and] not the majestic national movements some believed them to be." Nardulli also finds that only the realignment in 1932 involved more than half the country. Clubb, Flanigan, and Zingale (1980) also argue that the New Deal shift was atypical in that it involved more of the nation than previous realignments. Thus, while we posit a clear shift from one party to the other, we do not expect one party to win all Senate elections in any particular year; indeed, we know this is not the case.

A Multivariate Model of Divided Senate Delegations

We offer in Table 3 a model to explain the cyclical variance in split Senate delegations from 1788 to 1996. The dependent variable is the percentage of divided Senate delegations in all states.[10] We model results in terms of the following.

1. A four-year lag ($t - 4$) on the dependent variable. This lag is not contaminated by those states whose status as unified or divided has not yet had an opportunity to change. Over a four-year period, at least one seat in each of the 50 states experiences a new Senate election.
2. A ($t - 2$) − ($t - 8$) lagged difference on the dependent variable to pick up a directionality. Recall that the percentage of divided Senate delegations is predicted by our realignment approach to be consistently moving, on average, in a given direction over a series of elections. Sometimes that direction will be upward, sometimes downward, depending upon the phase of the realignment cycle.[11]
3. Realignment-specific trend variables for each of the recognized realignment eras.[12]
4. A dummy variable to capture the putative effect of the 17th Amendment, which is coded +1 for elections after 1912 and 0 for earlier elections.

As can be seen in Table 3, the lagged terms are highly significant. Of the four realignment terms, all have the correct sign, and 1932 is the only election that is nowhere near statistically significant. Finally, as expected, the post–1912 variable is positive and statistically significant. The coefficient on this term is 6.16, which means the introduction of popular elections to the Senate added, on average, about three states (6% of 50) to the overall number of states with divided party delegations. Since there are 101 observations, an adjusted R^2 of .67 with only seven variables is a quite reasonable performance for our model.[13]

The Modern Era: The Realignment/Dealignment Controversy

Students of realignment dispute exactly what kinds of changes the American political landscape has been undergoing recently. If the pattern is a shift every 32 years or so, then there should have been a realignment long ago, certainly by the end of the 1960s. Absent any sustained change in party control at all levels of government, some political scientists began to question the utility and explanatory power of realignment theory and its application to modern American politics (see Ladd 1991). We agree with others (e.g., Burnham 1991; Nardulli 1995), however, that realignment theory still offers important insights and testable hypotheses.

If there has been a realignment since World War II,

[9] The 19th Amendment was ratified in 1920. For a discussion of its effect, see Brown (1991, chapter 6). Figure 2 indicates a revival of Republican strength in Senate elections during this time.

[10] Since we use the percentage of states with divided representation each year as the dependent variable, we cannot use a two-year lag. Since Senate elections are staggered, one-third of the states exhibit no electorally based change in representation from $t - 2$ to t. By using a two-year lag, we would in part be using the dependent variable to explain itself.

[11] Because each of the three cohorts is only partly overlapping in terms of the states holding elections in any given year, even lags such as $t - 2$ and $t - 4$ are not fully autoregressive. Strictly speaking, only a $t - 6$ (or $t - 12$) lag would be fully autoregressive. We have not used those lags.

[12] Operationalizing each of the realigning period variables reported in Table 3 involves coding values of 1 for the three elections preceding a realignment, −1 for the three elections following a realignment, +2 for each critical election year, and 0 for all other years. We experimented with a variety of operationalizations and found this to be marginally best. Yet, little explained variance is lost by simply using realignment year dummy variables for peak elections (1830, 1862, 1896, 1932).

[13] This is especially true since we are using no explanatory factors that are truly election specific, other than realignment periodization. We should note that the same model was run on the data with the southern states removed. The only difference worth noting is that the coefficient for the post–1912 variable rose to 8.21. The coefficients for all other variables were nearly the same as reported in Table 1.

it is unlike any experienced previously. While the number of people identifying themselves as Republican has increased considerably, the magnitude of this change has been too small for most authors to conclude that a partisan realignment has taken place (see, e.g., Wattenberg 1994), especially since the most dramatic recent change in party identification is the rise in the number of voters identifying themselves as independent. Nonetheless, the ratio of self-identified Democrats and Republicans has gone from about 3:2 to a more nearly even balance. Similarly, while Figure 2 shows that the general trend in Democratic electoral success in the Senate has been downward since the 1960s, the secular trend is not clear, and we would not conclude that a Republican realignment has taken place. Moreover, there has been no critical election, financial disaster, or world war to spawn the political earthquake normally associated with a realignment.

We argue below, however, that *in the South* there is evidence of a regional realignment whose contours were masked by the dramatic rise in Democratic Party loyalty among black voters in the 1960s and the growth in black enfranchisement since then (Alt 1994). Furthermore, the shift occurred from the top (i.e., the presidential level) down (see Aistrup 1996; Bullock 1988) and moved at a glacial pace.

A New Era of Realignment?

After falling in the period following the New Deal realignment, the percentage of divided Senate delegations began to rise in the early 1950s and continued to do so reasonably steadily for nearly thirty years. From Figure 1 it looks like the New Deal shift never quite culminated, since the percentage of split Senate delegations did not return to the lows reached both before and after earlier realignments. Yet, this may be due simply to a long-term shift caused by the change in the mode of electing senators. Moreover, the percentage of divided Senate delegations peaked in 1978 and then began to decrease.

This feature of the data can be regarded as purely accidental, but it suggests a possible realigning trend. In particular, the model projection shown in Table 3 for the 1998 election predicts a decrease of a few points in the percentage of divided Senate delegations, from 38% in 1996 to 33% in 1998, which equates to two or three fewer states with a mixed delegation. If this occurs, then it will be the seventh decrease in the last ten elections (one of which involved no percentage change. Thus, one important point not mentioned in the recent literature (see, e.g., Fiorina 1992; Segura and Nicholson 1995) is that the most recent trend in split Senate delegations is clearly downward.[14] Nonetheless, even if the downward trend continues, a return to the previous historic lows in the number of divided Senate delegations is unlikely, given the increased volatility in the electorate, the decline in party loyalty, and the rise in the number of independents.

Glacial, Top-Down Realignment in the South

Although the overall percentage of states with divided Senate delegations may not fall much farther, it is plausible to believe that the South is experiencing a prolonged top-down Republican realignment, despite occasional temporary reversals in the pattern in some southern states. It is primarily this development that accounts for the decline nationally in mixed Senate delegations.

Our finding are fully consistent with other research on the South. Aistrup (1996) documents a "Southern Strategy" initiated in the 1960s by the Republican Party to transform the South into a GOP stronghold through a "top-down advancement" of Republican candidates. Similarly, Bullock (1988, 562) found that "the share of GOP Senate seats in the South grew by 40 percentage points between 1960 and 1980." He also found that while the general trend in the South is toward the Republicans, there have been setbacks (such as the 1986 Senate elections), and that the general progression has been a top-down realignment.

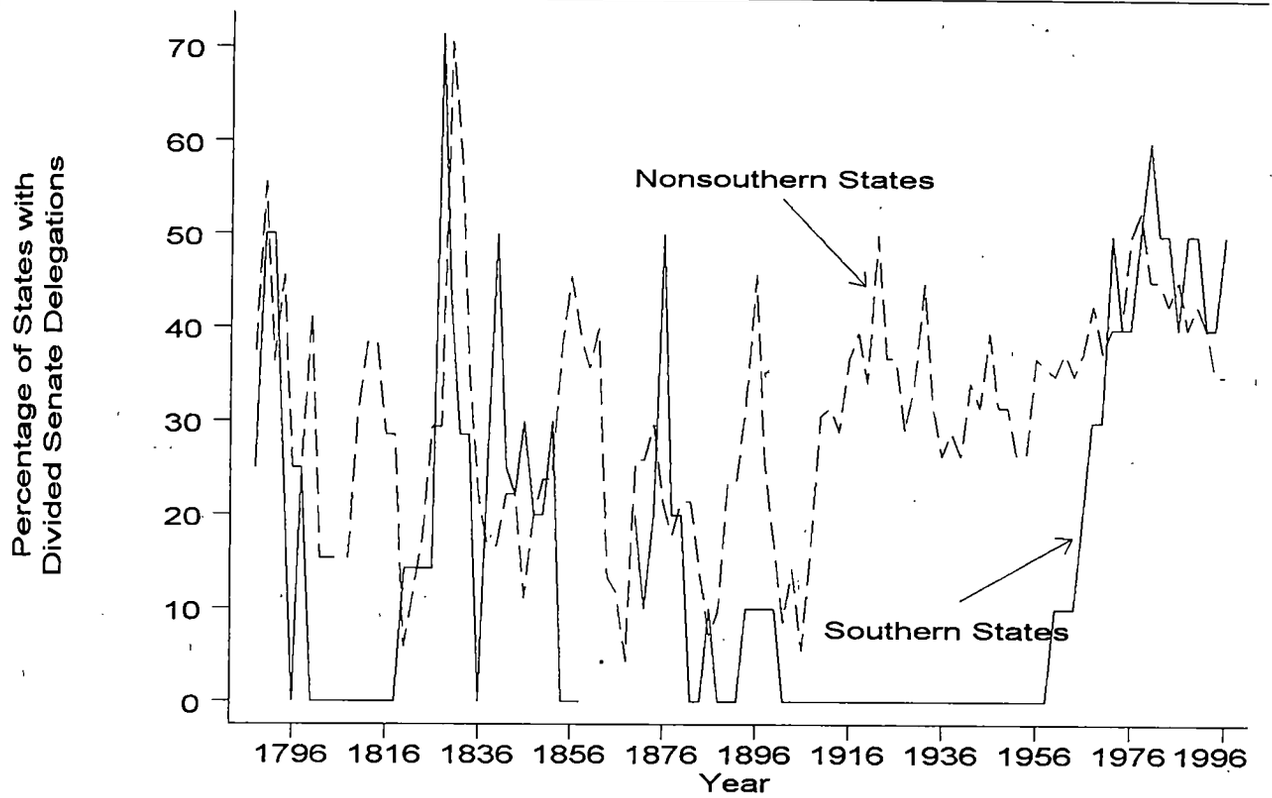
In Figure 3 the percentage of divided Senate delegations is shown separately for the South and nonsouthern states. We see a clear upward tendency in the number of divided Senate delegations since 1960. Indeed, comparing the two trends, we see that much of the change has occurred in the South. Yet, we also see that this upward trend in the South may be reversing as the Democrats lose their historical strength in the region and the Republicans gradually become the dominant party. Given recent events (particularly the 1994 elections), we might expect a continuing Republican realignment in the South and thus a period in which many of these states elect two GOP senators.[15] As previously noted, we consider this movement in the South glacial, since the time frame is like none of the past realignments in the American political experience.[16]

[14] We think of this dealignment as a combination of many small events, or tremors, that are generally but far from completely moving the political landscape toward the Republican Party, with the most important evidence of long-lasting change coming from the South (see below).

[15] Further evidence of a slow realignment in the South comes from within-state correlations for the vote share in Senate elections. First, we ran the correlation for the Democratic percentage of the vote for the same Senate seat (Senator A's vote share in 1990 correlated with Senator A's vote share in 1996) and found that the correlations were at an all-time low ($r = .10$) for the elections of the late 1980s and early 1990s. After running a second set of correlations are for the vote share between the two different senators in a state (Senator A's vote share in 1992 correlated with Senator B's vote share in 1994 or 1996), we found that in the late 1980s the correlation was negative ($r = -.4$), and only for the most recent elections was it positive. We interpret this as a slow shift from all Democrats to what eventually will be a nearly unified Republican South. Currently, only Louisiana is represented by two Democrats. This is remarkable since as late as 1960 all ten southern states were represented by unified Democratic delegations in the Senate.

[16] In the patterns we observe, small to moderate gains made by one party or the other in any given election may quickly be wiped away, even in the very next election. In contrast, past partisan realignments have been portrayed as involving massive unidirectional change (see,

FIGURE 3. Percentage of Divided Senate Delegations in Southern and Nonsouthern States



Note: Divided Senate delegations are those with senators from different parties. The South is defined as Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, and Virginia. The break in the trend line for the southern states (1860–66) is due to the Civil War and the expulsion of senators from these states.

DISCUSSION AND ISSUES FOR FURTHER RESEARCH

Split Senate delegations are not merely a result of the modern era of divided government. There have been long-run cyclical patterns in the percentage of nonunified delegations since the beginning of the Republic, and in much earlier political eras there was a high percentage of states represented by senators of opposing parties. We argue that the rise and fall in the number of divided Senate delegations can be tied to periods of electoral realignment in American politics. As a realigning trend begins, the number of divided delegations increases and peaks in (or near) some critical election. As the realigning wave works its way through the six-year senatorial election cycle, the number of divided delegations declines, as one party begins to dominate electoral politics. Then, a new wave begins and starts the cycle all over again. We have provided convincing empirical evidence and a clear theoretical rationale for the existence of such repeated patterns.

Also, focusing on longitudinal patterns in split Senate delegations provides a new perspective on an old debate about exactly when realignments take place. The standard approach, dating back at least to Key's (1955) classic article, is to date realignments from the critical election in which one party's dominance for the next period is established. Thus, the realignment of 1896 is followed by the realignment of 1932. But it may be even more meaningful to think about realignments not from peak to peak (e.g., 1896 to 1932), but from trough to trough (e.g., from 1846 to 1888, from 1890 to 1906, from 1908/1922 to 1950, from 1952 to ??). What has been regarded as *the* critical election becomes a high point (maximum point of inflection, or near to one) of a realigning trend, somewhere in the middle of our new definition of a realigning era.

From this perspective we see the realigning wave as being visible long before the actual critical election. This approach largely sidesteps controversy as to whether it is 1928 or 1932, 1828 or 1832, say, which should be taken as the "defining" election. It also suggests very different answers to the question of how best to periodize the changing epochs of American party politics. Yet, full resolution of this question requires us to look at a variety of other types of data on the pace of realignment across different levels of government.

Unfortunately, there are other questions for which our data (and theory) cannot yet provide a fully satisfactory answer. In particular, did a national realignment emerge in or around 1980? Here the evidence is mixed, but the probably answer is "no." If not,

e.g., Brady and Lynn 1973). Our finding is consistent with Bullock's (1988, 569) statement that "unlike realignments of the latter half of the 19th century, the changes of the last generation have been more gradual and less persistent."

is a realignment taking place, but only in the South? We believe the probable answer is "yes." Will we ever return to the previous low levels of divided Senate delegations? While definitive evidence is lacking, the answer is likely to be "no," given the fundamental change in 1913 to popular election of senators.

Our results are important to students of American politics and not just to specialists on Senate elections. First, we show the usefulness of time-series data in helping to develop less time-bound explanations. Second, our results contribute to the ongoing revitalization of realignment theory (Aistrup 1996; Bullock 1988; Burnham 1991; Nardulli 1995; cf. Ladd 1991). Third, our model predicts a future decline in the number of divided Senate delegations that flatly contradicts the established wisdom (Fiorina 1992).

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Estimating the Effect of Campaign Spending on Senate Election Outcomes Using Instrumental Variables

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To examine the traditional view that challenger spending is more effective than incumbent spending, I reestimate the effects of spending using instrumental variables that affect a candidate's ability to raise campaign funds, such as candidate wealth levels. When the endogeneity of candidate spending levels is properly taken into account, the marginal effects of incumbent and challenger spending are roughly equal. In contrast to previous research showing that, because of higher marginal returns to challenger spending, the incumbent's spending advantage cannot explain high incumbent reelection rates, this article shows that in an average Senate election the incumbent's spending advantage yields a 6% increase in the incumbent's vote share. That incumbent spending wins elections has direct implications regarding the consequences of campaign finance reform. My findings imply that equalizing spending levels may significantly increase incumbent defeat rates, and caps on candidate spending may improve the chances of challengers.

In American congressional elections incumbents routinely win reelection. Even in the 1994 Republican landslide, change in the partisan makeup of Congress occurred mainly through open seats switching from the Democrats to the Republicans. A common explanation for high congressional reelection rates is the large campaign spending advantage enjoyed by incumbents. It is widely believed that challenger spending is very important, but there is surprisingly little evidence in the academic literature that incumbent campaign spending has an important effect on congressional election outcomes. In light of the substantial effort incumbents devote to fundraising, as well as some nagging methodological questions about existing academic studies, the effect of incumbent spending on election outcomes remains an unresolved issue.

Accurately measuring how incumbent spending leads to votes is of obvious importance. Politicians want to win elections, and so if money matters, then this will influence their behavior. If campaign spending effects are trivial, then concerns about politicians being purchased by contributions from PACs and other "special interests" are probably exaggerated. Accurate measurement of the effect of campaign spending is essential for evaluating the effects of campaign finance reforms. Finally, most of the academic literature on campaign finance has yielded a provocative finding that fuels additional investigation: For given spending levels, incumbent campaign spending appears to have much smaller marginal returns than challenger spending.

This article measures the effect of campaign spending on Senate election outcomes. I begin by briefly reviewing the academic literature. A key issue in estimating the effect of campaign spending is that campaign spending levels respond to expectations about the closeness of the election. Most research ignores this or downplays its significance. After reviewing the existing studies, I present a model of Senate election outcomes and describe instrumental variables that can be used to estimate the effects of spending. I argue that contrary to a view often expressed (Jacobson 1985, 1990), valid instruments can be found to permit the identification of a two-stage least-squares model of the effect of campaign expenditures on election outcomes. The major innovation here is the use of a new set of instrumental variables that permits consistent estimation of the effects of candidate spending. This study employs instrumental variables which shift a candidate's cost of funds, such as candidate wealth levels. If variables such as candidate wealth levels do not have a direct influence on election outcomes, then they can be used to obtain consistent estimates of the effects of spending. Following a description of the model and data, I report the results of model estimation. Ordinary least-squares (OLS) and two-stage least-squares (TSLS) estimation of a standard model of Senate election outcomes produce very different results. OLS estimation confirms the conventional view that incumbent spending has a lower marginal effect than challenger spending, while TSLS estimation shows the marginal effects of spending by challenger and incumbent to be both statistically equivalent and substantively important. The article concludes with an examination of the implications of the findings. I consider, among other things, how my results regarding campaign spending in Senate elections relate to the more general issue of when campaign spending may be more or less effective.

REVIEW OF THE LITERATURE

Over the past two decades, there have been numerous attempts to measure the effects of spending on election outcomes. Most authors have assumed that candidate spending levels may be viewed as exogenous (among others, see Abramowitz 1988, 1991; Ansolabehere 1990; Caldeira and Patterson 1982; Giertz and Sullivan

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This research was supported by a National Science Foundation dissertation fellowship and by the Institution for Social and Policy Studies. An earlier version of this paper was chapter 1 of my dissertation (Massachusetts Institute of Technology, 1993). I am very grateful to Steve Ansolabehere, Donald Green, Morris Fiorina, Bill Keech, Jim Poterba, Jim Snyder, and Charles Stewart for comments on the earlier version of this work. I also thank the anonymous reviewers for many very helpful suggestions.

1977; Glantz, Abramowitz, and Burkhart 1976; Grier 1989; Kenny and McBurnett 1992; Levitt 1994; Lott and Warner 1974; Shepard 1977; Silberman and Yokum 1978; Thomas 1989; Welch 1974). The canonical example of work in this literature is a regression of a candidate's vote level on some function of the candidate's spending levels and other variables relevant to the election outcome.[1] The basic differences among studies are use of alternative functional forms for converting spending into votes (linear, quadratic, and logarithmic functions are used) and analysis of different types of campaigns (the U.S. House is most common). The model specification in many of the earliest studies focuses on partisan differences and restricts the marginal effects of challenger spending and incumbent spending to be identical. When this assumption was relaxed, the central finding of this literature was uncovered: Challenger spending has much greater marginal returns than incumbent spending; the effects of the latter are small and often not statistically significant (e.g., Abramowitz 1988; Jacobson 1980, 1985, 1990). The result that challenger spending is important while incumbent spending is not has been verified consistently by OLS regressions. Jacobson (1985, 23) summarizes the findings of numerous academic studies: "The idea that the challenger's spending level is what matters for election results is repeatedly supported. Indeed, it is supported by results from almost every set of elections where the question has been tested."

The result that incumbent spending has little or no effect on the incumbent's election chances, and in some studies actually appears to reduce the incumbent's expected vote, has provoked two responses. The first is attempts to explain why these findings make sense theoretically. The second is a few attempts to improve the empirical methods to correct for possible endogeneity of campaign spending.

Gary Jacobson provides the main theoretical explanation for the relative ineffectiveness of incumbent spending. He argues that attempts by both the challenger and the incumbent to influence the voters are subject to decreasing returns. Since the incumbent begins with the built-in advantages of staff and free mailings, any spending by the incumbent is an addition to already high levels of campaign activity. What is more, even before spending the first dollar, the incumbent is well known to the voters, so any additional communication will add relatively little to their knowledge. Challengers, in contrast, are generally unknown and so benefit greatly from campaign exposure (Jacobson 1978, 1990).

Jacobson's theory, originally developed to explain differences in the effectiveness of spending by incumbents and challengers in U.S. House elections, can be applied to Senate elections to generate predictions about Senate incumbent and challenger spending effects. First, roughly speaking, Senate incumbents are at least as well known as House incumbents, so we should expect incumbent spending to have minimal effect. Second, since the level of voter knowledge of Senate incumbents is greater than the level of voter knowledge of Senate challengers, we would expect incumbent spending to be less effective than challenger spending. These expectations are in fact confirmed by existing work on Senate elections (Abramowitz 1988; Jacobson 1985).

Jacobson's explanation for observed differences in the effectiveness of spending is straightforward and appealing, but there are grounds for skepticism. First, while senators are relatively well known, they have the opportunity to spend money to address new issues or concerns, some of which may not have arisen in previous years. Second, campaigning not only informs the voters about oneself but also brings to light damning information about one's opponent. When the voters do not know much about the challenger, this gives the incumbent a great opportunity to use money to "define" the challenger.

In theory, incumbent spending may be *more* effective than challenger spending. Incumbents typically have advantages in organization and expertise that make their expenditures more efficient and therefore more effective dollar for dollar than those of challengers. If this is an important consideration, then the marginal effect of spending by the incumbent may be *greater* than the marginal effect of challenger spending. A final reason for skepticism is that the actual behavior of incumbents, who are political professionals, appears to contradict the premise that incumbent campaign spending has little effect. If incumbents are sensible, then it is hard to explain their substantial fundraising efforts.[2]

It is possible that Jacobson has constructed an ingenious theoretical explanation for a nonexistent fact. The true effects of spending may be mismeasured due to methodological problems that result when spending levels are treated as exogenous variables. There are a number of reasons to suspect that regressions which assume spending is exogenous yield biased coefficients. Some reasons to expect that spending levels are influenced by electoral conditions include: (1) As the probability of victory rises, it is easier for a candidate to raise money; (2) if the election looks close, supporters are more likely to contribute to the candidates; and (3) if the incumbent becomes sure of victory, the incumbent scales back fundraising activity. For these reasons and others, if there are factors influencing the election outcome that are not captured in a single-equation regression model, then the effects of spending on election outcomes will be biased due to correlation between the spending levels and the regression error.[3]

[1] For an analysis using survey evidence, see Kenny and McBurnett (1992).

[2] This common argument can be refuted if it is believed that the marginal cost of raising funds for the incumbent is very low, since the large amounts of money raised can be easily reconciled with low marginal benefits. Anecdotal evidence suggests otherwise; politicians find raising money distasteful and time consuming. Consider the view of Hubert Humphrey, who called raising funds "a disgusting, degrading, demeaning experience" (Jacobson 1978, 475).

[3] For a game theoretic model showing formally how omitted explanatory variables will lead to biased estimation, as well as how variables

Most of the empirical literature ignores or dismisses the issue of the endogeneity of spending levels, although several studies attempt to correct for possible biases. Attention has been focused on elections to the U.S. House. As part of his seminal work estimating the effects of campaign expenditures, Jacobson (1978) attempted to estimate a TSLS model of spending in House elections.[4] A recent paper estimating the effects of campaign spending using an instrumental variables approach is an important study of U.S. House elections by Green and Krasno (1988).[5] Their work has heightened debate, as the results call into question the conventional view that incumbent spending is less important than challenger spending.

Green and Krasno show that, in contrast to the usual OLS findings, incumbent spending effects are statistically significant and roughly equal to challenger spending effects. This is a welcome finding for those who suspect that incumbent spending is important, but several methodological questions have been raised regarding their study.[6] Most important, because of a lack of instrumental variables, they assume that challenger spending is exogenous, and only incumbent spending is endogenous. If this assumption is false, then inconsistent estimates of both challenger and incumbent spending effects will result.

Nearly all the research on Senate elections relies on OLS regressions.[7] Jacobson (1985) analyzes six years of Senate elections, presenting regression results separately for each year. He finds that challenger spending has strong and statistically significant effects on candidate vote totals, while incumbent spending effects are smaller and rarely statistically significant. Abramowitz (1988) estimates a model of Senate elections which includes many explanatory variables omitted from Jacobson's regressions and pools the results of seven years of Senate elections. Like Jacobson, he finds that challenger spending has strong and statistically significant effects. He shows that while incumbent spending is significant at the .05 level, the marginal effect of challenger spending is approximately three times that of incumbent spending.[8]

This article reestimates the effect of campaign spending on Senate election outcomes and extends the literature in several ways. First, it improves upon some of the best existing work by treating both challenger and incumbent spending as endogenous. Second, it employs a new set of instrumental variables. Estimation will focus on variables, such as candidate wealth levels, that make raising campaign funds easier or harder for the candidate. These instrumental variables avoid some of the problems that called earlier work into question. In addition, this study attempts to ensure the accuracy of the regression results by performing formal statistical tests of the assumptions underlying the instrumental variable regressions, as well as by undertaking several different regressions to establish the robustness of the findings.

DATA AND METHODS

In the following two subsections, I first present a model of Senate election outcomes and describe the data. I then discuss the instrumental variables used to estimate the model.

Election Outcome Model and Data

Data were collected for all Senate elections occurring in 1974 through 1992. After eliminating open seats and elections for which there were missing variables, 229 elections remained.[9] The model estimated had the basic form:

$$\textit{Incumbent Vote \%} = \alpha + \beta_1 \log(Spend_c) + \beta_2 \log(Spend_i) + \beta_3 Z + E\,,$$

where $Spend_c$ is *Challenger Spending*; $Spend_i$ is *Incumbent Spending*; Z is a vector of variables capturing political conditions, economic conditions, and other factors thought to be important to the Senate election outcome; B_3 is a vector of coefficients; and E is an error term.

In all regressions, the variable on the left-hand side is the incumbent's percentage of the two-party vote (*Incumbent Vote Share*). In addition to candidate spending levels, a number of variables are included on the right-hand side. These measure the effects of challenger quality and partisan, ideological, and economic conditions on the incumbent's percentage of the

that affect a candidate's "cost of campaign funds" function can be used as instrumental variables, see Gerber (1993). Due to the various ways spending levels may be influenced by expectations regarding the election, it is not possible to predict the bias in OLS regression coefficients. A downward bias in the incumbent spending coefficients can easily follow if incumbents vary their fundraising intensity according to whether their reelection appears safe.

[4] This study can be criticized on the grounds that the excluded variables, such as the challenger's political party, and the strength of the challenger's political party in the district, are not excludable from a regression of candidate vote percent on spending levels. Jacobson has conceded that this is a problem (Jacobson 1985, 32). In 1985, Jacobson updates his 1978 work using a similar methodology.

[5] See also Kenny and McBurnett (1994).

[6] For a discussion, see Jacobson (1990) and Abramowitz (1991).

[7] I am aware of two attempts in the literature to estimate a two-stage model of Senate election outcomes. Jacobson (1978) performs the regressions described in footnote 4 for House elections using Senate election data. Stewart (1989) also estimates a model of Senate election outcomes. In his study, variables relating to the quality of the challenger, state ideological orientation, and special conditions regarding the incumbent, such as scandal or poor health, are omitted from the election prediction model to permit estimation of spending effects. The use of omitted variables such as those listed above as instrumental variables is inappropriate because they have demonstrated explanatory value in the election prediction model and therefore will be correlated with the error in the regression explaining the Senate election outcome.

[8] For another OLS analysis of Senate elections, see Grier (1989), who uses a functional form for spending effects different from that of Abramowitz and Jacobson. Grier's findings are similar to those of Abramowitz. Grier concludes that the net effect of spending in Senate elections (1978–84) was to increase challenger vote shares by around 3%.

[9] The measure of ideological distance of the incumbent from the ideological outlook of the state and the partisan makeup of the state's electorate were not available for Alaska or Hawaii, so these states were excluded from analysis.

vote. This model format is familiar in the literature. The remainder of this section briefly discusses the variables.

To capture the influence of economic conditions on the election outcome, the variables used were *State Unemployment Levels* in the election year and state unemployment levels in the election year interacted with a dummy variable that is coded as one when the incumbent was from the same party as the president (*State Employment Level *Incumbent in Governing Party*). It was expected that bad local economic conditions would help all challengers, and it would help them more if they were not from the president's party.

To measure challenger quality, the level of political experience was used.[10] Challengers were divided into five groups according to whether they had been (1) a state *Governor*, (2) a *U.S. Representative*, (3) a *Major State or Local Official*, such as attorney general or mayor of an important city, such as Pittsburgh or Indianapolis, (4) a *Minor Public Official*, such as member of a city council or a state legislature, or (5) held no previous elected office.[11]

Several variables relating to state political conditions and characteristics of incumbent senators were taken from Abramowitz's study. Abramowitz based measures of a state's partisan and ideological orientation on work by Wright, Erikson, and McIver (1985), who compiled data on state-level partisan and ideological orientation from CBS News-*New York Times* surveys conducted between 1974 and 1982.[12] The difference in the percentage of Democrats and the percentage of Republicans in each state was used to capture partisan differences across states (*State Partisanship*). An updated version of Abramowitz's measure of the *Ideological Distance* between the voting record of incumbents and the ideological leanings of their state is also included in the model.[13]

Dummy variables were included to account for special conditions that may affect the Senate race.[14] Several types of special condition were isolated. A "scandal" involves allegations of illegal activity by the incumbent (*Incumbent Scandal*). Examples are Senator Jepson, who belonged to a "health spa" used for prostitution, and Senator Brooke, who misreported his financial worth in divorce proceedings and may have been involved in Medicaid fraud. A "controversy" is an incident that raises questions about the honesty, judgment, or competence of the incumbent (*Incumbent Controversy*). Examples are Senator Hartke's excessive foreign travel and Senator Andrews's medical malpractice suit. A "health" problem occurs when the incumbent's ability to function in office appears in question (*Incumbent Health*). Examples are senators Goldwater, Dominick, and Magnuson, all of whom had difficulty walking.

Several challengers were distinguished as "celebrities" if they were well known for some reason other than politics (*Celebrity Challenger*).[15] These challengers were the astronauts Schmitt and Lousma, U.N. Representative Moynihan, Vietnam POW Thorsness, and S. I. Hayakawa, a university president made famous by a confrontation with student demonstrators.

Dummy variables were included for each party for each year in order to assess the influence of partisan tides and swings in feeling toward or away from incumbents.

The logarithm of candidate spending levels, measured in real 1974 dollars per voter, were included in the election outcome model.[16] The exact form for the spending variables is natural log (real spending per voter +.01). The constant .01 was added to real spending per voter before taking the logarithm due to the fact that, for very low spending levels, the log transformation sends the value to negative infinity.[17] The logarithmic functional form was selected, since it is a simple function consistent with the desirable features that candidate spending should have positive returns at all observed levels and should exhibit decreasing returns as the most critical tasks are attended to with the first expenditures.[18]

Instrumental Variables Used for Estimation of Spending Effects

Three instrumental variables were selected to estimate the model. The objective was to find variables likely to influence campaign spending without directly affecting the election itself. The first instrumental variable is based on a measure of *Challenger Wealth*. Richer

[10] Use of political experience as a proxy for challenger quality is a common practice in the literature on congressional elections. See, for example, Bianco (1984), Born (1986), and Jacobson and Kernell (1981).

[11] It is possible that the challenger quality variable is endogenous, if the challenger's decision to run responds to electoral weakness of the incumbent. While this may be a problem, it may be minor for two reasons. First, candidates enter well in advance of the election, and therefore entry cannot respond to factors that develop nearer the election. Second, as Squire (1991) has argued, since Senate seats are very scarce, it is difficult to time one's run; there likely will be only a few opportunities and many potential challengers.

[12] Restricting analysis to a subsample that more closely overlaps the Wright, Erikson, and McIver survey data would not alter any conclusions of this paper.

[13] This variable, which is calculated according to a formula based on voting record ratings by Americans for Democratic Action (ADA), Americans for Constitutional Action (ACA), and state ideological rankings based upon the survey data, is detailed in Abramowitz (1988).

[14] The classification of scandal, health, and controversy is similar to that used by Abramowitz (1988), with some updating for recent events, such as the savings and loan scandal's "Keating five."

[15] Westlye (1991) isolates a set of candidates for this distinction. Abramowitz (1988) also distinguishes a similar set of candidates.

[16] This treatment follows Jacobson (1985) and Grier (1989), who use spending per voter in their analysis of Senate elections. Abramowitz (1988) assumes that there are economies of scale and adjusts the denominator of the spending variable.

[17] The $5,000 added to spending levels in House elections by Green and Krasno (1988) is of very similar magnitude to an addition of $10,000 per million voters in a Senate election. Qualitatively similar TSLS results were obtained when different small constants were used.

[18] Several other functional forms were analyzed as well. The major finding of the estimation—that TSLS equalizes the marginal spending effects of incumbent and challenger spending, and that the marginal spending effects for both challenger and incumbent increase substantially over the OLS coefficients when reestimated by TSLS—held for all functional forms tested.

challengers have more money to spend on their Senate race. This variable was generated by reading through the descriptions of the upcoming Senate races contained each election year in the *Congressional Quarterly* election preview issue. Challengers were categorized as wealthy or not based upon the description.

Challenger wealth was set equal to 0 if the *Congressional Quarterly* election preview listed the challenger's occupation or former occupation as, for example, a public sector job, teacher, military, or lawyer. Challenger wealth was set to 1 if the description indicated a real estate developer, an independent business owner or president of a business, a banker, a top executive, or used terms such as "wealthy," "independently wealthy," "millionaire," or "heir." Overall, challengers' contributions to their own campaign constitute about 8% of total challenger spending. This spending is concentrated in a subset of races, and those classified as wealthy account for a large portion of challenger self-financing.[19] The mean level of real campaign spending per voter in 1974 dollars by a rich challenger was around 60% greater than the mean level of spending for other challengers.[20]

The second instrumental variable was based on state voting age population (*State Population*). If holding the office of senator allows incumbents to raise a fixed sum independent of their state population, then senators from small states can raise large sums per capita, while those from populous states will have more difficulty raising the same amount per capita. The next two paragraphs discuss reasons to expect that per-capita contributions from both individuals and PACs vary inversely with state size. Empirical evidence shows that campaign spending per capita is lower in populous states.

Senators raise many individual contributions from out of state. There is no systematic accounting of the geographic origins of campaign money, but there is some evidence of its importance. The *Washington Post* found that, in the 1984 Senate elections, 16% of all individual contributors of more than $200 were from out of state (Sorauf 1992, 47). Senators exploit this source of campaign funds: "Unlike most House incumbents, [senators] can and do raise large sums from individuals in other states. They offer much greater eminence than do House members, and some even cultivate the well-tailored, photogenic manner of celebrities. Their campaigns are the classic locus of the well-brokered reception in which the senator flies in for cocktails, smiles, handshakes, a few words, and a covey of $1000 checks" (Sorauf 1992, 90). If the fee for an appearance is not a function of the population of the senator's home state, then the amount raised per voter will vary inversely with state population.

In addition to out-of-state individual contributions, the amount raised from "investor PACs" (those classified by the FEC as trade, membership, and health organizations, corporations, labor unions, and cooperatives) does not increase with the population size of the senator's state (Snyder 1989, 1990). The favors a senator can deliver to an interest group are a function of the senator's political influence, and each senator gets only one vote, regardless of state size. The implication is that, in per-voter terms, a senator from a small state has much more to sell than a senator from a populous state.[21]

Population is a valid instrument if it is correlated with candidate spending levels but does not directly affect Senate election outcomes. Early regression analysis by Hibbing and Brandes (1983) suggested that population is inversely correlated with incumbent vote shares in Senate elections. More recent work by Westlye (1991) and Krasno (1994), however, vigorously contests this view. While both recognize the theoretical possibility that incumbents from smaller states may outperform incumbents from larger states, Westlye and Krasno show that there is no relationship between population and vote share, with the possible exception of the very largest states (e.g., California, New York, Texas). To confirm that my findings were not contaminated by any possible problems with using population as an instrument, I examined robustness by excluding the largest states from the sample. The results were unchanged.[22]

The third instrumental variable was based on *Lagged Spending* by Senate incumbents and challengers.[23] The exact variable used was the sum of spending by the incumbent and challenger in the *previous* Senate election in the state. Due to the staggered nature of Senate elections, the previous race and the current race rarely involve the same incumbent or challenger. The variable is therefore free from the criticism that might be applied to lagged spending by the same candidate, namely, that specific candidate attributes are correlated with both the regression error and past fundraising levels. Lagged spending should be correlated with the included spending variables for the current election, however, since candidates from the same states may face similar fundraising environments.

Table 1 shows summary statistics for the variables used in the empirical analysis.

[19] The FEC final reports for 1990 and 1992 show that wealthy challengers accounted for a large portion of candidate self-financing. For contested Senate races in which an incumbent ran, total unpaid loans and contributions amounted to $11.9 million. Spending by those that *Congressional Quarterly* described as affluent or rich (12% of all candidates) was $8.4 million. This implies that the latter accounted for about 70% of all candidate self-financing.

[20] In the regression results reported later, it is possible that the instrumental variable indicating challenger wealth should be divided by the state voting age population. It was found that dividing the wealth variable by state population had no significant effect on the results.

[21] A final reason that spending varies inversely with state population is that the legal limits on contributions are fixed sums, not scaled to state population.

[22] In Table 3, column 2, I present the TSLS coefficient estimates for the effect of incumbent and challenger campaign spending, based on the complete sample. The coefficients are 6.46 and −6.00 (n = 229), respectively. When the sample was restricted to states with a population of less than 6 million, the coefficients were 6.78 and −7.00 (n = 142); for 8 million or less, 7.05 and −6.61 (n = 148); for 10 million or less, 6.88 and −6.71 (n = 152). All coefficients were significant at the 5% or 1% level.

[23] This is not the variable used by Green and Krasno (1988), who selected a candidate's own lagged spending.

TABLE 1. Sample Statistics for Model Variables

Variable	Mean	Standard Deviation
Incumbent Vote Share	58.95	9.22
Incumbent Spending (1974$)	.57	.56
Challenger Spending (1974$)	.30	.40
State Partisanship	3.93	16.59
Ideological Distance	27.76	15.41
State Unemployment Level	6.62	2.11
State Unemployment Level, Incumbent in Governing Party	3.15	3.42
State Population (in millions)	3.60	3.72
Lagged Spending	1.01	1.02
	Observations Where the Variable Is 1	
	Percentage	Number
Challenger Wealth	14%	32
Governor	4.4%	10
U.S. Representative	20.1%	46
Major State or Local Official	17.9%	41
Minor Public Official	20.1%	46
Incumbent Scandal	3.1%	7
Incumbent Controversy	6.6%	15
Incumbent Health Problem	2.2%	5
Celebrity Challenger	3.1%	7

Note: There are 229 observations for all variables except lagged spending, where $N = 156$.

ESTIMATION RESULTS

This section reports the results of estimation of the Senate election model. First, I examine the results of OLS regression. I then present the results of instrumental variable estimation.

OLS Estimation Results

Table 2 reports the OLS estimation of the Senate election model for both the full sample of 229 elections and the subsample of 156 elections for which lagged spending was available. The OLS regressions show that, for any given level of spending, challenger spending has roughly twice the marginal effect of incumbent spending. To get some perspective on the coefficient magnitudes in Table 2, I examine the effect on the incumbent's share of the vote of a small change in candidate spending levels. Consider the effect of increasing spending by $300,000 in a state with a voting age population of 3 million. Since the marginal return decreases as spending increases, base levels must be specified for calculating the marginal effects. Table 1 shows that the mean value of spending per voter in 1974 dollars was 57 cents for incumbents, while the mean value for challengers was only about half that, 30 cents per voter. Using these figures as a base level, the coefficient estimates from Table 2, column 2 imply that adding 10 cents per voter to challenger spending, holding all other variables fixed, reduces the incumbent's share of the vote by 1.13%. A 10-cents-per-voter increase in incumbent spending, holding all other variables fixed, raises the incumbent's vote share by .33%. The marginal benefit is lower for the incumbent than for the challenger for two reasons. First, the coefficient estimates in Table 2 indicate that, for any given level of spending, an additional dollar is only half as effective for the incumbent as for the challenger. Second, since the benefits from campaign spending exhibit decreasing returns to scale (i.e., the logarithmic function is concave), and incumbents typically have a larger campaign budget than challengers, any spending boost by incumbents is operating at a "flatter" portion of the curve relating money spent to votes received, and so yields fewer votes.

Using the mean values of incumbent and challenger spending and the OLS regression coefficients, the total effect of candidate spending on Senate elections was also calculated. The average incumbent outspent the average challenger nearly two to one, but this larger campaign budget was more than offset by the higher marginal return to challenger spending. The total effect of campaign spending was a 4.97% reduction in the incumbent's vote share.[24]

A formal test of the hypothesis that the coefficients on challenger and incumbent spending are of equal magnitude was performed for the regressions reported in Table 2. An F-test of the restriction that challenger and incumbent spending effects are of equal size was rejected at the .01 level.

The regressions show that, for any given level of candidate spending, incumbent spending has a smaller marginal effect than challenger spending, but the marginal effect is in the intuitively expected direction and is statistically significant. These results are close to those found in Abramowitz (1988) but different from those reported by Jacobson (1985), who finds incumbent spending statistically insignificant.[25]

The preliminary conclusion from OLS regressions is that, for Senate elections, the extreme form of the incumbency spending problem, where incumbent spending has no effect, does not exist. Incumbent spending helps, but at any given level of spending, it is only about half as effective as challenger spending. If the arguments regarding the endogeneity of spending made earlier are correct, however, then OLS does not provide consistent estimates of the model. In order to

[24] The total effect of campaign spending captures how much the vote total differs from a hypothetical election in which campaign spending played no role. It is calculated as the product of the coefficient on incumbent spending times the value of the spending variable for the average incumbent minus the .01 dollars per voter base level of spending used in constructing the variable plus the product of the coefficient on challenger spending times the value of the spending variable for the average challenger, with challenger's spending level adjusted in the same manner as for the incumbent. The exact calculation of the total effect of campaign spending is 2.19*(4.06) + (−4.04)*(3.43) = −4.97.

[25] Replicating Jacobson's (1985) OLS regressions showed that the main reason for the difference between the Table 3 results and his is Jacobson's treatment of very low spending races. Jacobson regressed the incumbent vote percentage on incumbent spending, challenger spending, and a dummy variable indicating whether the challenger was a Democrat. Jacobson adds only $1.00 to the raw spending totals, and then takes the log of spending, leading to very large negative values for spending levels near zero, and a poorer fitting model.

TABLE 2. OLS Regressions of the Effects of Campaign Spending

	Dependent Variable for All Specifications Is Incumbent Share of Two-party Vote			
Independent Variable	(1)	(2)	(3)	(4)
Challenger Spending	−5.32***	−4.04***	−5.31***	−3.94***
	(.47)	(.46)	(.57)	(.58)
Incumbent Spending	2.58**	2.19**	2.78**	1.99*
	(.75)	(.69)	(.92)	(.82)
Governor	—	−2.63	—	−1.75
		(1.94)		(3.52)
U.S. Representative	—	−3.91***	—	−3.35**
		(1.02)		(1.26)
Major State or Local Official	—	−3.38**	—	−1.89
		(1.13)		(1.47)
Minor Public Official	—	−.86	—	.11
		(1.11)		(1.55)
State Unemployment Level	—	−.45	—	−.61
		(.29)		(.39)
State Unemployment Level, Incumbent in Governing Party	—	.23	—	.20
		(.42)		(.51)
Incumbent Scandal	—	−6.35*	—	−6.55*
		(2.69)		(2.82)
Incumbent Controversy	—	−3.65*	—	−6.42**
		(1.50)		(1.84)
Incumbent Health	—	−5.77*	—	−6.57
		(2.56)		(3.53)
Ideological Distance	—	−.03	—	−.03
		(.03)		(.04)
State Partisanship	—	.12***	—	.13**
		(.03)		(.04)
Number of observations	229	229	156	156
R^2	.57	.67	.55	.67

Note: Year and party dummy variables are included in all regressions. Columns 1 and 2 use the full sample, and columns 3 and 4 use the subsample for which lagged spending was available. *$p < .05$, **$p < .01$, ***$p < .001$. Standard errors are in parentheses.

get accurate measurement of the effect of spending on the incumbent's vote percentage, the estimation technique should account for the endogeneity of the spending variables.

Table 3 reports the results of TSLS estimation of the Senate election model. The instruments used in the first-stage regressions for the second-stage estimation results shown in columns 1 and 2 were the challenger wealth variable and voting age population; all three instrumental variables were used in the first-stage regressions for the second-stage results shown in columns 3 and 4.[26] Table 4 reports the reduced form estimation of the candidate spending levels.

Reestimation using instrumental variables shows an increase in the campaign spending coefficients for both challenger and incumbent, as well as an especially large increase in the effect of incumbent spending. In Table 3, column 2 shows that the marginal effects of incumbent and challenger spending are roughly equal, in contrast to the OLS results. The hypothesis that the coefficients are equal cannot be rejected even at the .10 level. One possible objection to the regressions reported in Table 3 is that the instruments may not be exogenous.[27] In order to test the assumption of exogeneity of the instruments, a test of the overidentifying restrictions was performed. The hypothesis that the instruments were exogenous could not be rejected at even the .10 level.[28]

[26] Specification tests to detect heteroscedasticity and autocorrelation were also performed. Comparing the standard error produced by OLS to those generated by White's (1980) heteroscedasticity robust estimator revealed very little evidence of heteroscedasticity. Some senators appear in the data set more than once, raising the possibility of autocorrelated errors. To explore this possibility, the correlation between residuals and six-year lagged residuals was examined. Standard tests for autocorrelation could not reject the null hypothesis of no autocorrelation, even at the .10 level.

[27] Wealthy candidates may be attractive to voters if their wealth reduces the chances of corruption, or they may be unattractive if they appear out of touch with the concerns of average people or if wealth inspires resentment. The overall importance of these effects is likely to be small, and the net direction is unclear. It should be noted that among the large body of empirical work on Senate elections, it seems that candidate wealth has never been included as an explanatory variable. Some objections may also be raised regarding the use of population as an instrument, especially if there are scale economies that are not accounted for in the model linking campaign spending to election results. It is not clear that there are important scale economies to spending, and the evidence sometimes taken to indicate their existence (spending is lower in larger states) may be sensibly explained by variation in the supply of funds available to Senate candidates.

[28] A formal test of endogeneity of spending levels was performed

TABLE 3. TSLS Regressions of the Effects of Campaign Spending

	Dependent Variable for All Specifications Is Incumbent Share of Two-Party Vote			
Independent Variable	(1)	(2)	(3)	(4)
Challenger Spending	−6.50**	−6.00**	−6.92*	−5.57*
	(2.08)	(1.88)	(2.95)	(2.68)
Incumbent Spending	7.44**	6.47**	8.78**	6.06*
	(2.27)	(2.04)	(3.30)	(2.90)
Governor	—	−2.25	—	−1.22
		(3.24)		(5.45)
U.S. Representative	—	−2.87	—	−2.11
		(2.02)		(3.42)
Major State or Local Official	—	−2.44	—	−1.15
		(1.64)		(2.56)
Minor Public Official	—	−.23	—	.67
		(1.30)		(1.95)
State Unemployment Level	—	−.41	—	−.48
		(.36)		(.49)
State Unemployment Level, Incumbent in Governing Party	—	.35	—	.01
		(.58)		(.81)
Incumbent Scandal	—	−6.40*	—	−6.44*
		(2.67)		(3.05)
Incumbent Controversy	—	−4.59*	—	−6.64*
		(2.02)		(3.20)
Incumbent Health	—	−4.02	—	−5.89
		(3.44)		(4.36)
Ideological Distance	—	−.05	—	−.07
		(.04)		(.05)
State Partisanship	—	.12*	—	.12*
		(.05)		(.06)
Number of observations	229	229	156	156
R^2	.46	.60	.41	.61

Note: Year and party dummy variables are included in all regressions. Columns 1 and 2 use the full sample, and columns 3 and 4 use the subsample for which lagged spending was available. R^2 is from the second stage regression. $^*p < .05$, $^{**}p < .01$. Standard errors are in parentheses.

Briefly examining the reduced form regressions presented in Table 4, the challenger wealth, voting age population, and lagged spending variables all had the expected signs. There was a statistically significant positive relationship between challenger spending and challenger wealth. Both incumbent and challenger spending showed a statistically significant positive relationship with lagged spending, and negative relationship with voting age population. One interesting finding is the large effect of challenger political experience on challenger spending levels. Political troubles for the incumbent due to controversies are also strongly associated with high levels of spending for both candidates.

The regressions in Table 3 show that, for any given level of spending, incumbent and challenger spending have similar marginal effects on Senate elections. These new regression coefficients can be used to recalculate the effects of spending an additional 10 cents per voter by challengers and incumbents, as well as the total effect of candidate spending on Senate election outcomes. Again, the base levels are 57 cents per voter for incumbents and 30 cents per voter for challengers. The results in Table 3, column 2, imply that increasing challenger spending by 10 cents per voter leads to a 1.69% decrease in the incumbent's vote percentage, while a similar increase by incumbents increases their vote percentage by 1.00%. In contrast to the OLS estimates, when we correct for endogeneity bias, we find that for any given level of spending, campaign spending by the incumbent and challenger have similar effects. Yet, raising spending levels a fixed amount still benefits the challenger more than the incumbent. This stems from the fact that the average incumbent has a much larger campaign budget than the average challenger, and the return to additional spending falls as the total amount spent increases.

Recalculation of the total effect of spending shows that the new coefficient estimates reverse the earlier conclusions about the influence of candidate spending on Senate elections. Contrary to the findings based on the OLS regression coefficients, the overall effect of campaign spending strongly favors the incumbent. Us-

using a regression-based version of the Hausman-Wu (Hausman 1983) specification test. The fitted values from a regression of incumbent spending and challenger spending on the instruments were included, along with incumbent spending and challenger spending in an OLS regression of the incumbent vote share model. Under the null hypothesis that incumbent and challenger spending are exogenous, the coefficients on the fitted variables should be zero. The F-test of the exclusion restriction is distributed $F(2,192)$ under the null hypothesis that spending levels are exogenous, and the null is rejected at the .01 level. The test was performed using the challenger wealth variable, voting age population, and lagged spending.

TABLE 4. Reduced Form Regressions for Candidate Spending Levels

	Dependent Variable			
Independent Variable	Challenger Spending	Incumbent Spending	Challenger Spending	Incumbent Spending
Challenger Wealthy	.79***	.13	.66**	.09
	(.20)	(.13)	(.26)	(.16)
State Population	−1.00***	−1.14***	−.74**	−.99***
	(.02)	(.13)	(.27)	(.17)
Lagged Spending	—	—	.16**	.12***
			(.05)	(.03)
Governor	1.73***	.61**	2.39***	.87**
	(.36)	(.23)	(.54)	(.33)
U.S. Representative	1.22***	.38**	1.41***	.27
	(.20)	(.13)	(.26)	(.16)
Major State or Local Official	.68**	.16	.86**	.17
	(.21)	(.14)	(.27)	(.16)
Minor Public Official	.23	.03	.33	−.04
	(.20)	(.13)	(.27)	(.17)
State Unemployment Level	−.08	−.03	−.05	−.02
	(.05)	(.04)	(.07)	(.04)
State Unemployment Level, Incumbent in Governing Party	−.07	−.02	−.09	.04
	(.09)	(.06)	(.12)	(.07)
Incumbent Scandal	−.16	−.16	−.52	−.29
	(.42)	(.27)	(.47)	(.29)
Incumbent Controversy	.76*	.55**	1.13**	.60**
	(.30)	(.19)	(.42)	(.26)
Incumbent Health	.64	−.19	.55	−.08
	(.48)	(.31)	(.65)	(.40)
Ideological Distance	.009	.010**	.005	.013**
	(.005)	(.003)	(.007)	(.004)
State Partisanship	−.022**	−.010**	−.016*	−.005
	(.006)	(.004)	(.008)	(.004)
Number of observations	229	229	156	156
R^2	.49	.57	.52	.61

Note: Year and party dummy variables are included in all regressions. Columns 1 and 2 use the full sample, and columns 3 and 4 use the subsample for which lagged spending was available. $^*p < .05$, $^{**}p < .01$, $^{***}p < .001$. Standard errors are in parentheses.

ing mean incumbent and challenger spending levels and the new regression coefficients, the total effect of spending is to boost the incumbent vote by 6.28%, in sharp contrast to the OLS figure of a 4.97% reduction. This reversal is due to the fact that, when endogeneity is taken into account, for each level of campaign spending the incumbent and challenger are roughly equally effective (i.e., the regression coefficients are of the same magnitude), rather than incumbent spending being only about half as effective as challenger spending. Since typical incumbents spend much more than their opponents, the larger campaign budget of incumbents translates into a large electoral advantage. The OLS coefficients, in contrast, imply that the advantage of the incumbent's larger campaign budget is outweighed by the greater effectiveness of challenger spending.

CONCLUSIONS AND FUTURE RESEARCH

Upon showing that the marginal effect of challenger spending in Senate elections is much larger than that of incumbent spending, Abramowitz (1988, 397) noted: "The most important conclusion about the effects of campaign spending remains secure." The main finding of this article is that the traditional view of incumbent campaign spending does not hold up when OLS regressions are reestimated using an instrumental variables approach. In fact, after taking the endogeneity of spending into account, the marginal effects of incumbent and challenger spending are statistically equivalent. This result is very robust to changes in the set of instruments. The assumptions underlying the TSLS estimation hold up very well; standard statistical tests confirm the endogeneity of candidate spending levels and the exogeneity of the instruments.

In evaluating the implications of the empirical findings, it is important to remember that Senate elections differ from other types of elections. The article's results clearly oppose the traditional view of the relative importance of incumbent and challenger spending in Senate elections. Do the findings also cast doubt on the standard theory for why incumbent spending should matter less than challenger spending?

Jacobson's original theoretical explanation for the ineffectiveness of incumbent spending and the effectiveness of challenger spending applies more forcefully to the House than the Senate. The standard argument

about the relative effectiveness of spending must be modified slightly to apply more closely to the informational environment in Senate elections. It may be that since the difference in voter information about Senate incumbents and challengers prior to the campaign is smaller than the difference between House incumbents and challengers, we should expect more equal spending effects for Senate candidates. If we suppose that Senate challengers are as well known as incumbents, then Jacobson's theory would lead us to expect no difference at all in the effect of challenger and incumbent spending. This reasoning leads to an alternative interpretation of the TSLS results for Senate elections; the findings oppose the empirically derived "rule" that incumbent spending is relatively unimportant in general and in Senate elections in particular, but the results may be consistent with the more general theory that the level of voter familiarity determines when campaign spending does and does not matter.

In fact, the findings contradict not only the generality that incumbent spending is less effective than challenger spending but also the conventional theory for why this may hold. On the first count, informational differences between House and Senate elections cannot explain the empirical finding that spending by Senate challengers and incumbents has the same effect. It is true that the voter information gap is smaller for Senate than for House candidates, but voter familiarity with Senate challengers appears to have been overestimated in early work based on the 1978 CPS/NES sample (Mann and Wolfinger 1980). The level of voter familiarity with Senate incumbents is similar to that for House incumbents, while Senate challengers are in many cases better known than the typical House challenger but often not as well known as the Senate incumbent (Westlye 1991). Thus, the informational advantage of incumbents is smaller in Senate elections, but they appear to enjoy a definite edge. This would lead to a prediction that estimates of spending effects in Senate elections might show a smaller relative advantage for challenger spending over incumbent spending than that observed in House elections, but not no advantage at all.

On the second count, I find that spending by Senate candidates, including incumbents, is very effective. According to the conventional explanation, spending by House incumbents does not increase their vote share because they are already well known. Accordingly, since Senate incumbents and, for the sake of argument let us stipulate Senate challengers, are also well known, spending by Senate candidates should have only minimal effect on the vote. On the contrary, I find that spending by both the challenger and the incumbent has large and statistically significant effects on vote shares.

To sum up, the results indicate that OLS underestimates the effectiveness of incumbent spending in Senate elections. What is more, the new estimates are not easily unified with the standard theory used to explain the difference in the effectiveness of incumbent and challenger spending levels. This implies that the contrary results of some previous work may follow from failure to account for endogeneity of spending levels, rather than from unique characteristics of Senate elections.

The finding that incumbent spending wins elections has important implications for recent American politics. Campaign finance, and specifically the level of incumbent spending, is a potentially critical factor in the competitiveness of congressional elections. The finding that incumbent spending effects are important also requires reconsideration of the consequences of campaign finance reform. The debate typically turns on what happens to challengers and neglects incumbents. Spending limits that apply to both are seen as severely biased in favor of incumbents. As Jacobson (1980, 186) argues, "campaign spending does have an important effect on who wins [congressional elections] and it is the amount spent by challengers (and other disadvantaged candidates) that actually makes the difference. Spending limits, if they have any effect at all on competition, can only work to the detriment of the challenger." In a companion paper, I conduct simulations of policy alternatives and show that, when the new estimates of incumbent spending effects are used, the conclusions inspired by the traditional view of campaign spending need major revision (Gerber 1993). For example, spending caps, even if set lower than some challengers' campaign spending levels, can significantly increase the chances of challenger victory.

Important issues remain to be addressed. First, the mechanism by which campaign spending influences vote totals is summarized here by a reduced form relationship. This may be a useful approximation of a complicated process, but additional work that attempts to isolate cases in which spending may have greater or lesser effect on election outcomes could lead to important new insights into the role of money in elections.[29] One useful direction would be to study variations in resource allocation by candidates, which would permit analysis of when different kinds of spending are most effective.[30] Finally, this paper estimates one equation in a multiequation system. Another area for research is to extend current work to examine in detail the sources of candidate funds. The goal of such analysis would be to construct and estimate a full system model linking contributors' and candidates' decisions.

[29] Exactly how campaign spending leads to more votes is an ongoing research question. Some theoretical work emphasizes the role of campaign spending in conveying information about the policy positions of the candidate and the opposition when voters are risk averse. Others have included campaign expenditures in the voter's utility function directly. See Hinich and Munger (1989) for a discussion of this literature. Alternative models presumably could emphasize credibility of communications and signalling considerations, as well as the component of spending that does not involve communication with the voters.

[30] Relatively little is known about how money is spent. This is due at least in part to difficulties in interpreting candidate expenditure reports, which allow substantial discretion in how expenditures are categorized (personal communication with Robert Biersack, statistician at the Federal Election Commission).

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Consequences of the Condorcet Jury Theorem for Beneficial Information Aggregation by Rational Agents

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"Naïve" Condorcet Jury Theorems automatically have "sophisticated" versions as corollaries. A Condorcet Jury Theorem is a result, pertaining to an election in which the agents have common preferences but diverse information, asserting that the outcome is better, on average, than the one that would be chosen by any particular individual. Sometimes there is the additional assertion that, as the population grows, the probability of an incorrect decision goes to zero. As a consequence of simple properties of common interest games, whenever "sincere" voting leads to the conclusions of the theorem, there are Nash equilibria with these properties. In symmetric environments the equilibria may be taken to be symmetric.

In addition to his contributions to the theory of elections in which the various agents have different preferences, Condorcet ([1785] 1994) established a result, known as the Condorcet Jury Theorem, giving certain conditions under which majority rule is superior to dictatorship for a society composed of individuals with identical preferences but diverse information. Specifically, Condorcet considered a two-alternative election in which the voters all have the same preferences but do not know which alternative best meets the agreed desiderata, and each voter receives a signal that provides some information about which alternative is better. He contrasted the choices made by any individual acting as a dictator, using only her own signal, with the choices made under majority rule with *sincere voting*, meaning that each voter votes for the alternative that she would choose if only her own information could be brought to bear on the choice, finding that majority rule yields decisions that are better than a dictator's. He also presented a precursor of the statistical fact now known as the *law of large numbers*, showing that the decisions made under majority rule are asymptotically perfect as the population of voters becomes large.

Recent interpretations, variations, and extensions of this result include Berg (1993), Grofman and Feld (1988), Ladha (1992, 1993), Miller (1986), and Young (1988). In these models, as in Condorcet's, it is generally the case that the probability of making the (full information) optimal choice converges to one as the number of voters goes to infinity. Federson and Pesendorfer (1998) is an interesting exception, however, showing that if, instead of majority rule, one of the choices ("convict") is selected only when the vote is unanimous, then there is a positive lower bound on the probability of making a wrong decision that is independent of the size of the electorate. (See also Coughlan [1997], who argues that unanimity may lead to attractive equilibria when there is either preplay communication, in the form of a straw vote, or the possibility of a mistrial.)

Austen-Smith and Banks (1996) point out that the pertinence of Condorcet's argument, for rational agents, depends, or at least seems to depend, on an assumption (that had rarely been discussed explicitly) that sincere voting constitutes a Nash equilibrium of the game induced by majority rule. They demonstrate that, in fact, this can easily fail to be the case.

Thus, there arises the question of the extent to which the conclusions of the Condorcet Jury Theorem continue to hold when the game induced by the voting procedure is played rationally in the sense that the profile of strategies is a Nash equilibrium. Myerson (1994b) and Wit (1996) give models in which some of the symmetric equilibria have the desired properties: Equilibrium decisions are superior to any individual's (benevolent but only privately informed) dictatorship, and the probability of an incorrect decision goes to zero as the number of agents becomes large. Ladha, Miller, and Oppenheimer (1996) give examples of asymmetric equilibria that outperform the symmetric equilibria and therefore also yield the conclusions of interest. Three papers by Fedderson and Pesendorfer (1996a, 1996b, 1996c) emphasize information aggregation by voters with common, or similar, preferences, but in settings in which there are other voters with different values. These papers display equilibria in which (asymptotically, as the population becomes large) the outcomes are those that would be chosen by a fully informed electorate. The contrasting result of Fedderson and Pesendorfer (1998), that with a unanimity requirement there is a positive lower bound on the probability of making a faulty decision that is independent of the size of the electorate, does not depend on any particular behavioral assumption, so it also pertains to rational agents.

In this article I demonstrate that, in a very general setting, when sincere voting has the desirable properties described by Condorcet, there are Nash equilibria that also have these properties. By assumption all individuals have the same preferences, so the game induced by any voting procedure is a game of common interest: There is a single common utility function. The key observation (Theorem 1 below) is that, for such a

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I thank Richard McKelvey for bringing literature on the Condorcet Jury Theorem to my attention, and for very helpful and encouraging conversations. I am also grateful to Timothy Fedderson for insightful remarks, and to Beth Allen for comments on an earlier draft.

game, a mixed strategy vector that is optimal, in the sense of maximizing the common utility function, is necessarily a Nash equilibrium. Games of common interest occur in the economic theory of teams, and this fact has been noted by Radner (1972, 195) and perhaps others as well. There can be other Nash equilibria that are not optimal. In particular, for voting games a profile of votes will be a Nash equilibrium if no single agent can reverse the outcome by changing her vote.

For the Condorcet model this observation implies that whenever sincere voting is a better aggregator than any individual's dictatorship, an optimal strategy profile is both at least as good and an equilibrium. In addition, as the population of voters increases, optimal strategy profiles yield asymptotically perfect decisions whenever sincere voting has this property.

It may happen that optimal equilibria succeed in aggregating information when sincere voting would not. For example, suppose that there are two possible states of nature, G (guilt) and I (innocence), two possible signals, also denoted by G and I, and two possible social alternatives, again denoted by G and I. Suppose that the two states have equal prior probability—the probability that an agent receives signal G when the true state is G is 2/3 and the probability that an agent receives signal I when the true state is I is 2/3—and, conditional on the state, the various agents' signals are statistically independent. Suppose G is the preferred action when G is the state, while I is the preferred action when I is the true state. If it is disastrous to choose G when I is the true state, but only mildly disadvantageous to choose I when the true state is G, then sincere voters will always vote for I. If the population is large, however, expected payoffs under majority rule will be higher if all voters vote their signal (Austen-Smith and Banks [1996] describe such behavior as *informative voting*) since the law of large numbers implies that the best alternative will be chosen with very high probability. Such behavior will not, in general, be an equilibrium (Austen-Smith and Banks 1996, 38, lemma 2), but optimal equilibria will be even more successful in aggregating information.

When the environment is symmetric, in that the agents are interchangeable, an equilibrium assigning different strategies to identical agents embodies a degree of coordination that may seem implausible when the population is large. Myerson (1994a) has argued that population uncertainty, in that the number of players of each type is random, is best modeled by a framework in which asymmetric behavior is not merely implausible but in fact very difficult to describe at all. Theorem 2 establishes that, in a symmetric common interest game, a symmetric mixed strategy profile that is optimal in the set of such profiles is a Nash equilibrium. In the Condorcet model (with majority rule, a unanimity rule for selecting one outcome, or any other supermajority rule) sincere voting is a symmetric profile of strategies, so when it yields better decisions than any individual's dictatorship, there is a symmetric equilibrium that does at least as well.

To show how the optimal profile in the space of symmetric profiles may not be optimal in the space of all profiles, and to illustrate the framework concretely, we consider the example above, with a unanimity requirement for conviction, specialized to the case of three jurors. For the point we are making it works well to assume that convicting an innocent is only 9/8 as bad as acquitting a guilty suspect, in which case society, and each juror, will be indifferent between the two possible verdicts when the posterior probability of guilt is 9/17. There is an equilibrium in which one of the jurors always votes for guilt, and the other two vote informatively. To see that the juror who votes for guilt prefers to do so even with an innocent signal, consider the circumstance in which the vote is relevant, namely when the other two jurors have voted for guilt. Even though the juror has an I signal, she "knows" that the other two jurors have G as their signals, and the resulting posterior probability that the suspect is guilty is 2/3, which is sufficient to warrant conviction. Normalizing by setting the loss resulting from acquitting a guilty suspect to 1, we may compute that the expected loss in this equilibrium is the sum over states of the prior probability of the state times the probability of making the "wrong" decision times the resulting loss:

$$\frac{1}{2}\cdot\left(\frac{1}{3}\right)^2\cdot\frac{9}{8}+\frac{1}{2}\cdot\left(1-\left(\frac{2}{3}\right)^2\right)\cdot 1=\frac{49}{144}.$$

We compare this with a symmetric equilibrium in which each agent votes for G when their signal is G and for I when the signal is I with a probability that we denote by p. All agents behaving in this manner will be an equilibrium when an agent with signal I is indifferent between the two votes, which occurs when the posterior probability of guilt, assuming the other two voters have voted for guilt, is equal to the cutoff probability:

$$\frac{9}{17}=\frac{\frac{1}{3}\left[\frac{2}{3}+\frac{1}{3}(1-p)\right]^2}{\frac{1}{3}\left[\frac{2}{3}+\frac{1}{3}(1-p)\right]^2+\frac{2}{3}\left[\frac{1}{3}+\frac{2}{3}(1-p)\right]^2}.$$

One may verify that $p = 3/4$ solves this equation. The expected loss for this equilibrium is

$$\frac{1}{2}\left[\frac{1}{3}+\frac{2}{3}(1-p)\right]^3\cdot\frac{9}{8}+\frac{1}{2}\left(1-\left[\frac{2}{3}+\frac{1}{3}(1-p)\right]^3\right)\cdot 1$$
$$=\frac{13}{32}>\frac{49}{144}.$$

A strategy in which a juror both sometimes votes G with an I signal and sometimes votes I with a G signal can be a best response only when the juror is never pivotal. Using this fact to reduce the number of cases, it is fairly easy and straightforward to show that the only other symmetric equilibrium has all agents voting for I with probability 1, regardless of signal. The expected loss resulting from this profile is $1/2 > 13/32$, so the symmetric equilibrium above is optimal in the set of symmetric equilibria, and from Theorem 2 it

follows that it is optimal in the set of symmetric profiles.

On the whole the results presented here can be seen as reviving the relevance of the Condorcet Jury Theorem to rational agents: A "sincere" statistical version of the result implies a "rational" strategic version. The endorsement of majority rule obtained in this way is, however, in a sense, weaker than in Condorcet's original formulation, since there are many voting procedures other than majority rule for which some mode of behavior leads to decisions that are superior to any individual's dictatorship (or asymptotically perfect decisions for large populations), which is all that is required for our results to be applicable. Passing from sincere to rational agents also raises, or at least reinforces, the criticism that the models of democracy and dictatorship considered here are both artificially encumbered with an assumption that the agents are unable to communicate prior to the voting or decision stage. Unfortunately, the strategic analysis of communication is in its infancy—Coughlan's (1997) introduction of a straw ballot is an initial step in this direction and representative of the state of the art—so there is little to be said in response at this time.

Since optimal equilibria of common interest games have not been extensively studied elsewhere, it seems appropriate to consider here their status vis-à-vis the extensive literature (e.g., van Damme 1987) that considers "refinements" of the Nash equilibrium concept. Although optimal equilibria of common interest games are, in an obvious sense, desirable, this is not the same as saying that they represent plausible modes of behavior. A refinement of Nash equilibrium is a solution concept that distinguishes some Nash equilibria from the rest, typically by requiring that the "good" equilibria be robust with respect to various types of perturbations of the environment, with the hope that robust equilibria will be more plausible or likely. Here we analyze the optimal (asymmetric) profiles of mixed strategies from the point of view of strategic stability, as defined by Kohlberg and Mertens (1986). Specifically, Theorem 3 asserts that the set of optimal equilibria contains stable sets of equilibria. Since stability is one of the most demanding refinements, it follows that there are always optimal equilibria that are not excluded by other known refinements.

The remainder of this article is organized as follows: The next section introduces basic notation, which is essentially all that needs to be done to make our main point precise. The following section demonstrates the existence of suitable symmetric equilibria in symmetric environments. The penultimate section presents the result on stability, and the final section briefly considers the extent to which our framework might be more general.

OPTIMAL PROFILES OF VOTING STRATEGIES

We now lay out notation for a general voting environment for agents with common preferences and diverse information. In the descriptions of both this object and the derived normal form game, we emphasize the perspective of agents before they have learned their type, in order to make the common interest property more obvious. The points of view of agents who know their types can obscure this, since expected utilities are conditioned on different information and do not obviously express the commonality of preferences.

Fix a finite set of *alternatives* A, a set of *agents* $I = \{1, \ldots, n\}$, spaces of *types* $T_1, \ldots, T_n$, and a probability distribution P on $\mathbf{T} := T_1 \times \ldots \times T_n$, which is interpreted as the common prior belief concerning the distribution of types. Each agent is informed of her type but does not observe the types of the other agents. Assume throughout that each type of each agent has positive probability under P, so that Bayesian posterior beliefs for each type of each agent are well defined. Fix a function $u : \mathbf{T} \times A \to \mathbf{R}$, which is interpreted as the common von Neumann-Morgenstern utility function of all agents.

A *voting scheme* consists of nonempty finite sets $V_1, \ldots, V_n$ of allowed votes and an *aggregation rule* $f : \mathbf{V} \to A$ where $\mathbf{V} := V_1 \times \ldots \times V_n$. The tuple

$$((T_1, \ldots, T_n), P, A, u, (V_1, \ldots, V_n), f)$$

is called a *voting environment.*

A (pure) *voting strategy* for agent i is a function $s_i : T_i \to V_i$, and a *voting profile* is an n-tuple $s = (s_1, \ldots, s_n)$ of voting strategies, one for each agent. Typically, each type of each agent will have a unique favorite alternative, prior to learning anything about the types of other agents, but nothing here depends on this. In general, a map $c_i : T_i \to A$ is *sincere* if

$$(\forall t_i \in T_i)\, c_i(t_i) \in \operatorname{argmax}_{a \in A} \mathbf{E}(u((t_1, \ldots, t_n), a) | t_i).$$

A voting profile s is said to be *acceptable for f,* in the sense that otherwise some individual's privately informed decisions would be unanimously (weakly) preferred, if, for all i and all sincere $c_i : T_i \to A$,

$$\mathbf{E}(u((t_1, \ldots, t_n), f(s_1(t_1), \ldots, s_n(t_n)))) > \mathbf{E}(u((t_1, \ldots, t_n), c_i(t_i))).$$

(Due to symmetry, in Condorcet's formulation all potential dictators are equally adroit. It seems closest to the spirit of his argument to compare the performance of the voting mechanism with the performance of the most effective potential dictator, but everything below would be equally valid, *mutatis mutandi,* if we took the most inept potential dictator, or, for that matter, any of a variety of notions of an "average" dictator, as the standard of comparison.) We extend this terminology to profiles of mixed voting strategies (viewed here as mixtures over pure voting strategies, though behavior strategies [Kuhn 1953] would be equivalent) in the obvious way, by requiring that the induced expected utility is greater than what any agent could attain with only private information.

Since Condorcet's theorem, and its variants and extensions, require a notion of sincere voting, the general notion makes sense only when $V_i = A$ for all i. We might say that a *Condorcet Jury Theorem of the first*

type is any result stating that any profile $(c_1, \ldots, c_n)$ of sincere maps is an acceptable aggregator. A *Condorcet Jury Theorem of the second type* is a result that considers a sequence of voting environments in which the number of voters goes to infinity, and which asserts that sincere voting yields probabilities of suboptimal decisions that go to zero asymptotically.

We now introduce notation for (normal form) common interest games for the given set of agents I. For each i let S_i be a finite set of *pure strategies*, let $\mathbf{S} := S_1 \times \ldots \times S_n$ be the set of *pure strategy profiles*, and let $U : \mathbf{S} \to \mathbf{R}$ be a function that is interpreted as the common von Neumann-Morgenstern utility function of all agents in I. Such a framework is derived from the general voting environment above by letting $S_i = A^{T_i}$ be the set of voting rules for each agent i, and letting

$$U(s_1, \ldots, s_n) = \mathbf{E}(u((t_1, \ldots, t_n), f(s_1(t_1), \ldots, s_n(t_n)))).$$

The set of mixed strategies for i is denoted by $\Delta(S_i)$, with typical elements $\mu_i, \mu_i', \ldots$. The set of *mixed strategy profiles* is $\Sigma := \Delta(S_1) \times \ldots \times \Delta(S_n)$. Abusing notation, we let U denote also the mixed extension of U, that is, the unique multilinear function $U : \Sigma \to \mathbf{R}$ that agrees with the original U when we identify the elements of $\mathbf{S}$ with the corresponding vertices of Σ.

We reiterate our main finding.

THEOREM 1: *For a game of common interest given by* $S_1, \ldots, S_n$ *and* U, *if*

$$(\mu_1^*, \ldots, \mu_n^*) \in \operatorname{argmax}_{(\mu_1, \ldots, \mu_n) \in \Sigma} U(\mu_1, \ldots, \mu_n),$$

then $(\mu_1^*, \ldots, \mu_n^*)$ *is a Nash equilibrium.*

Proof: For each i the hypothesis clearly implies that

$$\mu_i^* \in \operatorname{argmax}_{\mu_i \in \Delta(S_i)} U(\mu_1^*, \ldots, \mu_{i-1}^*, \mu_i, \mu_{i+1}^*, \ldots, \mu_n^*). \qquad \textit{Q.E.D.}$$

If the common interest game is derived from a voting environment as above, and there is any strategy vector that is acceptable for the aggregation rule f, then $(\mu_1^*, \ldots, \mu_n^*)$ must also be acceptable for f. In particular, a Condorcet Jury Theorem of the first type implies the existence of a Nash equilibrium that is an acceptable aggregator. If, along some sequence of voting environments, the probability of a correct decision under any sequence of strategy vectors goes to one, then the probability of a correct decision under any optimal mixed strategy will also converge to unity. Thus, a Condorcet Jury Theorem of the second type implies the existence of a sequence of Nash equilibria for which the probability of a correct decision goes to one.

SYMMETRY

We now consider symmetric voting environments. The aggregation rule f is *anonymous* if $V_i =: V$ is the same for all i and the chosen alternative depends only on the numbers of agents casting the various possible votes. More formally, $f(v_{\sigma(1)}, \ldots, v_{\sigma(n)}) = f(v_1, \ldots, v_n)$ for all $(v_1, \ldots, v_n) \in V^n$ and all permutations $\sigma : \{1, \ldots, n\} \to \{1, \ldots, n\}$. The notion of symmetry relevant to our analysis also requires that the information structure and u are symmetric in the sense that $T_1 = \ldots = T_n =: T$ and

$$P(t_{\sigma(1)}, \ldots, t_{\sigma(n)}) = P(t_1, \ldots, t_n)$$

and

$$u((t_{\sigma(1)}, \ldots, t_{\sigma(n)}), a) = u((t_1, \ldots, t_n), a)$$

for all $(t_1, \ldots, t_n) \in T^n$, all permutations σ, and all $a \in A$. We say that the voting environment is *symmetric* when all these symmetry conditions are satisfied.

Turning to the common interest game given by $S_1, \ldots, S_n$ and U, we say that this game is symmetric if $S_1 = \ldots = S_n =: S$ and

$$U(s_{\sigma(1)}, \ldots, s_{\sigma(n)}) = U(s_1, \ldots, s_n)$$

for all $(s_1, \ldots, s_n) \in S^n$ and all permutations σ. I leave to the reader the conceptually simple, but tedious, algebraic exercise of showing that a symmetric voting environment induces a symmetric common interest game. When the game given by $S_1, \ldots, S_n$ and U is symmetric we may say that a mixed strategy profile $(\mu_1, \ldots, \mu_n)$ is symmetric if $\mu_1 = \ldots = \mu_n$.

THEOREM 2: *Suppose that the game given by* $S_1, \ldots, S_n$ *and* U *is symmetric. If a symmetric mixed strategy profile* $(\mu, \ldots, \mu)$ *is maximal for U in the set of such profiles, then it is a Nash equilibrium.*

Since the space of symmetric mixed profiles is compact, the existence of a symmetric equilibrium follows as a corollary, but this is a (very) special case of a result of Nash (1951).

Proof: If $(\mu, \ldots, \mu)$ is not Nash, then there is a mixed strategy υ such that $U(\upsilon, \mu, \ldots, \mu) > U(\mu, \ldots, \mu)$. Applying multilinearity and symmetry, we find that $(\mu, \ldots, \mu)$ is inferior to the symmetric strategy vector obtained by having each agent combine a high proportion of μ with a small bit of υ, contrary to hypothesis. Specifically, for all $\varepsilon \in [0, 1]$ we have

$$U((1-\varepsilon)\mu + \varepsilon\upsilon, \ldots, (1-\varepsilon)\mu + \varepsilon\upsilon) = U(\mu, \ldots, \mu) + n\varepsilon(U(\upsilon, \mu, \ldots, \mu) - U(\mu, \ldots, \mu)) + \varepsilon^2 P(\varepsilon),$$

where P is a polynomial function of ε, so

$$U((1-\varepsilon)\mu + \varepsilon\upsilon, \ldots, (1-\varepsilon)\mu + \varepsilon\upsilon) > U(\mu, \ldots, \mu)$$

for sufficiently small $\varepsilon > 0$. *Q.E.D.*

Therefore, to produce an equilibrium symmetric (mixed) strategy vector that is acceptable for f, it suffices to prove the existence of a (possibly nonequilibrium) symmetric mixed strategy vector with this property. For this condition to make sense the model must give all agents the same set of allowed votes, but again this set need not be A.

STABILITY

Especially when they are asymmetric, the optimal strategy profiles may prescribe behavior that is complex and highly coordinated, or implausible on other grounds. Nevertheless, these profiles do survive the criteria posed by the literature on refinements of Nash equilibrium, as we now explain.

If $\hat{\sigma} \in \Sigma$ and $\varepsilon = (\varepsilon_1, \ldots, \varepsilon_n) \in (0, 1)^n$, the *perturbed game* induced by $(\hat{\sigma}, \varepsilon)$ is the common interest game with the original pure strategy sets and the perturbed utility function

$$U_{(\hat{\sigma},\varepsilon)}(s) := U((1 - \varepsilon_1)s_1 + \varepsilon_1\hat{\sigma}_1, \ldots, (1 - \varepsilon_n)s_n + \varepsilon_n\hat{\sigma}_n).$$

Recall (Selten 1975) that a mixed strategy profile σ^* is a *perfect equilibrium* if there are sequences $\{\hat{\sigma}^r\}$, $\{\varepsilon^r\}$, $\{\sigma^r\}$ such that: (a) for each r, σ^r is a Nash equilibrium of the perturbed game induced by $(\hat{\sigma}^r, \varepsilon^r)$; (b) $\varepsilon_i^r \to 0$ for each i; (c) $\sigma^r \to \sigma^*$. A compact set E of Nash equilibria is *robust with respect to trembles* if, for any neighborhood W of E, there is $\delta > 0$ such that for all $\hat{\sigma}$ and ε with $\varepsilon_i < \delta$ for all i, W contains an equilibrium of the perturbed game induced by $(\hat{\sigma}, \varepsilon)$. The arguments below use the (obvious) fact that if E is robust with respect to trembles, and F is a compact set of Nash equilibria that contains E, then F is robust with respect to trembles. If E is robust with respect to trembles and has no compact proper subset that is robust with respect to trembles, then E is *stable* (Kohlberg and Mertens 1986). Ideally, one would like to find stable singletons (when this happens the unique element is said to be a *strictly perfect equilibrium*), but it is possible that there are none (e.g., §1.5 of van Damme 1987).

Our interest in stable sets of equilibria is not predicated on a belief that stability is, in some sense, the "correct" or "ideal" refinement of Nash equilibrium. It is a very demanding criterion, in that stable sets necessarily contain equilibria satisfying a variety of other refinement criteria, so the result below reinforces the pertinence of optimal equilibria from a variety of points of view. In addition, stability has interesting relationships with analyses of equilibrium selection motivated by considerations quite different from those adduced by Kohlberg and Mertens; for an example see Swinkels (1992).

Let $M := \max_{(s_1,\ldots,s_n)\in S} U(s_1, \ldots, s_n)$ and let $K := U^{-1}(M)$ be the set of optimal mixed strategy profiles. It is interesting to note that K can be characterized as the union of the various sets $\Delta(R_1) \times \ldots \times \Delta(R_n)$ where $R_1, \ldots, R_n$ is a collection of subsets of $S_1, \ldots, S_n$, respectively, such that $U(r_1, \ldots, r_n) = M$ whenever $r_1 \in R_1, \ldots, r_n \in R_n$.

THEOREM 3: K *is robust with respect to trembles.*

The following argument is another application of this article's key insight: The perturbed game induced by $(\hat{\sigma}, \varepsilon)$ is a game of common interest, so an optimal strategy vector must be an equilibrium. If the perturbation is small, then the utility obtained from such an optimal strategy vector cannot be much less than the optimal utility of the original game, so the strategy vector must be close to the set of strategy vectors yielding this utility.

Proof: For a given neighborhood W of K choose $\gamma > 0$ small enough that $K_\gamma := \{\sigma \in \Sigma : U(\sigma) > M - \gamma\}$ is contained in W. Choose $\delta > 0$ small enough that

$$U(\sigma) - \gamma/2 < U_{(\hat{\sigma},\varepsilon)}(\sigma) < U(\sigma) + \gamma/2$$

for all σ, $\hat{\sigma}$, and ε with $\varepsilon_1, \ldots, \varepsilon_n < \delta$. Fixing $(\hat{\sigma}, \varepsilon)$ with $\varepsilon_1, \ldots, \varepsilon_n < \delta$, let σ^* be a maximizer of $U_{(\hat{\sigma},\varepsilon)}(\cdot)$. Just as earlier, σ^* is clearly a Nash equilibrium of the perturbed game induced by $\hat{\sigma}$ and ε. We have $U_{(\hat{\sigma},\varepsilon)}(\sigma) > M - \gamma/2$ whenever $\sigma \in K$, so $U_{(\hat{\sigma},\varepsilon)}(\sigma^*) > M - \gamma/2$ and thus $U(\sigma^*) > M - \gamma$, whence $\sigma^* \in K_\gamma \subset W$, as desired. *Q.E.D.*

COROLLARY: K *contains a stable set of equilibria.*

Proof: Let $\{O_1, O_2, \ldots\}$ be a countable base[1] of the topology of Σ. Set $K_0 := K$, and define $K_1, K_2, \ldots$ inductively by setting $K_{j+1} := K_j \backslash O_j$ if $K_j \backslash O_j$ is robust with respect to trembles, and otherwise setting $K_{j+1} = K_j$. Then each K_j is compact and robust with respect to trembles. Clearly $K_\infty := \cap_j K_j$ is also compact and nonempty. (One may take a convergent subsequence of a sequence $\{x_j \in K_j\}$.) If K_∞ was not robust with respect to trembles, it would have a neighborhood W such that for all $\delta > 0$ there was some $(\hat{\sigma}, \varepsilon)$ with $\varepsilon_i < \delta$ for all i, such that the perturbed game induced by $\hat{\sigma}$ and ε had no equilibria in W. But $K_j \subset W$ for large j (else the limit of a convergent subsequence of a sequence $\{x_j \in K_j \backslash W\}$ would be a point in $K_\infty \backslash W$) contrary to the presumed robustness of K_j. Therefore, K_∞ is robust with respect to trembles. If K_∞ is not minimal in the class of compact sets that are robust with respect to trembles, then, for some j, $K_\infty \backslash O_j \neq K_\infty$ is robust with respect to trembles. But $K_\infty \subset K_j$, so $K_j \backslash O_j$ is robust with respect to trembles and $K_\infty \subset K_{j+1} = K_j \backslash O_j$, so that $K_\infty = K_\infty \backslash O_j$. This contradiction completes the proof. *Q.E.D.*

CONCLUDING REMARKS

Certainly the framework of this article is very general in comparison with most models considered in related literature. Additional generality can be attained easily by allowing voting schemes that yield nontrivial lotteries over the set of alternatives; this has no effect on the arguments. In particular, by identifying a probability that an agent does not vote with a probability that her vote does not count, one may represent situations in which the population of voters is random. From a mathematical point of view, the main losses of generality in the framework are those associated with the finiteness of the sets of agents, types, and alternatives. I hope that mathematically sophisticated readers will agree that the arguments underlying theorems 1 and 2 are not critically dependent on these assumptions, at least in the sense that only minor and mundane tech-

[1] That is, any open set is a union of members of $\{O_1, O_2, \ldots\}$. For example, we may take the open balls of rational radii whose centers have rational coordinates.

nical details arise in the extensions to several sufficiently well behaved infinite dimensional settings.

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Civility, Enlightenment, and Society: Conceptual Confusions and Kantian Remedies

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Critics of what is called the "Enlightenment project" have argued that it has been responsible for a number of current social pathologies. At the same time, the term "civil society" has been used to designate those patterns of solidarity that the Enlightenment project allegedly disrupts. This article (1) argues that characterizations of the Enlightenment project tend to be elusive and historically questionable, (2) suggests that the concept of civil society is ambiguous in both its object and its intent, (3) explores how Kant provided a more rigorous account of the relationship between enlightenment and civil society, an account which rests on a contrast between civil and cosmopolitan society, and (4) considers some of the difficulties that plague attempts to define "civility" as a virtue.

Writing in the *Berlinische Monatsschrift* in 1784, Immanuel Kant speculated that the "hidden plan of nature" was to bring about justice in civil society and peaceful relations between nations by means of the very antagonism that seemed to promise only civil unrest and international conflict. He assured his readers that "though folly and caprice creep in at all times, *enlightenment* gradually arises," and thus the "chiliastic expectations" of philosophy for the triumph of justice were something more than an illusion (Kant [1784] 1923, 27–8). As we close this millennium, Kant's "chiliastic expectations" regarding enlightenment and civil society do not appear to be generally shared.

Over the last several decades, a remarkably diverse group of philosophers and social critics have traced the origin of a number of social and cultural maladies to something they call the "Enlightenment project." Over the same period, an equally diverse group of political and social commentators have hailed the promise of, lamented the demise of, or sought ways to reinvigorate what they call "civil society." The concerns at stake in these discussions would appear to be related. As Kant's essay shows, the nature and promise of civil society were important concerns in the Enlightenment. Yet, with a few exceptions, current debates over the viability of the Enlightenment project and discussions of the prospects for civil society are conducted in different registers. On those rare occasions when the relationship between the two notions is broached, some commentators tend to see greater difficulties than Kant.

Consider, for example, the explanation that Berger and Neuhaus ([1976] 1996, 161) offer for the tendency of liberalism to underestimate the role played by "mediating structures" in public policy:

> Liberalism's blindness to mediating structures can be traced to its Enlightenment roots. Enlightenment thought is abstract, universalistic, addicted to what Burke called "geometry" in social policy. The concrete particularities of mediating structures find an inhospitable soil in the liberal garden. There the great concern is for the individual ("the rights of man") and for a just public order, but anything "in between" is viewed as irrelevant, or even an obstacle, to the rational ordering of society. What lies in between is dismissed, to the extent it can be, as superstition, bigotry, or (more recently) cultural lag.[1]

This argument repeats a long-standing charge against the Enlightenment: While it proved effective in tearing down what tradition had wrought, it was unable to foster those mediating structures which sustain civil society. This was the common theme of such otherwise dissimilar works as Burke's *Reflections on the Revolution in France* ([1790] 1987) and Tocqueville's *The Ancien Régime and the French Revolution* ([1856] 1955). Such an argument also played a central role in Hegel's account of the relationship between the Enlightenment and the Terror in the *Phenomenology of Spirit* ([1806] 1977). Something similar may be found in Nietzsche's suggestion that the Enlightenment's critique of religion was but the first step toward a critique of the idols of science, morality, and reason that carried with it the prospect of a collapse into nihilism.

This paper proposes that arguments of this sort rest on confusions about what the Enlightenment involved and what civil society implies. They invoke a characterization of the Enlightenment that is historically questionable and an understanding of civil society that is rarely clearly defined. To understand what is amiss, it will be necessary to (1) examine the radically different ways in which the Enlightenment is understood by social critics and by historians, (2) explore some of the ambiguities that plague the concept of civil society, (3) return to Kant, in order to consider an alternative way of thinking about the relationship between Enlightenment and civil society, and (4) consider some of the difficulties that plague accounts of the virtue of civility.

CRITICIZING THE ENLIGHTENMENT PROJECT

In recent decades the notion that there is something short-sighted, narrow, and deficient about the Enlight-

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This article is drawn from a paper presented at a UNESCO Conference, "Future Ethics," held at the Institute of Cultural Pluralism, Candido Mendes University, Rio de Janeiro, July 2–4, 1997. I am indebted to Enrique Larreta, the executive director of the ICP, for inviting me to draw some connections.

[1] For a discussion of the significance of Berger and Neuhaus's notion of "mediating structures" for current discussions of civil society, see Dionne 1997.

enment has become so pervasive that many commentators, as Wilson (1987, 53) observed, find it impossible to resist the temptation to

> take a punch at enlightenment thought. . . . It is difficult to get through an academic day without having to witness somebody jabbing away just to enjoy the pleasure of getting off on the demystification of the functioning of humanist ideals or slapping the Enlightenment around with the intention of knocking it out of its stupor and redeeming the hopes of mastery of nature by reason.

Wilson's quip highlights one of the more distinctive features of recent criticisms of the Enlightenment project: Today, punches are thrown from the Left as well as the Right, and the Enlightenment stands accused not only of undermining tradition but also of reinforcing patriarchy, fostering anti-Semitism, sustaining an ideology of white supremacy, embracing a vision that sees progress only in terms of the ever-increasing subjugation of nature, and—more generally—harboring a hostility toward "otherness" in any of its forms (Schmidt 1996, 1). As a way of sorting out the different charges that have been raised, it might be useful to outline, briefly, three different versions of the critique of the Enlightenment project.

The first and perhaps most familiar line of criticism argues that the indifference of Enlightenment rationality to traditional norms and practices destroys the ties that bind individuals together into societies, paving the way for complete anarchy or the rule of brute force. The locus classicus for this interpretation is Burke's *Reflections on the Revolution in France*. Criticizing French "literary men and politicians" as having "no respect for the wisdom of others," Burke argues that the fatal flaw of the Enlightenment lay in its habit of "exploding general prejudices" rather than seeking "to discover the latent wisdom which prevails in them." Because "prejudice, with its reason," is capable of moving men to action in a way that "naked reason" cannot, it is wiser "to continue the prejudice, with the reason involved, than to cast away the coat of prejudice and leave nothing but the naked reason" (Burke [1790] 1987, 76–7). More recent versions of this line of criticism may be found in Oakeshott's ([1962] 1991) critique of political "rationalism" or Gadamer's ([1960] 1989) critique of the Enlightenment's "prejudice against prejudice." We also find it in passing swipes at the Enlightenment by conservative and neoconservative commentators.

A second line of criticism originates in Hegel's discussion, in the *Phenomenology of Spirit*, of the transformation of Enlightenment into Terror. While much of Hegel's imagery bears a striking resemblance to Burke, his argument differs in crucial ways. Where Burke sought to defend the reason of tradition against the abstractions of philosophers, Hegel argues that the central failing of the Enlightenment was that it was insufficiently enlightened about the limitations of its own conception of reason. The struggle between faith and Enlightenment turns out to be little more than shadow boxing, since all that Enlightenment can offer in place of religious faith is a faith of a different sort: a belief in the primacy of the category of utility. Enlightenment proves to be "irresistible" in its struggle with faith, but its victory is a bitter one, as is amply demonstrated, in Hegel's view, by the collapse of the French Revolution into Terror. A politics guided by utility alone can produce nothing except "the coldest, shallowest of deaths, with no more significance than cleaving a cabbage head or swallowing a gulp of water" (Hegel [1806] 1977, 360, translation modified).[2] A more recent version of this line of criticism can be found in the account of the self-destruction of Enlightenment sketched by Horkheimer and Adorno ([1947] 1972).[3]

A third line of criticism originates in the work of Nietzsche, whose stance toward the Enlightenment reflects a profound ambivalence. In some of his discussions of the Enlightenment he calls for the rescue of what he characterizes as its original aristocratic ideals from the contaminating influences of democracy and egalitarianism that he associates with Rousseau and the French Revolution (Nietzsche [1878] 1986, 169, 367; [1882] 1974, 293; [1888] 1968b, 101–2). On other occasions, however, he suggests that the Enlightenment's critique of religion was but the first faltering step in the direction of a thoroughgoing critique of the idols of science, morality, and reason, and this carried within it both the danger of a collapse into nihilism and the promise of a new Enlightenment in which the idea of aesthetic self-fashioning would replace the search for universal values (Nietzsche [1882] 1974, 181; [1883–88] 1968a, 56). Nietzsche's present-day heirs are a motley lot, encompassing both those who, like Gray (1995), share his conviction that the Enlightenment paves the way to nihilism but who reject his aestheticization of ethics and those who, like Foucault, are more concerned with exploring the workings of power and knowledge than with meditating on the danger of nihilism (Schmidt and Wartenberg 1994).

Each of these lines of criticism has a different sense of what ought to be done in the face of the damage allegedly wrought by the Enlightenment. The solution of latter-day Burkeans is perhaps the most familiar: They counsel a defense of traditional norms where they still survive, a distrust for any social policy that smacks of rational "social engineering," and support for measures that strengthen the damaged "mediating institutions" of civil society. Those who take Hegel's account as their point of departure, in contrast, are concerned to elaborate a conception of rationality that seeks to transcend the limits of instrumental reason (Habermas 1987). And Nietzsche's present-day disciples may, like Gray, embrace Heidegger's notion of *Gelassenheit* as a means of avoiding the nihilism they see inherent in the Will to Power (Gray 1996, 182–3) or seek, as Foucault did in his last writings, to redefine Enlightenment as an open-ended project of self-creation (Foucault 1984,

[2] For a discussion of the peculiar images Hegel employs, see Schmidt 1998.

[3] Horkheimer himself saw the work as an elaboration of Hegel's basic thesis. See his letter to Friedrich Pollock of May 7, 1943 (Horkheimer 1996, 446).

32–50). But if the proposed antidotes for the damage done by the Enlightenment are remarkably diverse, the picture of the Enlightenment that emerges from these critics is strikingly consistent. Let us review its main features.

First, the Enlightenment is routinely characterized as embracing a conception of reason that is held to be insufficiently sensitive to its own limits. Sometimes it is criticized for underestimating the degree to which norms and values can be detached from traditions and conventions of everyday life, resulting—as MacIntyre (1981) has argued—in a situation in which moral discourse has lost all meaning. Other critics charge that it embraces a restricted understanding of reason, modeled on the natural sciences, which equates reason with the successful manipulation of nature, resulting in the rule of "instrumental reason" (Horkheimer 1947). Still others suggest that it naively underestimated the role played by passions and sentiments in human conduct and, as a result, leads to a wildly impractical politics, which Oakeshott ([1962] 1991, 27–31) characterized as a "politics of the book" or an "abridgement of a tradition." Whatever the particular form of the criticism, the central charge remains the same: The Enlightenment represents an uncritical rationalism that is dangerously unaware of the complexity of reality.

Closely related is the charge that, because the Enlightenment loses itself in abstract universality, it has an ignorance of, or worse still, a contempt for the particular, the local, the specific. According to Oakeshott ([1962] 1991, 9), the "political rationalism" that is the bitter legacy of the Enlightenment combines a "politics of perfection" with a "politics of uniformity." Horkheimer and Adorno ([1947] 1972, 6) provide an even more chilling image: Enlightenment, in their view, is inherently "totalitarian." Critics from the Left have charged that its talk of universal rights remained oblivious to inequalities in gender, race, and class, while those on the Right argue that by reducing all social relations to a series of abstract and impersonal rights, it tears the fabric of society to pieces. In all these cases, the Enlightenment stands accused not simply of being uncritically rationalist but of being insufficiently concerned with particularity.

It is further argued that the abstract, uncritical conception of reason embraced by the Enlightenment culminates in an obsession with domination and control. Thus, in Foucault's *Discipline and Punish* (1979), the prisoners who have been freed from the darkness of the dungeon are captured all the more securely in the light that floods through the Panopticon. In Horkheimer and Adorno's *Dialectic of Enlightenment* ([1947] 1972), the ultimate project of the Enlightenment is the domination of nature and of other human beings. And for Gray (1996, 166, 180), the Enlightenment project is yet another manifestation of that "Will to Power" which lies at the heart of the "Western humanist hubris."

Finally, the Enlightenment's critics are united in the conviction that there *is* such a thing as *an* Enlightenment project—that it is possible to attribute a common set of intentions to a rather diverse group of thinkers, scattered across a number of different nations. Critics may differ on how exactly this project is to be defined, just as there is considerable difference, from critic to critic, as to who counts as a representative of it. But central to all these criticisms is the conviction that it makes sense to speak of a single, coherent Enlightenment project. While such a possibility is taken as unproblematic by the critics, their confidence is hardly reflected in historical accounts of the period.

In a recent overview, Outram (1995) observed that as historians of the Enlightenment have moved from an examination of a limited canon of works by a few famous (and typically French) authors to a consideration of the Enlightenment as a social, political, and cultural transformation exhibiting a remarkable range of national and confessional variations, it has become more difficult to speak of the Enlightenment as "a unitary phenomenon." "It might . . . seem that as our picture of the Enlightenment became more complex, as we have begun to study ideas not as autonomous, discrete objects, but as deeply embedded in society, so the term Enlightenment itself might have become increasingly obscure or even meaningless" (Outram 1995, 12).

To the extent that it is still possible to impute an identity to the Enlightenment, Outram (1995, 12) suggests that it must be viewed "as a *capsule* containing sets of debates, stresses and concerns, which however differently formulated or responded to, do appear to be characteristic of the way in which ideas, opinions and social and political structures interacted and changed in the eighteenth century." This diversity is precisely what the critics tend to overlook when they speak as if there were a single, unitary Enlightenment project.

Gray's *Enlightenment's Wake* (1995) exemplifies how far afield a critic can go when trying to define the Enlightenment project. Turning first to MacIntyre's *After Virtue* (1981), he argues that while Enlightenment thinkers may have held differing views on other topics, they were at one in calling for "an independent rational justification of morality" (Gray 1995, 147). Rather quickly, he extends the Enlightenment project to embrace the "refounding" of "society" as well as "morality" on "universal, tradition-independent rational principles," a project to which "liberalism as a doctrine was, in all of its varieties, unreservedly committed" (pp. 149–50). A few pages later, the "Enlightenment project of human self-emancipation" is characterized as the "naturalistic form" of the "most fundamental Western commitment, the *humanist* conception of humankind as a privileged site of truth," a commitment that had been expressed in both "Socratic inquiry and in Christian revelation" (p. 155). Having equated the Enlightenment project with liberalism and humanism, Gray next characterizes it as also embracing the "modernist world-view," with its "conception of science as the supremely privileged form of knowledge," a supremacy that is, in turn, defined in terms of a view of the natural world as "an object of human exploitation" (p. 158). Within a few more pages, a "commitment to rationalism" becomes one of the "defining elements of the modernist world-view of which the Enlightenment

project is the most powerful expression" (p. 160). Several paragraphs later, the Enlightenment is characterized as "foundationalist," "representationalist," and "logocentric" (p. 160).

In Gray's account, the definition has so many differing characteristics that it is uncertain whether it makes sense to speak of a single, all-encompassing project. The attempt to provide "an independent rational justification of morality" (the characterization of the Enlightenment project that Gray takes from MacIntyre) does not require or even imply a view of the natural world as "an object of human exploitation" (a characterization Gray takes from Heidegger and from Horkheimer and Adorno). While the primacy accorded to the natural sciences merges rather easily into the project of the domination of nature, it is not entirely obvious that an "independent rational justification of morality" must ultimately be grounded in the natural sciences: For a thinker such as Kant, it obviously was not. It is also less than clear that "liberalism" is necessarily part of the Enlightenment project: Not all advocates of Enlightenment embraced liberalism, and there were eloquent defenders of liberalism who were also vigorous critics of the Enlightenment.[4] It is easy enough to make the "foundationalist," "representationalist," and "logocentric" characterization (after all, is not *everything* since the pre-Socratics "representationalist" and "logocentrist"?), but the price is to obscure a good deal of difference between a foundationalist procedure which, following Descartes, seeks to find a ground for reason and Kant's attempt to provide what O'Neill (1989, 3–27) has described as a "constructivist" vindication of reason.[5]

Accounts of the failure of the Enlightenment project such as Gray's typically tend to rest on criticisms of a number of separate and detachable claims. Thus, when critics take aim at the project's "rationalism," they are raising questions about the ability of reason to provide a grounding for law or morality. When they criticize its "scientism," they are questioning the adequacy of a criterion of rationality that is measured in terms of instrumental efficacy. When they point to its naive commitment to "progress," they are questioning the possibility of evaluating all societies and cultures in terms of a single measure of "development." Cobbling distinguishable claims such as these into something called the Enlightenment project misses an essential point: Criticisms of these very same claims were advanced by thinkers typically associated with the Enlightenment. Hume, for example, had doubts about the prospect of constructing a moral philosophy grounded on reason alone, Kant's entire critical philosophy can be viewed as an attempt to define reason in something other than instrumentalist terms, and both Voltaire and Diderot offered extensive criticisms of the idea of progress. What is bravely called the Enlightenment project thus turns out to be a pastiche of arguments, each of which—when viewed separately—would find critics within the Enlightenment itself.

Thus, current discussions of the Enlightenment are marked by a paradox. Those who speak most confidently of it as a single, coherent project have little confidence in the alleged project's viability. Those who speak with most competence about the historical reality of the Enlightenment have little confidence in offering a general characterization of what the Enlightenment was attempting to achieve. Thus, the Enlightenment project is coherent only for those who are in the process of rejecting it, while those who examine it more closely find the object of their concern dissolving into a host of particulars.

INVOKING CIVIL SOCIETY

In the writings of scholars, social critics, and political activists, the invocation of civil society is even more pervasive than criticisms of the Enlightenment project. During the 1970s and 1980s, critics of authoritarian regimes in both Eastern Europe and Latin America used the notion as a rallying point (Arato and Cohen 1992, 29–36, 48–69). Since then, it has played a major role in discussions of the prospects for democratization (Gellner 1996, 1–12). Over the last decade, social scientists have looked for signs of its emergence in societies where conventional wisdom assumed it did not exist (Hann and Dunn 1996; Schwedler 1995) and have sought to explain its apparent demise in the United States (Putnam 1995). Over the last few years its "renewal" has been the object both of foundation support and of proposed legislation—witness the Project for American Renewal drafted by Senator Dan Coats (R-Indiana), which consists of a series of measures designed to halt "the decline of civil society" by providing support for "families, churches, neighborhoods, voluntary associations" (Coats 1996).

Like the Enlightenment project, the meaning of "civil society" tends to be rather elusive. Shils's (1992, 3) often-quoted definition reads like a sketchy set of directions: "Civil society lies beyond the boundaries of the family and clan and beyond the locality; it lies short of the state." There is, however, a good deal to be found beyond the family but short of the state: markets, voluntary associations, churches, interest groups, labor unions, nongovernmental organizations, and Robert Putnam's (1995) steadily dwindling bowling leagues. It remains an open question whether much is gained by gathering these different forms of association together under a single label (Kumar 1993). Furthermore, rival sets of directions explain where civil society may be found: Arato and Cohen (1992, ix) differ from Shils by placing civil society "between economy and state" and arguing that it is "composed above all by the intimate sphere (especially the family), the sphere of association (especially voluntary associations), social movements, and forms of public communication." The possibilities for dispute about just what—and where—civil society is would appear to be endless.

Invocations of civil society resemble criticisms of the

[4] For one example, see Friedrich Heinrich Jacobi, "Something Lessing Said" (Schmidt 1996, 191–211).

[5] "Post-modernist" critics almost habitually equate Descartes and Kant. See, for example, Cornel West's comments on what he calls the "Cartesian-Kantian picture" (West 1979, 68).

Enlightenment project in a second respect: They come from across the ideological spectrum (Walzer 1991). Opponents of authoritarian regimes employ the term to denote something like the rights and liberties long associated with liberal democracies. Radical democrats use it to denote the ideal of an engaged, active citizenry, directly involved in public deliberation. For libertarians, it designates a market society, free from political coercion. For communitarians, it evokes the network of voluntary associations and the civic virtues they engender. If the Enlightenment project serves as a catch-all category that designates whatever particular aspect of the modern world aggravates the critic who marshals it, so "civil society" appears to be an equally elastic category, designating whatever the social critic finds central to the particular vision of society that is being articulated.

The looseness of the term may be attributed partly to the fact that, unlike "the Enlightenment," "civil society" has generally served as a theoretical concept used to designate a certain form of association rather than as a way of denoting a particular period.[6] Theorists are thus at liberty to propose new definitions, often with only the slightest familiarity with earlier formulations. The term first entered the world as a way of translating *koinonia politikē*, coined by Aristotle to describe the form of association more commonly termed a *polis* (Schmidt 1986). Until the close of the eighteenth century, "civil society" was employed, following the paradigm laid down by Aristotle, to designate that form of political association conventionally referred to as a "state" or *civitas*. This pattern of usage was taken over without alteration by natural law theorists. But by the early nineteenth century, it had been called into question (Schmidt 1995). Hegel's *Philosophy of Right* made use of a distinction between state and civil society that was unconventional enough to confuse his first reviewers (Schmidt 1982), and Tocqueville's drafts for *Democracy in America* proposed a tripartite division into "religious," "civil," and "political" societies (Schleifer 1980, 7). To speak, then, of civil society (following Hegel) as something distinct from both the family and the state, or (as in Tocqueville's initial drafts) from both religious and political forms of association is to suggest that there are patterns of association not adequately grasped by eighteenth-century political and legal theory. This altered use of "civil society" set the stage for the emergence of the various social sciences that went on to explore the domain carved out by the term.

There was also considerable ambiguity as to just what the proper focus of the newly emerging social sciences might be. Recasting Hegel's distinction between family, civil society, and the state into a dichotomy between state and society, Stein ([1850] 1964, 50) saw sciences of society—as opposed to sciences of the state—as including the "sciences of economics, of labor, of householding economy and of national economy, of the family and of law." Tocqueville, in contrast, turned from the consideration of the legal structure of the new American democracy to probe the patterns of association, the customs, manners, and mores, the "habits of the heart" that defined its mode of existence. And Marx ([1859] 1970, 20), interpreting civil society as the "sum total" of the "material conditions of life," argued that the "anatomy" of civil society was to be found in political economy. The nineteenth century's uncertainty about the boundaries of civil society continues to be played out today in disputes over whether to include the intimate sphere of the family, whether civil society is reducible ultimately to the market, and whether it is opposed to or requires the existence of the state.

The term "civil society" is unclear in one final respect. It can function either as a normative ideal used to designate the attributes that a political community ought to possess or as an analytic category that is used, within an empirical analysis, to designate a set of "ostensibly 'private' yet potentially autonomous public arenas distinct from the state" that prevent society from "degenerating into a shapeless mass" (Eisenstadt 1992, ix). Locke used the term in the first of these senses when he argued that absolute monarchies are not "civil" societies, since their rulers are not restrained by the terms of the social contract. Social scientists use the term in the latter sense when, for example, they explore the informal networks of association that provide the basis on which political structures arise. Thus, when the term "civil society" is invoked, it is not only often unclear what exactly it encompasses—markets? informal organizations? the domestic sphere?—but also sometimes less than clear whether the author is offering an empirical analysis of an existing social order or postulating a vision of what a good society would look like (Seligman 1992, 201–6).

As a result of the considerable elasticity of both the idea of civil society and the notion of an Enlightenment project, accounts of how the latter affected the former will diverge markedly, depending on the particular choices of the commentator. For example, while Berger and Neuhaus (who see mediating structures as rooted in particular, local circumstances) regard the Enlightenment project as undermining mediating structures (Berger and Neuhaus [1976] 1996, 161–2), Habermas (who understands civil society in terms of a public sphere of citizens engaged in free and open discussions) sees the development of civil society as a fulfillment of the incipient promise of the Enlightenment project (Habermas 1996, 329–87). Hence, any attempt to address the relationship between the Enlightenment project and civil society must first answer: Whose Enlightenment? Which civil society?

[6] There are notable exceptions. The German term *bürgerliche Gesellschaft* can also be employed as a way of designating a particular historical form of social life, "bourgeois society." Marx made the most of the ambiguity in *The German Ideology* ([1845] 1975, 89): "Civil society as such only develops with the bourgeoisie; the social organization evolving directly out of production and commerce, which in all ages forms the basis of the state and of the rest of the idealistic superstructure, has, however, always been designated by the same name."

KANT ON ENLIGHTENMENT AND CIVILITY

Those who have sought to revive the concept of civil society have generally taken their departure from either Hegel or Tocqueville. They have thus tended to overlook the contribution of a thinker who framed the relationship among civility, enlightenment, and society in a particularly suggestive fashion: Immanuel Kant. Late in 1784, Kant published two essays in successive issues of the *Berlinische Monatsschrift*: "Idea for a Universal History with a Cosmopolitan Purpose" and "An Answer to the Question: 'What Is Enlightenment?'" On first glance, they seem to be pursuing rather different concerns. The "Universal History" essay, Kant explained, was written in response to a note published in the *Gothaische Gelehrte Zeitung*, which mentioned a conversation in which Kant had outlined his ideas on the philosophy of history to a passing scholar. The second essay was prompted by an earlier article in the *Berlinische Monatsschrift*, which had noted that the term "enlightenment" was frequently invoked in the journal but rarely explained and had asked for a definition.[7] Yet, notwithstanding their diverging subject matter and almost casual character, when read together the two essays provide a remedy for at least some of the confusions that plague current discussions of the Enlightenment and civil society. They (1) offer a definition of the Enlightenment that rests on a novel conception of the "public use of reason," (2) employ the distinction between public and private uses of reason to distinguish civil society [*bürgerliche Gesellschaft*] from cosmopolitan society [*weltbürgerliche Gesellschaft*], and (3) use the perspective of a hypothetical cosmopolitan society as a critical vantage point from which to scrutinize civil society.

Kant's "What Is Enlightenment?" is by now so familiar that it is easy to overlook how novel it was and how theoretically fecund it remains. He defined enlightenment not in terms of what it achieves but by what it escapes: "Enlightenment," the famous opening line proclaims, is "mankind's exit from its self-incurred immaturity" (Kant [1784] 1923, 35). While Moses Mendelssohn's response, published a few months earlier but not seen by Kant, had defined enlightenment as one of the "modifications of social life" that bring a people "into harmony with the destiny of man," Kant measured the advance of enlightenment in terms of the elimination of constraints on the "public use of reason."[8]

In a number of important essays, O'Neill has stressed the significance of Kant's approach. It takes practical reasoning as fundamental and invokes neither perfectionist presuppositions about the proper ends of human action nor metaphysical presuppositions about the validity of human reasoning (O'Neill 1989, 28–50; 1990; 1996). The grounding for practical reasoning must be constructed rather than discovered: Reason, as Kant ([1781] 1929, 593) insisted in the *Critique of Pure Reason*, "has no dictatorial authority; its verdict is always simply the agreement of free citizens." O'Neill argues that the much-criticized tendency toward universalization and abstraction in Kantian ethics follows from Kant's requirement that practical reasoning must be "followable by those for whom it is to count as reasoning" (O'Neill 1996, 51–59).

> Nothing will count as a principle of reason if it demands submission to some unvindicated authority; anything that does count as a principle of reason must be one that all can follow. The principles of reason are those that can secure the possibility of intersubjectivity. Kant does not ground reason in actual consensus, or in the agreement and standards of any historical community; he grounds it in the repudiation of principles that preclude the possibility of open-ended interaction and communication (O'Neill 1990, 194).

Thus, what "communitarian" critics see as the central vice of the Enlightenment—its abstraction from particular, local norms and circumstances—is, for O'Neill, its central virtue. Insufficiently generalized explanations of practical reasoning, which require the acceptance of specific presuppositions or disputable ends, will be less likely to win intersubjective agreement than more abstract accounts of practical reasoning. "There are no general reasons for thinking," O'Neill argues, "that thick act descriptions are more comprehensible than thin ones" (O'Neill 1996, 68n).

Kant's distinction between private and public uses of reason plays an important role in his account of civil society. In "What Is Enlightenment?" he argues that when individuals are engaged in a "private" use of reason—a use constrained by the demands of the positions they hold and the associated duties and responsibilities—they function as "passive" parts of the "machine" of civil society. But individuals are never simply members of civil society. At every moment they are also (if only potentially) members of a "cosmopolitan society," and as members of this community they enjoy a right to the free and unrestricted public use of their reason. As participants in this cosmopolitan society of writers and readers, individuals retain the right to criticize the demands made upon them as members of the civil society in which they reside.

Private uses of reason are limited by presuppositions that must simply be accepted as a condition for occupying a particular post. Kant ([1784] 1923, 38) argues, for example, that a clergyman assigned the task of instructing students in the central beliefs of the faith does not have the right to instruct students in his own diverging interpretation of the church doctrine. Simply because private uses of reason are embedded in ongoing practices and institutions, they will often be more difficult for outsiders to follow than public uses. The latter, simply because they take less as given, will of necessity be framed in a more abstract and universal voice. As a result, civil society confronts its members with a host of local rules and restrictions that simply must be accepted as given. Only when these same individuals conceive of themselves as citizens of a cosmopolitan society will they be in a position to

[7] For a discussion of the background to Kant's essay, see Schmidt 1989.

[8] Compare Mendelssohn, "On the Question: What Is Enlightenment?" in Schmidt 1996, 53 with Kant, "An Answer to the Question: 'What Is Enlightenment?' in Schmidt 1996, 59–60.

examine the rationality of the practices in which they are engaged in their role as members of civil society. The opposition between civil and cosmopolitan viewpoints is thus central to Kant's understanding of the nature of enlightenment.

The opposition of civil and cosmopolitan society had already been deployed, for different purposes, in Kant's "Idea for a Universal History with a Cosmopolitan Purpose," which appeared one month before "What Is Enlightenment?" In the earlier essay, Kant argued that "the greatest problem for the human species, the solution of which nature compels it to seek, is that of attaining a civil society which can administer justice universally" (Kant [1784] 1923, 22). The solution, however, turns out to be "subordinate to the problem of a law-governed external relationship with other states" (p. 24). Justice within civil society cannot be achieved as long as states are engaged in constant preparations for conflict with one another, but—in the famous paradox on which the essay turns—it is precisely the considerable costs of war and the preparations for war that provide the external impetus for states to reform their own constitutions in the direction of republican forms of rule. The driving mechanism behind the entire process is the equally paradoxical notion of "unsocial sociability": the tension, endemic to the human race, of desiring both to live in society with others and to live as an individual (pp. 20–1). Civil society appears in this essay as a domain racked with antagonisms and tensions that provide the impetus for creating a cosmopolitan order, which in turn provides the background conditions needed for attainment of a just civil order. Once again, Kant provides an account of civil society which refuses to downplay the particularity and individuality that rules in civil society, but he also refuses to grant it the last word.

It is difficult to square much in Kant's account of civil society with recent discussions. The imagery he employs in the description of civil society in "What Is Enlightenment?" is hardly appealing: He likens it to a machine, and individuals are but passive cogs in its gearwork. The "Universal History" account of "unsocial sociability" is far removed from the cozy assurance that when "civil society is strong, it infuses a community with its warmth" (Coats 1996, 25). Both accounts also depart from recent discussions by remaining true to the conventions of natural law theory and using state and civil society as equivalent terms.[9] While those who have taken Hegel, Marx, or Tocqueville as their model have sought to explain changes in political society by examining the economic and social forces that operate within the domain they designate as "civil society," it is the opposition between civil and cosmopolitan society that is central to Kant's argument. The impetus for changes in the nature of public life comes from individuals learning to think of themselves as members of a society which transcends the individual state, not from individuals framing their actions in response to forces that originate somewhere beyond the family yet short of the state.

The weaknesses of Kant's argument should not be minimized. His example of a military officer criticizing in scholarly journals the policies that he executes on the battlefield (Kant [1784] 1923, 37–8) seems terribly naive after the outrages of the last two centuries. And while he grants (in his discussion of the clergyman whose criticisms of his faith leave him unable to carry out in good conscience the duties demanded by the "private use" of his reason) that individuals may come to a point at which their "public" misgivings necessitate their resignation from their "private" position (p. 38), it is not at all clear how this can be applied to the case of the taxpayer who disagrees with the policies of the government that her taxes support. Must she remain content with letters to newspapers and articles in journals? Does her public use of reason have no means of expression other than the written page? Could it not also take the form of a refusal to pay taxes as an act of civil disobedience in concert with others dissenting from such policies?

But neither should the abiding strengths of Kant's position be underestimated. Nowhere do they become more apparent than in the difficulties faced by theorists who have attempted to derive ethical and legal norms from current conceptions of civil society.

CIVILITY AS A VIRTUE?

For Kant, civil society denotes that form of association proper to a state or *civitas*, a society ruled by laws that pass a universalization test, which requires them to respect the attributes of "lawful freedom," "civil equality," and "civil independence" that citizens possess as hypothetical contractants who agree to subject themselves to civil order (Kant [1797] 1907, 311–6).[10] As such, Kant views civil society as a norm against which states are to be evaluated. By transforming the term into a set of institutions located somewhere between the individual and the state, those thinkers who have sought to define norms of "civility" have typically rejected the sorts of universalization tests to which Kant appealed. A brief consideration of Shils's attempt to describe the virtue of "substantive civility" suggests some of the difficulties encountered by such approaches.

> Substantive civility is the virtue of civil society. It is the readiness to moderate particular, individual or parochial interests and to give precedence to the common good. . . . Whenever two antagonistic advocates arrive at a compromise through recognition of a common interest,

[9] See Kant's equation of "civil condition (*status civilis*)" with the "state (*civitas*)" in *Rechtslehre* §43 (Kant [1797] 1907, 311) and his contrast of "juridical-civil society [*rechtliche bürgerliche Gesellschaft*]" to "ethical-civil society [*ethische bürgerliche Gesellschaft*]" in *Religion within the Limits of Reason Alone* (Kant [1793] 1960, 86–8, 90–1, 93). Seligman (1992, 43), nevertheless, argues that Kant does not view the state as "coterminus" with civil society, since "the publicness of rational debate and critique is seen (and indeed emphasized) as the province of civil society in its distinction from the State." This is difficult to square with Kant's own writings, which consistently equate state and civil society and which see public debate and discussion as evolving within a "cosmopolitan" (*weltbürgerlich*), rather than a "civil" (*bürgerlich*) society.

[10] This is Kant's reformulation of the idea of the social contract.

> they redefine themselves as members of a collectivity, the good of which has precedence over their particular objectives. The good which is accorded precedence by that decision might be no more than the continued existence of the collectivity in which they both participate. The common good is acknowledged wherever a more inclusive collectivity is acknowledged.
>
> Every action in which thinking of and attempting to reduce the prospective loss inflicted on one section of a society when another section would benefit from a particular event of policy is an act of substantive civility. It is always possible to consider the consequences of any particular action in the light of its effect on the wider circle within which a decision is made. Every action which bears in mind the well-being of a more inclusive collectivity is an action on behalf of the common good (Shils 1992, 16–7).

There are three immediate problems with Shils's formulation. First, the blanket assertion of the primacy of the good of the "more inclusive collectivity" over "particular, individual, or parochial interests" is fraught with difficulties. Is Shils seriously proposing that any assertion of individual rights against the greater good of the collectivity is an affront to the virtue of civil society? Does the good of a "more inclusive collectivity" always trump the rights of individuals? Second, what sort of metric are agents supposed to apply when they reflect on whether the consequences of their actions will advance the good of the collectivity? What degree of uncertainty about the complex chain of causes that link actions to consequences is tolerable? Finally, Shils's statement that it is "always possible to consider the consequences of any particular action in the light of its effect on the wider circle" raises an obvious question: What falls within "the wider circle"? Should the effect on future generations be considered part of our "common good"? And what of present and future members of other societies?

When contrasted with Shils's attempt to articulate a set of virtues specific to civil society, many of the conventional criticisms of Kant's views on enlightenment and civil society lose their force. Complaints about the Enlightenment's "excessive" concern with individual rights pale in comparison to a concept of civic virtue which, in its embrace of "communitarian" goods, would open the way to state intervention in domains that it long ago vacated, so long as we have assurances that the "public good" is being served. It may be the case, for example, that the enforcement of religious conformity would advance the "common good" of societies with a fair measure of religious homogeneity. But this hardly is a compelling reason for rejecting well-established principles of religious liberty. Likewise, it is by no means clear that an attempt to assess the consequences of actions on a less than clearly defined community gives us any greater guidance than Kant's attempt to construct an account which, by focusing on the intelligibility of practical reasoning, frees us from the necessity of calculating elusive chains of causal connections. Finally, whereas Shils can provide no reason for limiting the scope of ethical concern to a particular civil society, Kant explicitly recognizes that any account of the moral improvement of individual states must of necessity consider the relationship between actions within state borders and those involving the international community.

There are thus good reasons for being suspicious of the well-worn arguments against Enlightenment universalism and the new-found enthusiasm for the virtue of civil society. The Enlightenment project remains too ill-defined a notion to serve as an object either of allegiance or condemnation. What is needed instead is a careful weighing of the variety of different commitments and intentions—not all of them reconcilable—carelessly lumped together under that label. But as has been suggested above, to undertake a critical examination of these different claims is to take up a task which, with less violence to history than other accounts, might well be characterized as the Enlightenment project.

With regard to the current enthusiasm for the virtues of civility, vigilance of a different sort is required. In so far as "civil society" is a category of analysis in the social sciences, the question is whether this new use of an historical term advances our knowledge of the social factors that promote the emergence of democratic institutions. The answer will be decided by the quality of empirical research that the concept fosters.[11] With regard to the normative use of the concept, even greater skepticism may be justified. A rich tradition within political philosophy has sought to define civil society—understood as the most general term available for designating that form of association in which public life transpires—in terms of the norms of liberty, equality, and justice. Until we see a more compelling reason for giving preference to the alleged virtues of civility over the stricter demands of justice, we may be forced to second the advice that the Evangelist gives to the Pilgrim in John Bunyan's great allegory: "Mr. Legality is a cheat; and as for his son Civility, notwithstanding his simpering looks, he is but a hypocrite and cannot help thee." And just as the Evangelist advises that "there is nothing in all this noise, that thou hast heard of sottish men, but a design to beguile thee of thy salvation," so, too, we may do well to ask whether all this noise about civility is anything more than a design to turn us away from that concern with justice which lies at the heart of any "Enlightenment project" worth defending.

[11] For dissenting views on the analytic usefulness of the category, see Kumar 1993 and Seligman 1992.

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BOOK REVIEWS

Political Theory

Opera and Politics: From Monteverdi to Henze. By John Bokina. New Haven and London: Yale University Press, 1997. 240p. $25.00.

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In the words of the author, *Opera and Politics* is "the work of a political scientist, a political theorist who loves opera. Here I use my training in the history of political theory in order to highlight neglected aspects and meanings of works from the several periods of the history of opera: baroque, early classical, late classical, romantic, early modernist, late modernist, and postmodernist" (p. xii).

The chapter headings entice the reader with provocative wording. The first, "The Prince as Death, Beast, and Tyrant: Monteverdi's *Orfeo, Ulisse, and Poppea*," discusses three operas that "resonate with the characteristic political themes of the age: the contest between fortune and virtue, the simultaneous need to express and restrain the passions, the rival claims of political activism and contemplation, the intoxication with power" (p. 13). In the first opera (1607), Orpheus is seen as the godly prince who must learn a Platonic lesson. After his traumatic loss of Euridice, he learns under the counsel of Apollo the value of contemplation and the ability to rule his passions. Accordingly, the opera ends harmoniously, not with violent death at the hands of the Bacchantes as the myth has it. Bokina interprets this radical revision as the obligatory homage Monteverdi paid to his prince and patron, Francesco Gonzaga.

Then Monteverdi moved to Venice, and the other two operas reflect a different political milieu. In Bokina's view, *Il Ritorno d'Ulisse in Patria* (1641) evokes the Realpolitik of a powerful aristocratic republic in decline. Ulysses, that Homeric prince who uses cunning and bold courage to reclaim his realm, is interpreted as "beast" through a quote from Machiavelli: "The prince must be a fox, therefore, to recognize the traps, and a lion to frighten the wolves" (p. 28). The author argues persuasively that this opera glorifies the illustrious past of a failing republic. In contrast, *L'Incoronatione di Poppea* (1642) implicates the state of civic virtue in Monteverdi's Venice. Hedonism and the passions rule the Roman tyrant Nero, his seductive Poppea, and Venetian society as well.

In "The Dialectic of Operatic Civilization: Mozart's *Don Giovanni*" (1787), Bokina opposes social conformity to the drive for individual freedom, a dialectic which he sees as paramount in Mozart's time. For him, Don Giovanni's insistence upon his libertine life in the face of social censure, divine judgment, and hellfire makes him "a tragic hero" (p. 42). Out of this scenario Bokina formulates his own dialectic, which opposes this view to the text and music of the opera, for neither Lorenzo da Ponte nor Mozart conceived of their libertine as a tragic hero. So the author resorts to the tactics of deconstruction, in which marginal clues in the text yield a contradictory interpretation. Unfortunately, the characters imbued by Mozart with the richest music are now reduced to rigid stereotypes in order to justify a debatable proposition, which elevates Don Juan to *Übermensch*. Nonetheless, the eloquence of Bokina's argument compels the opera connoisseur to reexamine cherished assumptions and see issues of the time as cogent background.

In "Opera and Revolutionary Virtue: Beethoven's *Fidelio*" (1815), the author interprets this opera as "an allegory of the French Revolution, the supreme example of bourgeois republican virtue" (p. 75). His imaginative conception of the heroine, Leonore, enhances Beethoven's opera immeasurably and will give joy to all feminists. At the same time he tends to reduce the supporting characters (again) to melodramatic stereotypes. Beethoven took them seriously, as his music amply demonstrates. In one of the most celebrated ensembles in operatic literature, four of these flawed human beings rise above their petty concerns to express deeper aspirations, as ordinary people sometimes do. They, too, have their place in Beethoven's republic.

I think Bokina rightly conceives "The Utopian Vision of Romantic Anticapitalism: Wagner's *Parsifal*" (1883), despite the traces of racist ideology certain critics have detected in this opera. But Wagner's art is great enough to survive his ideology. Regarding "Wagner contra Nietzsche" (pp. 106–10), however, I do not think Nietzsche "misinterpreted" Wagner's intentions in *Parsifal*. The ideology of renunciation (Christian or otherwise) and the central role of tortured guilt in this opera revolted Nietzsche because they represented to him a retreat from life and Lust (joy, desire, delight, pleasure, and so on). Then, Wagner's conception here of Woman as Biblical temptress—fallen, yet penitent and submissive to male authority—cannot please everyone. Nonetheless, one can be uplifted by the aura of Utopian transcendence in Wagner's music, especially in the "Good Friday Spell" and the final scene. After Wagnerian Utopianism, "The Politics of Psychological Interiorization: Strauss's *Elektra* and Schoenberg's *Erwartung*" (both 1909) plunges us into a world of psychic disorder, evoked by a distorted, distended musical idiom, which Bokina interprets as a retreat from political disillusionment into the inner world of first causes.

The next chapter addresses "The Aesthetic Politics of German Artist-Opera" in Hans Pfitzner's *Palestrina* (1917), Paul Hindemith's *Mathis der Maler* (1938), and Arnold Schoenberg's *Moses and Aron* (1932). All three composers wrote their own librettos, and they identify with the protagonists they create. In Pfitzner's "musical legend," the composer Palestrina is threatened with imprisonment and torture unless he produces a stirring liturgical work, in the great tradition of Roman polyphonic church music, for political purposes at the Council of Trent. Depressed for various reasons and unwilling to compose on demand, he refuses to comply. In his darkest moment of despair, then, comes the light of musical inspiration. Moved by this ineffable source of creativity, not by political coercion, he creates a definitive masterwork that vindicates and gives new life to the tradition which nourished his art. Pfitzner saw himself in analogous terms. He sought to vindicate the great tradition of German romanticism in music and further its evolution, not "freeze" it, as Bokina claims (p. 165). External pressures, such as the fashionable modernism of the Weimar Republic or the demands of Nazi Kulturpolitik never swayed him. Daunted at times and depressed, he continued to go against the tide. He was hardly the victim of "passive acceptance" (p. 165). Why Bokina thinks *Palestrina* and its creator exude resignation is hard to understand. Palestrina is shown in the final scene of the opera exhausted by his monumental achievement but serene and fulfilled, as text and music tell us. Pfitzner knew resignation in the face of drastic upheaval, but he always rebounded with bursts of creative production and fierce polemics against radical modernism. *Palestrina* displays much in common with the ideas behind Hindemith's *Mathis der Maler*, which relates the painter Matthias Grünewald (c. 1480–c. 1530) to the social turmoil of his time.

I agree with Bokina's assessment of Schoenberg's *Moses und Aron* as "impotence in modernist opera" (p. 154). The author sees Aron as the embodiment of artistic impotence,

but I think Schoenberg's radical modernism is the fundamental problem. Schoenberg, like Moses, considered himself a law-giver. His "commandments" are revealed in the twelve-tone method (his solution for music in the future), a totalitarian system of pitch relationships which scrambles traditional musical syntax and thereby severely limits expressive range and dramatic differentiation. In *Moses und Aron*, the method fails to sustain the action, and the final act consists of spoken text only. Instead of the promised land, it leads to a dead end.

Bokina's admirable synopsis and compelling interpretation of Heinz Werner Henze's *The Bassarids* (1986), a postmodernist reworking of the Apollo versus Dionysus theme in a political context, makes me want to know this opera better. Here, the "aesthetic-politics" of Henze draws eclectically from traditional styles and procedures, reaffirming the past in ways that might have reassured Pfitzner.

Thank you, John Bokina, for this rich feast of provocative ideas on that extravagant art form called opera. Those who malign it with the epithet "What is too silly to be said may be sung" (attributed to Dr. Johnson) henceforth had better eat their words.

Cornerstones of Peace: Jewish Identity Politics and Democratic Theory. By Marla Brettschneider. New Brunswick, NJ: Rutgers University Press, 1996. 201p. $48.00 cloth, $16.95 paper.

The Narrow Bridge: Jewish Views on Multiculturalism. Edited by Marla Brettschneider, with an introduction by Cornel West. New Brunswick, NJ: Rutgers University Press, 1996. 291p. $48.00 cloth, $18.95 paper.

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Each of these volumes addresses the often overlooked place of American Jews as a complex of interests in American politics and as a part of the dialogue on multiculturalism.

Cornerstones of Peace is a critique of contemporary theory on interest groups, using American Jewish groups and their relation to Israel as case studies. Brettschneider's point of departure is her objection to the expectation that all Jews comprise one group with a unified interest. The misconception is reinforced by America's liberal worldview, which regards interest groups as individuals: In a Hobbesian model, each is isolated from and competing with all other interests. Brettschneider observes: "Mainstream politics in the United States... has long operated within a paradigm in which atomistic, competitive, and self-interested behavior is most likely rewarded" (p. 6).

The tendency to treat "Jewish interests" as monolithic and at odds with other American interests has been encouraged by Israel and reinforced by Jewish lobbyists in America. Political positions are dichotomized as either for or against Jewish or Israeli interests, with no regard for diversity or pluralism within the Jewish community in America and elsewhere. Brettschneider's volume is intended as a corrective, both politically and theoretically.

Politically, the book describes the emergence since the 1970s of Jewish interest groups in the United States seeking alternatives to blind support of Israeli policies, resisting the accusation that all protest reflects "Jewish self-hatred" and fuels support for Israel's enemies. These new Jewish activists are no less committed to their Jewish identities and to the need for Israel to exist than are the more powerful lobbyists who contributed to the exaggerated polarization into friends and enemies of Israel. "When rank-and-file Jews expressed-... dissenting opinions, they were virtually disinvited from the Jewish community. When Jewish leaders tried to do so, they were delegitimized. When Jews with such opinions had tried to come together organizationally in years past, their group was destroyed" (p. 5).

Brettschneider thus offers a political history of the recent struggles for alternative voices in the Jewish community to be heard, while confirming the unity within that diversity: There *is* a Jewish community. "It is possible to see that the Jewish community is a community, a unity, with all of its difference.... The apparently monolithic voice of Jewish leadership has always embodied dissent" (p. 36). Yet, that dissent does not undermine "an understanding of the cluster of groups involved in the creation and re-creation of pro-Israel identity in relation to one another" (p. 57). Brettschneider refers to this tension as dialectical: "multiple, fluid, and changing" (p. 37).

This corrective and progressively driven alternative vision to Jewish politics in America is important, and it is likely to become moreso as current right-wing extremist politics in Israel are played out. American Jews, many of whom have profound religious and political disagreements with Jewish religious fundamentalists in Israel, cannot be dismissed as disloyal Jews or as having anything less than a profound stake in the future of Israel. Their political positions cannot be dictated by Israel, nor will they constitute a unified front in the United States.

Brettschneider offers a promising vision for America in a multicultural age, refusing to accept Madison's definition of factions as inherently parochial and antisocial: "We can understand better the concrete obstacles faced by the alternative Jewish groups when we realize that the problem of a politics based on the notion of self-interest is that it tends to squelch diversity behind a mask of the 'common good' supposed to transcend our 'particulars'—or differences—resulting in a closed political space" (p. 37). She makes a plea to reject the "Hobbesian" basis for a politics of fear: "Perhaps the most important time for open honest dialogue within a community is during a time of crisis" (p. 47). Given the very real fear of annihilation that has driven Jewish politics in the late twentieth century, this seems like a tall order.

The weakest aspect of the book is its theory. While the discussion of Jewish interest-group politics is informative and important, the continual return to discussion of Hobbes, Mill, Madison, Bentley, and, somewhat discordantly, Seyla Benhabib and Martha Ackelsburg is belabored and distracting, rather than useful to our understanding of Jewish politics in America. The application of theory remains superficial, not well integrated into the political discussion. It is not that Brettschneider is either wrong or inaccurate in her discussion of democratic theory; it is simply unnecessary to use Thomas Hobbes in order to understand Jewish politics in America. Does Brettschneider mean to say that only the American political structure is at fault for seeing the world as warring Hobbesian individuals, thus distorting historically pluralistic Jewish politics? Or does she implicate Israel as well? For Brettschneider is as concerned by Israel's ability to control American Jewish politics as she is by American Jews' response to their own liberal individualistic political culture. And, surely, she cannot mean that Thomas Hobbes and James Madison have had a profound influence on Israeli politics?

Finally, is Brettschneider's apparent need to discuss Hobbes, Mill, and Madison before talking about anything else in American politics itself assimilationist? Does this volume exemplify that, before granting the doctorate to a student writing about Jewish politics in America, the American

political science establishment demands for legitimacy a demonstrated familiarity with a handful of European male thinkers? The result is that an original thesis such as Brettschneider's strains to include political theorists whose fit with her discussion is problematic. It diminishes the work, and it should make us think hard about what the profession demands of doctoral dissertations.

The Narrow Bridge continues the discussion of Jewish pluralism and Jewish multiculturalism, but without the distraction of Anglo-American political theory. It is a rich and intense volume, addressing issues of: diversity within the Jewish community, especially class and sexual orientation; possibilities for dialogue between Jews and other ethnic and racial minorities; Judaism as a part of multicultural education at both schools and universities in America; and the challenges of including Jewish representation as an aspect of political and social action. Some of the essays are stunning. Bob Goldfarb's short piece, "Lesbians and Gays in the Jewish Community," is one of the best I have read on this controversial subject. He powerfully addresses the challenges to including these groups in all aspects of Jewish *religious* life, by confronting and interpreting sections of the Torah and Talmud that have been used throughout the millenia of Jewish history to condemn male homosexuality. (The argument against lesbianism is less problematic, since traditional Jewish sources rarely even acknowledge female sexuality in their rush to exclude all women from the most sacred and respected aspects of Jewish life.)

Felice Yeskel's discussion of the taboo of social class deals with anti-Semitic misconceptions of Jewish wealth. Martha Ackelsberg addresses Jewish feminism in the context of feminist literature on coalition-building.

Two essays on Jewish relations with African Americans, dealing with early involvement in the Communist Party (Gerald Horne) and the civil rights movement (Melanie Kay/Kantrowitz), provide insightful historical background on this troubled relationship. Naomi Nim's account of her experience as a cotrainer, along with an African-American woman, in an antiracism, antioppression session for a primarily white Protestant woman's group touches upon Jewish conflicts about acknowledging anti-Semitism and Jewish deference to the presumption of the "greater oppression" suffered by African Americans. Other contributions with more practical applications make the volume a valuable sourcebook.

Apart from my sense that the volume may attempt to cover too much territory, I have only one complaint: the unquestioned correlation of oppression with material poverty. All the contributors accept poverty as the "worst" form of suffering. Without diminishing the seriousness or severity of material deprivation in America and elsewhere, I would have appreciated an acknowledgement of the feminist position, that oppression is not always visible. To privilege material suffering over the less tangible psychological oppression characteristic of racism, sexism, and homophobia is to accept conventional wisdom and to miss a dimension of prejudice that has contributed to the exclusion of Jews from multicultural dialogue.

God and Man in the Law: The Foundations of Anglo-American Constitutionalism. By Robert Lowry Clinton. Lawrence: University Press of Kansas, 1997. 293p. $35.00.

Robert P. Kraynak, *Colgate University*

The opposition to judicial activism is a growing movement that no longer can be ignored by politicians and legal scholars. President Clinton has felt its power in the determination of a Republican Senate to leave vacancies on the federal bench rather than fill them with activist judges. A respected journal, *First Things*, published a polemical attack on "the judicial usurpation of politics" (November 1996) that immediately provoked a debate about how far the judiciary has gone in undermining American democracy. Over the last thirty years, many critical studies have been written by influential scholars, such as Alexander Bickel, Robert Bork, Raoul Berger, Christopher Wolfe, Stanley Brubaker, and Jeremy Rabkin. To this list, one may add Robert Clinton, a self-described conservative who has written two impressive books on the problem of judicial activism.

In his first book, *Marbury vs. Madison and Judicial Review* (1989), Clinton exposed the myth of judicial supremacy that has grown up around *Marbury* and proposed "coordinate review" by all branches as a way of breaking the monopoly of the courts on judicial review. In his second book, *God and Man in the Law*, Clinton argues that judicial activism cannot be overcome without a proper understanding of the higher law foundations of the Constitution.

To show the continuity of his thinking, Clinton begins *God and Man* with a summary of his earlier work. He emphasizes the fact that the Supreme Court did not explicitly claim supremacy over the other branches in interpreting the Constitution until 1958, when the Court said in *Cooper* versus *Aaron* that its judgments were final (pp. 13, 27). Until that time, Clinton argues, the other branches had interpretive roles; finality was left unspecified, permitting the checks and balances of the separated powers to work as the Framers intended, without one branch dominating as a matter of principle. Clinton's advice is to return to the pre-*Cooper* practice, which would give finality to the judiciary only in cases pertaining to judicial functions. In addition, he recommends that all branches follow the rules of interpretation derived from English common law: Stick to the literal meaning of the text as intended by the lawgiver, unless it is absurd in the present context or requires exploration of the remedial intention of the law (pp. 111–6).

But Clinton is quick to admit that rules of interpretation, especially ones that sound like "original intent" or conventionalism, are insufficient to overcome judicial activism. In fact, it was precisely the accusation of being a conventionalist (made by Soterios Barber in reviewing his earlier work) that inspired Clinton to write his new book. Here, Clinton states explicitly that he opposes judicial activism not merely because judges are ignoring the text and usurping power but because they are making law according to their subjective preferences, unconstrained by objective standards of truth or goodness.

Following the lead of Eric Voegelin, Clinton traces the subjectivism of judges to the spiritual crisis caused by the death of God and the loss of belief in cosmic moral order. He uses Voegelin's concept of "gnosticism," or deformed spiritual knowledge, to describe the crisis, arguing that judicial activism is nothing less than "the gnostic replacement of God with man as the ruler of the legal cosmos" (p. 218). What Clinton means is that judges who lack belief in God and transcendant order inevitably fill the void with their own willful assertions or with degraded, materialistic views of man.

Clinton's remedy is a return to the foundations of the Western legal tradition, which he understands to include four propositions: the belief in God as the creator of the cosmos and the providential ruler of human affairs; the belief in an immaterial mind or soul in man that can grasp objective reality; the belief in natural law as a norm for human or constitutional law; and the belief that constitutional law emerges from natural law, not through imposition of abstract

principles but through the consensus expressed in custom—known to us as the English common law (pp. 221–2). For Clinton, this set of propositions was best stated and defended by Thomas Aquinas, which makes his proposal a brand of Thomistic jurisprudence.

Obviously, Clinton's attempt to overcome judicial activism in this fashion is extraordinarily bold and ambitious. While he argues persuasively that recovering the traditional Western belief in transcendent moral order is necessary, I am not sure that he has put the pieces back together properly and formed a coherent and workable theory of law that descends from God, to natural law, to common law, to American jurisprudence. I see at least three problems with his argument.

The first concerns God. Clinton shows remarkable intellectual courage in devoting two whole chapters (15 and 16) to proofs for the existence of God, derived from the distinction made by Aquinas and Adler between necessary and contingent being. I thought these chapters showed flashes of brilliance, and they may even lead agnostic or atheistic judges to reconsider their doubts about God. But Clinton does not really establish a link between God as the necessary being who creates the world and God as the providential ruler of human affairs. Much more is needed to establish that link, by analyzing either the Bible's political teaching (as Augustine did), or the providential features of natural law (as Aquinas did), or the providential design of historical evolution (as Burke did).

The connection between English common law and American constitutionalism is also not sharply drawn. Clinton devotes only a few pages to discussing the evidence for his controversial claim that the Constitution is largely a product of the English common law. He argues that the first four articles contain numerous provisions and phrases that are "directly traceable to English sources" (pp. 96–9). But the English common law system as a whole establishes a constitutional monarchy, while the Constitution establishes a republic. Though both may aim at "ordered liberty," Clinton's analysis misses the distinctive American blend of English common law, reformed Protestantism, Lockean natural rights, and Madisonian republicanism.

A third problem is the actual link with American jurisprudence. It is not clear how one is supposed to apply Clinton's grand theory to concrete cases of American law. Nor is it clear whether it could be applied without spawning a new kind of judicial activism. I think the missing ingredient in Clinton's work is an understanding of prudence, a type of practical reason that combines legal conventionalism and higher law. Prudence enables one to follow precedent in ordinary decisions because the procedure itself is an imperfect approximation of a higher law of virtue; but prudence also counsels one to refrain from invoking the higher law justification most of the time in order not to tempt the lust for power that constitutionalism tries to control. I am sure that such a prudential combination of positive law and higher law is Clinton's primary goal; but we must await volume three of his collected works to see it fully elaborated.

Habermas and the Unfinished Project of Modernity: Critical Essays on *The Philosophical Discourse of Modernity*. Edited by Maurizio Passerin d'Entrèves and Seyla Benhabib. Cambridge, MA: MIT Press, 1997. 305p. $35.00 cloth, $17.00 paper.

Marie Fleming, *University of Western Ontario*

This volume of essays takes its title from Jürgen Habermas's well-known address, "Modernity: An Unfinished Project." Habermas gave the talk in September 1980 upon accepting the Theodor W. Adorno Prize from the City of Frankfurt, and he brilliantly used the occasion to defend modernity as a project for the future. He was concerned about three types of conservatives: traditionalists who value the cultural integrity of a period prior to modernity; neoconservatives who promote technological progress and capitalist growth; and such contemporary theorists as Michel Foucault and Jacques Derrida, whom he regarded as wishing to break with modernity altogether. According to Habermas, antimodern conservatives like Foucault and Derrida evoke the aesthetic principle of will to power or Being itself to show opposition to instrumental rationality, but they only repeat the mistakes of aesthetic modernity when they undervalue modernity's cultural potential for rationality. As interest grew in Jean-François Lyotard's claims about a "postmodern condition," Habermas continued to hold that poststructuralist theories were theoretically inadequate and politically conservative, and he argued that shifting to a communicative understanding of rationality would allow us to view modernity in a positive light. He pursued these themes in several lectures, and in 1985 they formed the core of a book, *The Philosophical Discourse of Modernity*.

Maurizio Passerin d'Entrèves and Seyla Benhabib, editors of the volume under review, provide the first complete English translation of Habermas's 1980 talk and bring together a series of essays on his 1985 book. Those by Jay M. Bernstein, Fred Dallmayr, David Couzens Hoy, Christopher Norris, and Joel Whitebook appeared some years ago in *Praxis International*, while those by James Bohman, Diana Coole, David Ingram, and James Schmidt are new.

Specialists will be pleased to have the collected views of this impressive group of established scholars, but even they may wonder about the precise connections among these essays. The introduction by d'Entrèves consists mainly of lengthy summaries of the arguments of the individual authors and does not give the needed analytical overview. To be fair, he does suggest that the issues separating Habermas and the postmodernists can be understood in terms of two different understandings of responsibility. Thus, whereas "Habermas privileges the responsibility to act in the world in a normatively justified way, the postmodernists celebrate the responsibility to otherness, namely, the openness to difference, dissonance and ambiguity" (p. 2). This discussion is much too brief, however, and not altogether satisfactory. It is also striking, given the title and implied aims of the volume, that d'Entrèves does not say why we should be so concerned about the "unfinished" project of modernity. Why should we want to think about modernity as unfinished rather than finished? Neglect of this question in the introduction is unfortunate, because what is missing in the individual essays is precisely what the volume as a whole promises—serious and engaged insight into what should be said, in the late 1990s, about the "unfinished" project of modernity.

On a straightforward reading of the majority of the essays, one might be tempted to conclude that Habermas's question is simply wrong-headed. As Schmidt suggests, there is indeed a troubling neatness and directionality in his argument. Habermas interprets post-Kantian philosophical discourse, from Hegel and Marx, to Nietzsche and Heidegger, to Foucault and Derrida, as grappling unsuccessfully with the principle of subjectivity; he sees post-Nietzschean and post-Heideggerian thought as reproducing the dilemmas of subject-centered reason; and he gives his own model of communicative rationality as the obvious solution. As well, several authors rightly object to his tendency to minimize the ambiguities and tensions in the different thinkers. According to Dallmayr, Habermas tends to project onto Hegel, Nietzsche,

and Heidegger the very contradictions he claims to be finding in their thought. Norris argues that he misreads Derrida and fails to see that deconstruction actually belongs to the philosophical discourse of modernity. Similarly, Schmidt argues that Habermas repeatedly misunderstands Foucault, and Whitebook maintains that his attack on Cornelius Castoriadis, who cannot be consigned to the postmodernist camp, reflects divergent theoretical agendas motivated by different understandings of psychoanalysis.

These authors indicate where Habermas's reconstruction of modern philosophical discourse is limited and why he is not always reliable on Hegel, Nietzsche, Heidegger, Derrida, Foucault, and Castoriadis. They also show why his arguments do not lead straight to his communicative model, as he claims. Nonetheless, the idea of an "unfinished" modernity, which is not identical with Habermas's reconstruction of philosophical discourse, endures. It also persists despite Jay Bernstein's excellent critique of his procedural view of communicative rationality. For Bernstein, discourses that resist separation from the body, *différence*, and the nonidentical point to a local and comprehensive reason that is continually at risk of being silenced by the operation of both instrumental rationality and procedural communicative rationality. This piece and the others are intrinsically interesting, but for the most part they reflect the mood and theoretical commitments of the late 1980s, when the modernity/postmodernity dispute seemed more urgent.

To be sure, there are also indications of a desire to move on. Hoy suggests that we "split the difference" between critical theory and deconstruction and work through suspect oppositions like history and theory, modernity and postmodernity. The more recent essays also explore some new territory. Bohman maintains that critical theory can respond more positively and effectively to poststructuralist theories of language by resisting Habermas's restrictive understanding of world-disclosure. Coole wants critical theory to be attentive to otherness and argues that the formal procedures of discourse ethics must be supplemented by a politics of alterity. Ingram identifies areas of considerable agreement in Habermas and Lyotard and maintains that each philosopher understands justice in terms of the dialectic of enlightenment and that each, in different ways, seeks a solution in Kant's notion of aesthetic judgment.

I came to this volume expecting to find focused discussion on how to understand the "unfinished" project of modernity. In retrospect, it was perhaps because of what was missing that I began to think all the harder about modernity. I conclude that Habermas's question, first raised in 1980, not only survives the philosophical controversies of the past two decades but also is more important than ever before.

The Power of Identity: Politics in a New Key. By Kenneth Hoover. Chatham, NJ: Chatham House Publishers, 1997. 160p. $19.95.

Mary Caputi, *California State University, Long Beach*

The field of psychology has much to offer political theory, for it makes possible a venue of analysis which, while pertinent to political life, often remains opaque to even the most rigorous empiricist. All of us prefer to think of the political sphere as the by-product of calm Rawlsian reflections and considered judgments; we like to imagine that the public arena showcases our most noble and praiseworthy characteristics, just as Aristotle said. Yet, more often than not these are wishful notions. Inasmuch as psychology insists that visceral emotions and irrational impulses inform political life, sullying an otherwise pristine reading of things, it offers an invaluable contribution to the discipline. Psychological insight clarifies the degree to which unresolved internal conflict comes alive in the public arena, giving expression in frequently sanctioned ways to the hatred, rage, desire, and ambivalence that, if unbridled, can prove ruinous to any collective.

Should this emphasis on our less palatable impulses be minimized, psychology's contribution to political analysis takes on an entirely different cast. When the former abandons the deeply conflicted dimension of psychic life in favor of a more common-sense approach to things, when it overlooks the raucous emotions and unsettled feelings that characterize internal reality, something is lost in the scholarly enterprise. The latter flattens out into a problem-solving mission, a trouble-shooting exercise whose intellectual pursuit has been diminished by the very precepts that inform it. Indeed, combining the psychic and social spheres should not profess to smooth out the rough edges of political life, for psychological insight does not lend an optimistic reading to every situation.

Kenneth Hoover's *The Power of Identity* may be faulted for such a display of optimism. The efforts to bring Erik Erikson's developmental psychology to bear on identity politics surely have the feel of affirmative resolution and ironed-out contradictions; at the extreme, Hoover's recommendation of this developmental approach lacks critical distance, and he stands too ready to offer solutions to complex social issues that have long plagued the public sphere. Indeed, the author's argument glistens with conviction where Erikson's usefulness is concerned; it assumes too readily and in too upbeat a fashion that the developmental venue translates easily into a viable political agenda. Hoover's enthusiasm is commendable, of course, but the reader cannot help but feel that a more captious, incredulous, steely-eyed reading of Erikson is warranted given all that he is credited with delivering.

Taken alone, however, such criticism is excessively harsh. There are meaningful ways in which developmental psychology enriches our approach to identity politics, and Hoover succeeds admirably in presenting a comprehensive overview of what these are. A skilled writer and erudite scholar, Hoover expounds upon two concepts that keynote Erikson's work. One is the conviction that while the content of human identities may vary—while race, gender, sexual preference, and other categorizations may separate us—the process of identity formation itself unfolds along similar lines for us all. This unfolding creates parallel dimensions of human experience, a common basis to which we can all relate. To use Erikson's technical terms, identity formation necessarily includes the components of "integrity," "competence," and "mutuality"; no contextual caveat or mitigating specificity can vitiate the uniting force of these universal elements. Against the drift of much scholarship in identity politics, then, Erikson's theory casts aspersions upon the claims of incommensurability, thus making trouble for the incongruity so paramount to postmodern pastiche and for the celebration of difference so integral to multiculturalism.

The universalist component of Erikson's theory thus allows for a tolerance that our sustained and deliberate attention to difference might preclude. Yet, before a tolerant disposition can emerge, Erikson argues, the experience of cohesion with one's cohort must occur. Only the early, initial experience of integration into a familiar community can later breed the ability to recognize parallel dimensions of human experience: The ultimate goal of peaceful coexistence thus demands a more symbiotic precursor. "Unless individuals acquire strength from some source of cultural support, they will have

a very hard time rising above the parochialities of a particular background to achieve a measure of tolerance" (p. 37). This is the second component of Erikson's theory upon which Hoover draws. The author then deftly expounds upon the political implications of this assertion, for the prescribed "cultural support" surely raises a host of political questions, thereby pointing directly to the connection between developmental psychology and the social sphere.

It is the task of democratic systems, Hoover argues, to allow integrated communities to flourish, to foster cohesive enclaves which in turn breed tolerant citizens. Only within such contexts can the complexities of identity politics be accommodated, thereby discouraging the bigotry and violence which alternative political regimes may unwittingly encourage. Developmental psychology thus resonates deeply with the democratic process; in particular, it offers a welcome alternative to the sometimes vaunted problematics that swirl around identity politics by proposing an end to incommensurability's impasse.

Yet, as stated, the precise manner in which the theory may be applied remains insufficiently explored in the book, leaving the vicissitudes of its practical contribution somewhat obscure. While *The Power of Identity* unequivocally endorses developmental psychology for the study of identity politics, it leaves undeveloped some of the central questions that would surely animate any effort to bring the former into the social field. Hoover is a sophisticated thinker who rightly warns against allowing identity politics to devolve into a naive humanism. Yet, his own project would benefit from a more hard-edged incredulity, the kind that breeds a sustained critical inquiry marked by query and hesitation.

The Self at Liberty: Political Argument and the Arts of Government. By Duncan Ivison. Ithaca, NY: Cornell University Press, 1997. 213p. $39.95.

Peter C. Myers, *University of Wisconsin, Eau Claire*

Duncan Ivison sets out to accomplish two general tasks in *The Self at Liberty*. First, he means to challenge Sir Isaiah Berlin's famous dichotomous conceptualization of liberty along with its most influential contemporary variants. More affirmatively, by challenging rigid oppositions of negative to positive liberty, of liberal to republican, and juridical or deontological to perfectionist forms of political theory, Ivison seeks to enhance our historical understanding of the character- or identity-forming arts of government that liberal as well as republican regimes practice. Second, he means to employ this enhanced understanding to prepare us to think critically and imaginatively about the sorts of governmental arts and conceptions of self and citizen that we might endorse. This is an interesting and ambitious undertaking in both its parts, although one may judge the first, to which the author devotes the far greater part of his energy, more successful than the second.

Ivison's presentation of his argument can be conveniently divided into three sections. The first, comprising chapters 1 and 2, describes and defends the author's interpretive approach, which consists generally of an amalgam of the historical (linguistic-contextual) and critical approaches of Quentin Skinner and Michel Foucault. In the second section, comprising chapters 3–6, Ivison employs his historical-critical approach to reexamine major elements of our republican and liberal heritage. The major figures in these chapters are Machiavelli, Locke, and Rawls, who appear as chief representative of the classical republican tradition and the "natural-law" and "political" variants of modern liberalism, respectively. The general aim is to show that Machiavellian republicanism bears a closer similarity to each of these major strands of modern liberalism than scholars of either have consistently recognized. As Machiavelli displays a modern concern for domestic and personal security along with his classical devotion to active civic liberty, so Locke "fits comfortably into the category of a theorist of civic or political virtue," despite his forensic conception of the person and his hedonist psychology (p. 133). Likewise in Rawls, Ivison focuses on how the regime of political liberalism—having evolved from its neutralist incarnation—depends upon and seeks to foster a specific set of virtues and moral powers in the persons whom it would govern. In the third and briefest section, which consists of the concluding chapter, Ivison moves from historical investigation to the question of political legitimacy and seeks especially to imagine different objects and arts of government from those elaborated in earlier chapters (p. 162). Radicalizing Rawls's moral pluralism and appropriating the Machiavellian notion that a contestation among divergent "humors" helps sustain republican vitality, Ivison imagines a liberal republic that begins by recognizing the "deep diversity" of its constituents and therefore makes its binding principles the subject of continual negotiation and renegotiation (p. 168).

Various sorts of readers should find the book a useful addition to the growing body of scholarship affirming a republican-liberal synthesis. With respect to its historical-interpretive arguments, the book breaks a bit of new ground, but it mainly synthesizes and refocuses an important and ongoing controversy. Ivison has read widely and displays an impressive grasp of the extensive literatures in the various fields that he surveys. Readers sympathetic to the Skinnerian approach in particular should find in his book a useful synthesis of a mass of historical literature, although they and others may not always accept his interpretive findings. Readers sympathetic to perfectionist liberalism as well as those with postmodern inclinations should, for their different reasons, find the book a useful exposition of the liberal concern for character formation, and they should find provocative his concluding sketch of an alternative strategy of liberal governance.

The argument seems to me exposed to two general lines of objection. First, Ivison's historical interpretations require stronger defenses against the major alternatives. The need is especially visible in his readings of Machiavelli and Locke. The political thought of Ivison's Machiavelli is a house divided, harboring mutually hostile classical and modern elements. Acknowledging that Machiavelli's concern for balancing the divergent "humors" that appear in all republics runs clearly contrary to the Aristotelian-Ciceronian insistence upon civil concord, Ivison yet declares that Machiavelli "remains by and large within the [Aristotelian] mixed constitution tradition" (pp. 66–7). It is possible, of course, that Machiavelli's mind *is* divided in this decisive respect, but there is a considerable body of scholarly argumentation in defense of the contrary thesis, to which Ivison, the breadth of his reading notwithstanding, devotes barely a mention. That he should give it more serious consideration appears especially advisable in view of Machiavelli's well-known acknowledgment of his own and admiration for others' artful misdirections, as well as his preoccupation with innovation and his teaching that successful innovation often requires the presentation of the new in the guise of the old. If he had paid due regard to the possibility that Machiavelli is a more radically innovative and coherent thinker than it may appear on the surface, then Ivison would have presented a more defensible

account of Machiavellian republicanism and of its relation to modern liberalism.

The case is similar with respect to the author's treatment of Locke. Although his welcome discussion of the Lockean "art of governing men in society" yet leaves room for a more comprehensive account of that art, Ivison's treatment of the design of Locke's proposed poor-law reforms represents a useful and interesting contribution (pp. 122–30). Still more interesting and provocative is his attempt to reconceive the relation of the right to the good in Locke. This attempt seems to me in principle defensible but in its particulars not fully persuasive. Paying insufficient attention, for instance, to Locke's crucial individualist qualification of his affirmation of the duty to preserve mankind, Ivison contends problematically that the common or public good simply supersedes the claim of individual rights in Locke (pp. 101–8). This failure to see the extent of Locke's commitment to the individual's good and rights is related to a failure to think through the foundations of Locke's political philosophy. At once ascribing to Locke a theological foundationalism and observing that Locke's theological arguments may be dispensable at least for some members of society (pp. 99, 119–21), Ivison fails to inquire very probingly into Locke's conception of the good or happiness that a truly rational liberty and a truly rational government would serve.

In these two cases especially, Ivison's readings would benefit by a greater effort to take seriously their subjects' philosophical aspirations, an effort that appears precluded by his sympathy for Skinnerian methods. It is regrettable that even as he affirms the virtues in contestation and defends the Skinnerian approach as enhancing our capacity for self-criticism, Ivison endorses virtually without argument a dismissive caricature of a major alternative, one which could supply some of the defects of his own approach, as "the 'present-timeless' or 'pen-pals' approach" to the study of political thought (p. 12).

The second general line of objection concerns the viability of the reconceived liberalism that Ivison's historical argument is meant to prepare. By exposing the contingent, contestable character of our identities and therewith the depth and legitimacy of the fact of moral-political pluralism, Ivison hopes to persuade us of the importance of submitting our binding civic principles to negotiation. He recognizes not only the need for civic consensus but also that the premise of deep diversity "makes agreement more difficult" (p. 165). He does not seem, however, to appreciate the true depth of the difficulty, which lies in the very attempt to construct a viable liberalism on Foucaultian (or, ultimately, Nietzschean) premises. If the self is truly decentered, then how can the determination of identity be other than an effect of mere contingency and arbitrariness, or of accident and force? On the same premise, what reason can there be for a good-faith negotiation among selves, based upon a respect for the dignity of the selves with whom one negotiates, as distinct from a reluctant acquiescence in a modus vivendi, which Ivison renounces? If philosophy is to be understood as critique alone and freedom as mere negation or transgression (pp. 27–8), then by what reason could we affirm the legitimacy of *any* particular settlement among contesting parties? What is needful is a fuller explanation of how, within this theoretical framework, we can find a space for rational liberty that involves neither a restless, quixotic struggle for liberation nor a submission to political or social necessity (cf. pp. 166–7).

The Self at Liberty admirably raises our critical awareness of the arts of government that shape us even in liberal societies. But it contributes also, despite itself, a reminder that the enterprise of political philosophy requires not only our intrepid critique of the conceptions of the good that we do affirm but also our dialectical ascent toward a better understanding of the conceptions of the good that we should affirm.

Finding a New Feminism: Rethinking the Woman Question for Liberal Democracy. Edited by Pamela Grande Jensen. Lanham, MD: Rowman & Littlefield, 1996. 304p. $67.50 cloth. $24.95 paper.

Reconstructing Political Theory: Feminist Perspectives. Edited by Mary Lyndon Shanley and Uma Narayan. University Park: University of Pennsylvania Press, 1997. 246p. $45.00 cloth, $17.95 paper.

Renee J. Heberle, *University of Toledo*

The first collection under review, *Finding a New Feminism*, offers a series (interrupted only by Jean Elshtain's essay on the Mothers' Movement in Argentina) of interpretations of canonical Western political thought that assume *the* end of feminism: a resolution of the "equality/difference debate," or what the subtitle anachronistically calls "the woman question." These essays argue that the equality/difference debate is already resolved for liberal democracy within the canon of political theory. They argue that the treatment of women by authors of canonical texts indicates a normative commitment and sympathetic attention not only to "woman's" role as she sustains community but also to women's roles as powerful individuals, thus establishing the proper balance between the conflicting values of equality and difference. The second collection under review, *Reconstructing Political Theory*, furthers the well-established but necessarily ongoing project of revising political theory to take feminist perspectives and issues into account. The essays challenge both traditional and feminist constructions of concepts already taken to be political, while further arguing that discussions of the political must take into account issues typically constructed as private concerns, such as dependency, family, children, woman abuse, and compassion.

Most feminist discussions of Western political thought hold that women are systematically disempowered by the terms on which classical, republican, and liberal theory organizes the social order. While these critiques have ranged widely in their assessments of the strengths and weaknesses of political theory vis-à-vis the treatment of the feminine and presumptions about women's place, they agree that the "tradition" has contributed in a fundamental way to legitimating profound gender injustices.

The contributors to *Finding a New Feminism* set out to show these critiques to be misguided. The authors attempt to demonstrate not only that we can derive new insights for feminism from the equality/difference debate and the relationship between the public and private sphere in the texts of the canon as written (without fundamentally challenging their constructions of gendered roles) but also that these texts actually offer women satisfactory and, indeed, celebrated roles in the social and political life of humanity. Thus, for feminist critics of the canon, these essays will appear to be performing a kind of alchemy, showing a tradition that has been argued to be masculinist at best and mysogynist at worst to be a kinder, gentler force that empowers women to realize the best form of society. The roles prescribed for women in the canonical literature as interpreted here create the terms on which universal emancipation becomes more likely (Diane Schaub on Montesquieu) or offer women a complementary position to men that gives them effectual rather than nominal

control (Catharine H. Zuckert on Machiavelli and Ann Charney Colmo on Rousseau). Elshtain shows how a private role, mothering, can be deployed in the public sphere to hold up a mirror of morality to a militarist regime and a cowed population. Arlene Saxonhouse, in contrast, shows the dangers of enveloping public concerns (about war) into the world of the private (sexual desire) and losing what is valuable about the distinction between the spheres.

The essays together present a sophisticated argument for an ideal complementarity of gender roles (my construction) wherein women as mothers and erotic partners challenge and enrich the public world of men. Thus, the "woman question" can be resolved by imagining a society shaped by a version of equality (as shared by rights-bearing individuals who desire the opportunity for differentiation) which will allow us to remember the good things about why the sexes are essentially different (erotic attraction) and inhabit separate spheres (mutual support, moral education). Essentially, political theory does not have to change anything it already says about what women are doing in order to establish an argument for a complementarity between the sexes which will enhance liberalism: As feminists we must reevaluate women's roles as empowered positions from which to influence politics and allow and encourage them to do it better. This approach ultimately endorses traditionalist arguments about the necessity of gendered spheres, albeit in revised (and perhaps even more romanticized) form.

For example, Zuckert argues that Machiavelli's comedies show us how the prudent management of the household allows for a successful outcome for all involved, men and women. The management of the private sphere must parallel and cohere with the management of the public sphere. Women are in charge of and wield significant influence on the public world of appearances through the real world of household relations; therefore, for Machiavelli, women potentially embody virtu. In fact, since women are better at managing the private emotions, they may be superior to men, who only experience their desire but cannot channel it to achieve the best outcome. This essay is a more likely effort to recuperate Machiavelli for liberal feminist purposes than is Colmo's effort to recuperate Rousseau through a reading of his essay, "Emile and Sophie." Colmo attempts to recast Rousseau's idealized constructions of the "separate but equal" sphere of the feminine as empowering for women. She ignores the notoriously mysogynist context of Rousseau's larger body of work and never questions whether someone who so clearly fears and despises "woman" as perpetrator of the fall of man from the grace of nature can be relied on to establish her freedom. Colmo reads Rousseau figuratively, claiming his construction of Sophie and Emile together represents "the tension of what it is to be human." It is an unpersuasive effort to challenge feminist interpretations that criticize Rousseau because he creates the ideal woman for man, while he creates the ideal man for himself and for the masculine community.

In an essay that represents the "real life" issues at stake in reassessing woman's role as given under the terms of the Western tradition, Elshtain's scattered, personalistic reflections on the mothers of the Plaza de Mayo celebrate these women as they reconfigure the Argentine iconography of motherhood from "weeping woman" into "defiant witness." The Mothers' Movement was a significant force in exposing the cruelty of the militarist state that ravaged civil society in Argentina from 1976 through 1983. What kind of role it played in the democratization of Argentina in the wake of the Dirty War is a different and extremely complex matter. After the demise of the military regime in 1983, the movement split due to the "politicization" of the group. The split and subsequent marginalization of the movement were also about who would "own" the memory of the Dirty War. Elshtain only interviews her contacts on the side that insists on remaining "apolitical," in the sense of avoiding class politics or involvement with other popular movements. Elshtain's celebration of the mothers who would define their participation exclusively as safe harbors for the memory of suffering reflects the undemocratic imperative in Elshtain's critiques of feminism. That imperative is that woman remain in place as mother even as she shows herself in the public sphere in the name of democratization. Elshstain shows how the mothers, as they render public a previously private suffering, can use that identity in the name of democratic freedoms, but she suggests they should avoid becoming participants beyond that role. This aspect of Elshtain's argument reflects that of the volume as a whole, as it issues the call for woman to become a new and improved version of the identities Western political thought has always alloted her.

A word on the final essay brings me to another general criticism of this collection. Mary Nichols offers textual support to show that, rather than arguing for a natural and therefore just hierarchy between the sexes, Aristotle in fact thought that nature imbues both male and female with masculine and feminine traits. We can each embody the best characteristics of both genders. He does not say this directly, according to Nichols, because he was writing to a particular audience. She moves on to show how Aristotle's enlightened naturalism is reflected in the movie *Fried Green Tomatoes*. Nichols argues that the character Idgy represents Aristotle's ideal person, while in an aside she praises the film for excising the "lesbian component" of Idgy's relationship with Ruth (which is made clear in the novel). Nichols thus implies that this component of their identity and relationship has nothing to do with the "community" they are able to create in the context of an illiberal society. But could it not be the very illiberality of the moviemakers and of the movie-going public that led to this excision? It is particularly odd that the author would praise the movie for not "focusing" on the lesbian (read: sexual—and therefore irrelevant to the moral of the story) aspect of their relationship, and therefore getting the message about individuality and community across more powerfully, when that occlusion is precisely what inhibits the community of recognition and individuality the author is praising. There is clearly no room for the expression of lesbian desire or identity in the ideal complementarity Nichols finds that either sex may potentially embody.

In the introduction, Jensen dismisses contemporary feminist arguments that suggest differences among women may be as significant for a vibrant and liberatory feminism as what women have in common as well as efforts to deconstruct dualistic gendered roles altogether in the name of experimentation with freedom from the heterosexual matrix (obviously and unquestioningly celebrated in this volume). Her critiques construct these arguments narrowly in order to dismiss them more easily. Given the silence of the collection on race, class, and sexual identity, enforced by infrequent but significant swipes at identity politics (apparently "woman" as a group is a difference that can uphold liberal values, but race, class, and sexual group differences are anathema to the humanism espoused in this volume), I think this volume "finds" not a new but a very old, middle-class, Eurocentric feminism. As the authors fail to take issues of concrete and historical differences among women into account, preferring to treat "difference" as an abstract quality that comes naturally to all humans in their efforts to "differentiate," the new feminism of ideal complementarity will resonate for those women who

already associate "womanness" with ideal versions of the female role in the nuclear, heterosexual, bourgeois household. The abstract ideal of community and individuality all wrapped into one package and then labeled "liberalism" is eloquently described. For all its eloquence, however, I am not persuaded that the conditions prescribed for women in achieving it are or should be the aspiration of all women. They are certainly not conditions readily available to all women.

The essays in *Reconstructing Political Theory* are more diverse in their topics and purposes. The authors critically engage feminist (and nonfeminist) arguments and clarify some of the complex, interdisciplinary terrain of feminist political theory. The essays are held together by the fairly general goal of challenging typical constructions of political concepts and can be divided into two categories: those that advance (to varying degrees) discussions of concepts to which feminists historically have had ambivalent relationships and those that redescribe concepts traditionally thought of as apolitical or prepolitical.

Two essays on power, Anna Yeatman's "Feminism and Power" and Martha Ackelsberg's "Rethinking Anarchism/Rethinking Power," incorporate poststructuralist critiques of power as only repressive and of identity as unitary. Ackelsberg discusses the diffusion of power as theorized by anarchists and poststructuralists. She argues for social action that works at the very local level, "not on the basis of some universalized narrative about revolution but on the basis of challenging [a] particular oppressive relationship(s)—[which] partial and limited though it might seem, is the only politics consistent with a commitment not to deny the differences among us" (p. 171). Yeatman articulates the ambiguities of state power vis-à-vis emancipatory movements. State power as seen by emancipatory movements has usually been conceived as either coercive (which leads to a politics of rancor) or protective (as the giver of rights and entitlements). She points out, for example: "In general, women's accession to the rights of the modern individual is understood to have followed from the state's conferral of these rights. This is in direct contrast to the origins myth of men's rights as modern individuals" (p. 149). She goes on to argue for a concept of power as "capacity," which would allow feminism to take into account that women are capable of "violence, strategic cunning, and all kinds of wickedness; those which are playful and satirical as well as those which are harmful to others" (p. 154). These essays help show how rethinking power as a concept can also help us rethink the politics of social action—of what we *do* in the world as self-constituted groups and as individuals.

The final two essays in the volume also encourage us to think about how concepts are constructed and affect social action. Essays by Kimberle Crenshaw and Nancy Hirschman on violence address how policymakers and activists respond to battering. They suggest that policies ignore the lived context of women's lives and are based instead on the imagined figure of a white woman, or on abstract concepts of freedom realized by either a removal of barriers or an offering of protection, neither of which fully empower women's choices.

Taking Yeatman's essay as a point of departure, one can identify a weakness in some of the other essays, that is, they gesture toward the state as offering the means to realize their vision without acknowledging that the state is already part of the problem. The protective state (using Yeatman's construction), it seems to me, shadows their theory.

Anita Allen's essay on rights in the context of privacy as a constitutional and tort issue offers a brief historical survey of privacy doctrine (as founded in sexist ideologies about women's modesty) and an abbreviated argument of the virtues of privacy over equality as a value shaping jurisprudence about women's lives. I say "abbreviated" because, while Allen rightly points out that equality can be interpreted in a conservative fashion (as equality of opportunity rather than of result), she does not show why privacy is any more likely to be interpreted in a progressive fashion (as freedom to carry out decisions rather than as freedom from undue influence from the public or state). Her essay offers some useful reminders of the immediate strategic use of privacy for those populations most vulnerable to state and social scrutiny, but Allen fails to persuade that it is necessarily more progressive than equality in the long run. Privacy in itself does not empower, and Allen's suggestion that it can include an imperative on the part of the state to subsidize through education and/or funding the private choices made is simply an "add-on" strategy that does not acknowledge the ultimate weakness of privacy doctrine: It artificially removes individual decision making from the sphere of the social. Relying on the state or society to empower choices of individuals "as if" they are purely private may obscure the constitutive effects of state power and end up buttressing the very real scrutiny and pressure Allen is trying to avoid in advocating privacy in the first place.

Uma Narayan also relies on state largess to realize her reconstructed vision of citizenship. She argues that, rather than being defined by individual contributions (usually thought of as wage work) or nationality, citizenship should be defined by ongoing participation in a given political space. Preserving the rights of citizenship should include preserving basic social dignity for all individuals rather than preserving the negative right to self-government or the positive right of needs fulfillment. This subtle but important distinction is drawn out effectively. Yet, the state not only will be "affected" by this new version of citizenship and participation but also will make it possible through creating the conditions necessary for all to participate. Neither Allen nor Narayan acknowledges the ambivalent relationship feminism sustains with the liberal/capitalist state (for an excellent recent discussion, see Wendy Brown, *States of Injury: Power, and Freedom in Late Modernity*, 1995).

Two essays on "rights" further discussion of this especially contested concept. Elizabeth Kiss addresses feminist concerns about rights. She maps out the existing literature, showing both the conceptual and the practical politics of rights as constructed and critiqued by liberal feminists who support rights and by theorists of care who reject rights. In doing so she argues for a version of rights that will sustain private autonomy while fulfilling the obligations of public community. Yet, she does not finally tell how, in various contexts in which political decisions are made about particular individuals' lives, we might negotiate the often conflicting issues of the right to autonomy and the demands of mutual obligation, or morality. Similarly, in "Revisioning the Family," Shanley and Minow, while articulating a theory of rights as "relational" rather than as purely individuating, do not finally show how a judge, in making a decision about custody in the context of a surrogate pregnancy contract case, for example, might take the "moral and psychological dimensions of persons into account" (p. 102). Perhaps an imagined judicial opinion that takes relational patterns of life into account would help us recognize the paradoxes and ambiguities of putting these theoretical imperatives into practice, particularly within the context of the liberal/capitalist state.

The interpretive essays in the volume are those that move to include certain concepts in the political: the role of

children in politics (Elshtain), the notions of dependency (Fraser and Gordon) and compassion (Spelman). These essays reinterpret practices that have affected women (and children) historically but are not recognized or valued as legitimately political. Considered in the context of feminist theory writ large, they contribute effective and well-grounded arguments about the politics of the personal without collapsing the one into the other. Elshtain's essay on political children complements her essay in *Finding a New Feminism*. Yet, I would pose the same question to Elshtain that I did regarding the politics of the Mothers' Movement. That is, what happens to the political subjectivity of mothers and children once in the public sphere? Must they (will they) remain the mother and the child? Will, or should, that identity not be significantly affected? Would Elshtain then be disturbed that they are forgetting their true selves?

Finding a New Feminism counters feminist skepticism of the canon with representations of an idealized complementarity between the two (and only two) genders. These essays will offer students of political theory a sophisticated version of a contemporary conservative argument that is caricatured in debates over "family values." *Reconstructing Political Theory* offers a substantive advance in feminist political theory, as the essays map out and push feminist and nonfeminist scholarship forward on a number of political concepts. I appreciate the clarity with which the essays elaborate and advance traditional and feminist debates about critical issues of politics and everyday life.

The Holy War Idea in Western and Islamic Traditions. By James Turner Johnson. University Park: Pennsylvania State University Press, 1997. 185p. $45.00 cloth, $16.95 paper.

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This study compares the development of Christian and Islamic conceptions of holy war from antiquity to the present. Johnson nicely brings out several different meanings of the idea. A holy war may not mean a specific call to arms, for example, but a much more general command to strive for faithfulness (the "greater" form of *jihad* according to the prophet Muhammad). Holy war can also mean a war fought by God himself, by his duly authorized representatives, or by participants who are ritually or morally holy. While holy war is commonly thought of as an offensive war to spread religion or to punish deviance, Johnson draws our attention to the fact that "the people of the Book" have emphasized the defensive character of most wars of religion. (Unfortunately, this fact provides only limited comfort, as both traditions have often expanded their definition of "defensive" to justify essentially offensive and punitive wars.)

Both Christian and Islamic traditions begin from the idea of two worlds, one ordered toward God, the other not. As Johnson explains, for Christianity the distinction is primarily theological and motivational: the City of God cannot fully exist on earth, even in the church, but its members can be identified as those who are moved by love of God. For Islam, the distinction is primarily juridical and behavioral: Muslims are charged with replacing *dar al-harb*, the non-Islamic world of strife or "war," with *dar al-Islam*, the world of true peace, but this goal is one of achieving outer submission to law, not forcing inner conversion of belief. Johnson might have interrogated this distinction between outer submission and inner faith a bit more vigorously, as well as paid more attention to Islamic "modernists" who have challenged the medieval *dar al-harb/dar al-Islam* distinction as having little basis in an Islamic ethics derived from the Qur'an and the practice of the Prophet. Nevertheless, Johnson is certainly right that this distinction has dominated the tradition.

According to the classical jurists, only the *imam*, who is at once the supreme religious and political leader, has the authority (and duty as representative of the community) to declare offensive *jihad*. Because the line of succession from the Prophet has broken down, both the Sunni and Shi'ite Islamic traditions agree that there is now no one with the authority to declare an offensive *jihad*. Only a defensive holy war is possible, a view that fits fairly well with current international law. As Johnson explains, two later ideas of justification and authority are not as comfortable: the idea of a "defensive" war to recover previously possessed lands, based on the duty of each individual Muslim to protect Islamic territory; and the idea of *ghaza*, an offensive war in which military success proves the presence of the blessing of God and establishes the leader as direct heir of the tradition that began with the Prophet (Saddam Hussein invoked both ideas during the Gulf War). Johnson is quite good at taking the reader through the complexities of Islamic thought about the possibility of authorizing holy war, as well as through the many historical reasons that there is no longer any recognized justification or authority for holy war in the West. He is particularly informative on some of the more recondite sources of the latter development, such as the medieval Christian redefinition of the church's authority over war as juridical rather than executive in character.

Johnson rejects as inaccurate the common idea that holy war is waged without constraints, although he notes that Christian medieval standards were not aimed at constraining war outside Christendom, while classical Islamic doctrine from the beginning was meant to limit outside wars. Johnson argues that both traditions began with the assumption that war established belligerency between entire populations. The traditions then used "functional" criteria to identify noncombatants as those who were generally unable to bear arms and thus need not be killed (but who might be enslaved). According to Johnson, what is missing until the early modern period in Europe is the notion that persons have a basic human right to be treated as noncombatants unless they pose a culpable threat to others. Johnson observes that classical juristic ideas of noncombatancy still have normative force within the Islamic community, and this "constitutes a deeply problematical difference" between the traditions (p. 127). Nevertheless, he claims that many Muslims would condemn violations of modern noncombatant rights, such as terrorism and assassination, as having nothing to do with the broader requirements of *jihad*. It would have been helpful for Johnson to explore in more detail the reasons available within the tradition for making such judgments and also to have noted that modern Western justifications of noncombatant rights are more "functional" than may first appear.

The contemporary West sees the idea of holy war as a threat to civilization, yet "traditional Islamic culture finds in the Western concept of the secular state, the very political institution which makes holy war unthinkable, an offense to God's will for the right ordering of human community" (p. 15). Johnson tries to overcome this stark opposition by developing a more informed and sympathetic picture of each side (and by showing that there are, in fact, many sides). To advance this project, it will eventually be necessary to join the substantive moral and philosophical issues more directly, as well as face more squarely the possibility that some disagreements between these traditions can only be negotiated, not fully resolved. Johnson's book is intended as the beginning of such an engagement. It is not so much a work of political

theory as an historical and analytical study of how these traditions have thought about the justification, authorization, conduct, and statecraft of holy war. A short review cannot do justice to its historical detail and nuance, but this is an interesting, informative, and imaginative book, worth reading by anyone interested in the subject.

The Reign of Law: *Marbury* v. *Madison* and the Construction of America. By Paul W. Kahn. New Haven, CT: Yale University Press, 1997. 324p. $35.00.

Helena Silverstein, *Lafayette College*

One might think that a book with this subtitle would be largely devoted to an exploration of that paramount case. One might also think that by now legal scholars would have squeezed every drop of juice out of the most prominent case in American legal history. In both cases, one would be wrong. Paul W. Kahn offers a nuanced interpretation of *Marbury* v. *Madison*, but his attention is devoted more broadly to a theoretical exploration of the rule of law.

Kahn engages in what he terms an archaeological inquiry into the rule of law. Such an inquiry aims at the recovery of an event through examination of "the multiple layers of meaning constitutive of a single paradigmatic event" (p. 40). For Kahn, *Marbury* v. *Madison* is the event, and the goal of its recovery is to understand the structure of the imagination constructed within and maintained by that event. What Kahn recovers from *Marbury* is the imagination of the rule of law as a system of political order. This archaeological inquiry and recovery is founded on Kahn's view that the rule of law is a belief system structured by the imagination. As a belief system, the rule of law gives meaning to our political experiences, offers an organization of power, and provides a perspective for understanding both the community and the self.

Like all belief systems, the rule of law competes with alternative imaginations and ways of understanding our political world. Most important, law's rule, as imagined and constituted by *Marbury*, competes with a politics of revolution. The Court's ruling, Kahn argues, is an attempt to construct a political system governed by the rule of law and, as such, reveals an effort to counter and replace the politics of revolution offered by Jefferson. *Marbury* "represents the judicial contestation of the meaning of American political life in response to Jefferson's successful electoral campaign to wage a new revolution" (p. 10). But this contest over meaning is not a contest over fact. It is, rather, a battle of appearances, a struggle to capture the political imagination. Thus, for Kahn, the rule of law, like the politics of revolution and all politics, "is a matter of appearances. The truth of the rule of law *is* its appearance" (p. 27).

Kahn's analysis seeks to elaborate this appearance. The appearance of the rule of law should be understood in direct contrast with the appearance of action and revolution. Law's rule suppresses action, novelty, and revolution, for when action and revolution occur, law disappears. Law exists between revolutions, and the beginning of a new revolution marks the end of law. Although law and revolution are in opposition, our conception of one requires the conception of the other. Revolution needs law to negate, to define its boundaries, and to reveal to whom the revolution belongs. Law needs revolution to provide its foundational origins. Law draws legitimacy and validation from the prior revolution, yet the project of law's rule is to postpone revolution indefinitely. "To maintain the rule of law is to say 'not yet' to revolution, while saying 'yet still' to past revolution" (p. 67).

The elaboration of this conflict between law and revolution is the most compelling component of Kahn's book. Beyond *Marbury* (and it is worth emphasizing that much of Kahn's discussion of law and revolution goes well beyond this case), the illumination of this conflict has important implications for scholars interested in the relationship between law and social change. If the ambition of law is to conceal novelty and action, then those who push change through the use of law seek to engage in political action within an arena which suppresses that very action. Kahn does not resolve the tension between law and action, nor does he suggest that it is resolvable. If he is right, then those interested in law and social change will find it necessary to consider and respond to the paradox Kahn presents.

Scholars interested in the notion of representation may also be served by Kahn's work. Exploring the rule of law, Kahn finds the appearance of political representation. Distinguishing the representative character of the rule of law from the representative character of elections, Kahn expresses the contested and indeterminate nature of popular sovereignty. Within this indeterminacy, however, Kahn notes that the courts offer an appearance of representation, "because the rule of law, which informs their conduct, derives from the Constitution," a document authorized by the people (p. 203). The people become the locus for political authority, replacing the divine. This appearance of the people within the rule of law, Kahn suggests, is a claim to power that contributes to our way of knowing and experiencing the political world. Although Kahn does not use this view to answer explicitly the countermajoritarian critique of the courts, he adeptly draws on Foucault's discussion of power and knowledge to raise that perennial issue in legal scholarship.

Surprisingly, Kahn's direct treatment of *Marbury* seems almost incidental to the broader theoretical discussions of the rule of law and representation. Kahn examines *Marbury* by displaying the rhetorical techniques employed by the Court to advance the rule of law and suppress the appearance of action. Although the discussion seeks to illustrate how *Marbury* constructs law's rule, it is not difficult to get lost in the details of this rhetorical reading. Still, Kahn offers a unique interpretation of *Marbury* that will be especially appealing to those interested in rhetorical analysis.

There are moments of deep insight within this book's cultural inquiry into the law. The discussion of law and revolution as well as the analysis of law's relationship to time and subjectivity are examples. In these moments, the author's digging through the depths of law reveals significant structures of what has become known as the rule of law. But, as with all archaeological endeavors, there is much dirt and sand through which to sift, and at times the reader may feel like the archaeologist, sifting through the author's prose for the significance that lies within.

Liberalism's Crooked Circle: Letters to Adam Michnik. By Ira Katznelson. Princeton, NJ: Princeton University Press, 1996. 192p. $19.95.

William A. Galston, *University of Maryland, College Park*

Liberalism's Crooked Circle is not (in any of the usual senses) an academic book, and it does not call for a typical academic review. It takes the form of two open letters to the Polish dissident Adam Michnik, letters from a self-proclaimed American, radical, and Jew to a Polish counterpart. In the classic manner of this genre, it seamlessly blends personal reflections, politically and historically situated analysis of

current conditions, and broader theoretically informed proposals. I admire this book, despite certain disagreements with it, as a model of informed moral and political engagement with some of the deepest social issues of our day.

The book is directed against Western triumphalism and complacency in the wake of the end of the Cold War, against what Katznelson considers a "haughty, self-enclosed liberalism" (p. 18). And not just in the victorious West; the author is gently but firmly critical of the Eastern European dissidents who, he believes, were pushed by their aversion to totalitarianism into the reactive embrace of one-sided (Hayekian or Popperian) theories of negative liberty.

Katznelson addresses two principal substantive issues: the relation between liberalism and socialism and between liberalism and cultural pluralism. Strikingly, this turns out to be an inquiry *within* liberalism: Katznelson is searching, he says, for a "liberalism of adjectives" (p. 26). The experience of the twentieth century leaves us no morally and politically responsible alternatives to liberalism. The bedrock liberal guarantees of constitutional democracy and individual liberty must be primary and nonnegotiable. So in the end we must talk about socialist liberalism, not liberal socialism; and pluralist liberalism rather than any of the currently fashionable anti-liberal theories of "difference."

With regard to the first issue, Katznelson argues that liberalism needs socialism. He cites Stephen Holmes as a reluctant witness for the proposition that while liberalism permits a redistributive egalitarian politics, it neither requires nor by itself creates that practical basis for such a politics (p. 50). (He does not seriously question the claims of thinkers such as John Rawls and Ronald Dworkin that liberalism's underlying moral principles lead to theories of economic distribution as well as political organization.) More broadly, Katznelson asserts, the characteristic "thinness" of liberal institutions creates space for illiberal social movements, against which socialism is needed as a counterweight.

But what kind of socialism? Katznelson wants to revive the project of Laski, Hobhouse, Orwell, and Dewey in terms relevant to our times. This means accepting the primacy of liberal constitutional democracy and within its institutions creating a "self-limiting socialism" (p. 24) characterized by (1) the permanent separation of property and sovereignty, (2) the abandonment of the utopian goal of a total end to exploitation and scarcity, and (3) the acceptance of the constitutive liberal distinction between the public and private spheres. Now that socialism has surrendered its claims to be an alternative form of economic organization, the focus must shift to the control of capital, organization of markets, and rectification of market inequalities. In particular, property rights must be structured to lean against the destructive consequences of unfettered capital (pp. 84–6). This is a socialism chastened by the twentieth-century catastrophe of socialist intentions, so chastened, in fact, as to be virtually indistinguishable from the welfare-state liberalism of Rawls and Dworkin.

With regard to the second major topic, the relation between liberalism and cultural pluralism, Katznelson insists that there is no alternative to an expansive liberalism as pluralism's best defense, a liberalism that avoids both the repudiation and the deification of differences. One theoretical hero of this effort is Isaiah Berlin, who has "aimed to place within liberalism a rich pluralism of values and forms of life" (p. 161). Berlin achieves a generous pluralism without sliding into self-defeating relativism. The second hero is John Locke, whose account of toleration, Katznelson argues in one of the more interesting sections of this book, offers a better guide to pluralist liberalism than Mill, whose "obsessive antitraditionalism" makes him an enemy of the kinds of differences that should find a home within a generous liberalism (p. 141). Contemporary liberal theorists of cultural difference (Rawls, Kymlicka, Tamir) are asking the right questions but oversimplifying the dilemmas of difference (especially communal religious practices) in ways that reduce the relevance of their answers (p. 155).

There is a leitmotif in the discussion of cultural difference to which I cannot do justice in this brief review, namely, the historic role of Jews as both central and marginal. Katznelson's heartfelt personal understanding of these issues produces one of the most extraordinary passages in *Liberalism's Crooked Circle,* his criticism of Adam Michnik for being insufficiently Jewish. In Katznelson's view, Michnik has not solved the problem of affirming Jewish difference within a Polish national identity so deeply constituted by Catholicism. Michnik's desire to speak and act as a Pole means soft-pedaling the Church's historic complicity in anti-Semitism (pp. 116–23).

In practice, Katznelson's pluralist liberalism amounts to a regime of tolerance, defined as the right not to offer reasons for being different and backed by institutional and legal protections, especially in circumstances of inequality between majority and minority cultural power (pp. 171–3). In this connection, Katznelson invokes Susan Mendus's provocative metaphor of "neighborliness" as an alternative to a community.

While Katznelson's account of pluralist liberalism points us in the right direction, it leaves unanswered the "daunting question" of how the general precepts of political liberalism are to be squared with, or balanced against, the diverse perspectives and practices of subcommunities within a liberal polity. I regret that Katznelson did not conclude this book with a consideration of specific cases and controversies. Does he believe that religious communities should be exempt (on faith-based grounds) from some otherwise valid laws barring unequal employment practices for women? What liberty (if any) do Christian Scientist parents have to withhold their children from standard medical treatment? Was the touchstone case of *Wisconsin* v. *Yoder* rightly decided?

Despite these omissions, I am grateful for the specificity and candor (and warmth and vigor) of this book. I do not think I will be alone in reading it as a sign of the exhaustion, implausibility, and inhumanity of all alternatives to liberal constitutional democracy in the late twentieth century. The task before us is not to transcend liberalism but to enrich it in ways hospitable to the needs of a fuller human liberty.

Against Liberalism. By John Kekes. Ithaca, NY: Cornell University Press, 1997. 244p. $29.95.

The Growth of the Liberal Soul. By David Walsh. Columbia: University of Missouri Press, 1997. 386p. $39.95.

Aryeh Botwinick, *Temple University*

These two books in different ways seek to chart the shortcomings and limitations of liberalism, and their critiques are worthwhile and interesting. Yet, they share a common blind spot in that, under some possible redescriptions, liberalism can be shown to rebound from their criticisms and to situate itself at some point beyond them.

Writing in the tradition of Eric Voegelin, David Walsh conceives of liberalism as a system of ordering. Because it can neither fully articulate its theological and philosophical roots nor shake loose from them, it has given rise to a certain amount of confusion with regard to its fundamental commitments and values. For example, the liberal stress on human

dignity, equality, and freedom needs to be seen as both deriving from and being appropriately completed by the insights of Christianity. Walsh says that "liberal order is the trace of Christianity in a world from which it has withdrawn" (p. 201).

Walsh divides his argumentative and expository focus among historical reconstructions of key thinkers in the Western political tradition, from Hobbes to Hegel and Tocqueville, highlighting the religious dimension in their thought and making connections between his Christological framing of liberalism and central issues on the contemporary liberal political agenda. For example, with regard to abortion, Walsh wants to draw a straightforward linkage between the affirmation of the human person within Christian theology and a resistance to abortion. He says that "liberty means very little if there is no recognition of an unconditional right to life. How can the exercise of liberty be protected if we have not agreed that the being who possesses it must first be preserved" (p. 72)?

The most valuable part of Walsh's analysis consists in his explication of the metatheoretical problematic of liberalism. Walsh shares in common with Thomas Nagel and others the view that neutrality cannot be regarded as the ultimate value of liberalism, because neutrality tacitly presupposes a particular conception of the good that identifies the good with neutrality. Designating neutrality as its ultimate value has ensured "from the beginning" that liberalism would be "the tradition of the quest of the source from which it is derived" (p. 246).

It is at this juncture in his argument that Walsh proposes Christianity as the not-fully-articulated source of the conception of the good animating liberalism. Christianity for him becomes the premise that is best capable of explaining "why men and women are all equal and utterly inviolable sources of rights, deserving of the dignity that goes beyond any and all achievements within this world" (p. 247). It is not just Christianity that is conducive to this result, however, but the skeptical approaches that call Christianity into question as well. If we follow the route of the skeptic by acknowledging that we can call our knowledge statements and beliefs into question, then none of us is entitled to accord a higher status to our own statements about the world in contrast to those put forward by our competitors, and in a negative sense (since none of us can outdo our fellow human beings in the project of justification) our equality vis-à-vis one another and the need to respect one another's positions is established.

Of course, the connection between epistemological and metaphysical skepticism and political liberalism is merely rhetorical and informal in character. The skeptics in conformity with their own skepticism cannot tease out of their premises a particular blueprint concerning how to organize political life. By the same token, however, the connections between Christianity and liberalism also need to be regarded as informal and rhetorical in character. Out of the complex weave of theological and historical motifs that goes to constitute Christianity—which for long stretches has included a salience being assigned to authoritarian governing structures, to match the supremeness of divine rule—the notions of freedom and equality can be extrapolated from it and invoked to serve as the backdrop to liberalism only by way of highlighting an affinity, not by way of underscoring a tight logical, theoretical, or historical connection.

The juxtaposition of the tenets of skepticism to liberalism rather than the theological postulates of Christianity becomes even more compelling in view of Walsh's skeptical Voegelinian hedgings concerning the notion of order itself. Walsh says that "symbolisms of order are what structure our experience of reality. They provide the context in which analysis and critique of specific issues can be carried on, but they are not themselves amenable to the same kind of objective assessment. Only from within the perspective of participating in them and only in relation to the landmarks disclosed within them can they be evaluated" (p. 261). Only ways of life are internal to specific symbolic ordering systems; there is no transcendent external perspective—no set of master symbols to order all the other symbolic systems that we use—in terms of which the external validity in contrast to the internal coherence of a set of symbols may be assessed. The dilemma to which Walsh points is already implicit in Eric Voegelin's formulation: "All the gods have to live under pressure of a divine Beyond that endows them with their divine life while threatening to let them die from their compactness" (cited in Fred Dallmayr, *Margins of Political Discourse,* 1989). Walsh in his own way thus interprets Christianity both skeptically and relativistically as meeting a test of internal coherence while remaining arbitrary from a more externally grounded perspective. Liberalism shares the same fate as Christianity. It, too, is internally coherent or can be rendered such by adjusting political practice to reflect and conform to whatever set of components in its core principles one takes to be primary, while not being able to vindicate itself outside the charmed circle created by its own set of preferred postulates. In this regard, Christianity is not ontologically superior to liberalism. Why use one to "complete" the other?

John Kekes believes that pluralism, freedom, rights, equality, and distributive justice within liberalism achieve their maximum point and force when grouped around the central value of autonomy. These other components of liberal thought stand or fall with the defensibility of autonomy. Kekes argues that the stress on autonomy within liberalism is vitiated by the prevalence of evil in the world. This, in turn, leads him to propound the thesis that "the evil that people habitually cause reflects on them adversely, even if they cause it nonautonomously" (p. 212). Liberals overoptimistically assume that "virtues are autonomous, and vices are not" (p. 202). A more realistic assessment would get them to see that increasing autonomy harbors as much prospect for increasing vice as it does for promoting virtue.

With regard to liberalism's commitment to pluralism, Kekes points to a fundamental issue of consistency. Since liberalism is liberal about everything except about the necessity of maintaining a liberal society, its value allegiances resist consistent formulation. "Pluralism" conjures up the prospect of an antiliberal society as well as of a liberal one, but the point of including "pluralism" among the litany of liberal values is to prevent such an actualization. The upshot of this analysis is that liberalism must renounce its adherence to either liberalism or pluralism, since it cannot consistently sustain both.

This last criticism brings us to the heart of Kekes's argument and helps isolate the precise juncture at which it goes astray. Kekes is driven to locate autonomy as the central value of liberalism because he is primarily interested in liberalism as ideology, not as theory. He is preoccupied with liberalism as a set of organizational principles integral to a particular mode of collective being in the world, rather than as a series of reflections responsive to some of the deepest questions human beings have raised about themselves and the world. He arrests his inquiry at the level of ideology, without proceeding to the more broadly theoretical domain.

From an enlarged theoretical perspective, we could say that the major value informing liberalism is skepticism rather than autonomy. Autonomy itself can be viewed as derivative

from skepticism. Liberalism stresses the role of the self-directing, free individual as the authentic subject of political deliberation and action because, given the limitations to human knowledge advocated by skepticism, it is only a free, self-directing human being who can generate enough data concerning human needs, ends, and wants to satisfy the requirements of political deliberation and action. Skepticism faces a serious challenge of consistency, however. It cannot be formulated without engulfing, and therefore also undermining, itself. The version of skepticism that manages to escape these strictures is a generalized agnosticism, an agnostic approach extended across the whole map of knowledge, beyond the philosophy of religion; it facilitates the cultivation of an indefinite openness toward the future, an endless deferral of final crystallization of responses to the supreme questions of life and to the balances that should subsist among them.

A generalized agnosticism enables us to resolve the tension between liberalism and pluralism. Precisely because it seeks to nurture continuing explorations by multiple communities of investigators into different sectors of experience, a generalized agnosticism remains open to the prospect that multivalued logics which create space for more possibilities than just A and not-A may eventually overshadow the two-place Aristotelian logic as a more accurate encoding of the structure of reality. Such logics chart the suspension of the law of the excluded middle and thereby enable us to circumvent the contradictions present in the formulation of liberalism. Under the aegis of multivalued logics, we can map liberal pluralism without either confirming or denying its predication upon not-liberalism.

From a generalized agnostic perspective, redrawing the balance of liberal moral and political values so as to include greater attentiveness to the need for "prosperity, order, civility, peace, a healthy environment, security, happiness, and law-abidingness" (p. 210) does not represent a departure from liberalism but an elaboration of its commitment to pluralism. Kekes says at one point that "realistic principles must be derived from, formulated in response to, and continually revised to fit the changing, shifting, unpredictable contingencies of actual human circumstances" (p. 155). The liberalism whose primary theoretical notion is skepticism remains open on an intermediate level of deliberation and decision making to the pragmatic renegotiation of its distribution of value that Kekes envisions.

A Solution to the Ecological Inference Problem: Reconstructing Individual Behavior from Aggregate Data. By Gary King. Princeton, NJ: Princeton University Press, 1997. 342p. $55.00 cloth, $16.95 paper.

Douglas Rivers, *Stanford University*

A Solution to the Ecological Inference Problem was published with much fanfare. The NSF, which funded the research, issued a press release, and the *New York Times* and *Boston Globe* printed stories. Few works by political scientists, much less equation-filled tomes like this one, attract that kind of attention. The reason for the notoriety is Gary King's bold claim to be able to perform a seemingly impossible feat. Since the publication of Robinson's seminal paper in 1950, generations of social scientists have been warned about the "ecological fallacy." Has King really found "a method of inferring individual behavior from aggregate data that works in practice" (p. 1)?

The most popular method for ecological inference has been ecological regression, which was described by Leo Goodman in the *American Journal of Sociology* in 1959. King's proposed method is a random coefficients ecological regression model. The running example in his book involves data on precinct-level turnout rates (T_i) and the proportion of voting-age people in the precinct who are black (X_i). In King's notation, β_i^b and β_i^w are the proportions of blacks and whites, respectively, who turn out in precinct i, so

$$T_i = \beta_i^b X_i + \beta_i^w (1 - X_i). \quad (1)$$

Usually, the goal is to estimate the district-wide fractions of blacks and whites who turn out, denoted B^b and B^w, which are weighted averages of β_i^b and β_i^w. In ecological regression, B^b and B^w are estimated by a regression of T_i on X_i and $1 - X_i$. Goodman noted that if X_i is independent of β_i^b and β_i^w, then ecological regression provides unbiased estimates of $B^b = E(\beta_i^b)$ and $B^w = E(\beta_i^w)$. This is a well-known result in random coefficients regression models.

To this standard setup, King makes two original and useful suggestions. First, equation 1 differs from the usual random coefficients regression model because the coefficients must satisfy the inequality restrictions $0 \leq \beta_i^b, \beta_i^w \leq 1$ and because there is no disturbance. King suggests assuming a convenient distribution for β_i^b and β_i^w (truncated bivariate normal) and estimating the parameters of this joint distribution, including B^b and B^w, by the method of maximum likelihood. Second, King suggests estimating β_i^b by $E(\beta_i^b|T_i)$ and then aggregating across precincts to estimate B^b. (King actually uses a slightly more complicated procedure involving the predictive distribution of β_i^b, but the details are unimportant.) He effectively applies the computational machinery of modern Bayesian statistics to this setup.

It is important to understand that the assumptions required for King's estimator include all those required for Goodman's estimator and then some. Or, put another way, if the assumptions of conventional ecological regression fail, then King's model is also misspecified. I do not think that the additional assumption made by King—of a truncated bivariate normal distribution for the coefficients—is an unreasonable one (though it is not entirely innocuous). But readers may be misled into believing that King has somehow relaxed the assumptions of ecological regression. That is not true.

The assumption that the coefficients are independent of the regressors is the critical problem in ecological inference. Robinson illustrated the problem with a striking example: In the 1910 Census, states with high literacy rates tended to have high proportions of foreign-born persons, though literacy rates were higher for native than foreign-born persons. I found it odd that King did not test his method on the most famous example of the ecological fallacy (though he uses another empirical example from Robinson's paper). The only explanation he gives is in a cryptic footnote: "My brief analysis of some of Robinson's smaller original data sets clearly indicates the lack of information with which to make ecological inferences" (p. 241).

King does offer two arguments about the effects of aggregation bias on his procedure, but I did not find either to be fully persuasive. On the one hand, he says that the basic model can be extended to allow the coefficients to be correlated with the regressors (pp. 174ff). On the other hand, he argues in chapter 11 that his basic estimation procedure (which assumes no correlation) is somehow "robust" to violation of the assumption. It is difficult to reconcile these two claims: If the estimates are insensitive to an assumption of no correlation, then evidently the data cannot be very informative about the degree of correlation. (It turns out that the identification of King's extended model hangs by a thread

on the particular distributional assumption that he makes. A random coefficient regression model with untruncated normal errors, for example, is not identified.)

The argument that King's method is relatively insensitive to common forms of aggregation bias is harder to assess. In Part IV, King applies his method to four datasets for which microdata are available so that the accuracy of his estimates can be judged. Ansolabehere and I used the same strategy in an earlier paper; it does not prove anything, since these are just examples, but I think it provides insight into how the method performs in practice. The results King presents are encouraging. I would, however, have preferred a more systematic reporting of the results. In chapter 10, for example, King estimates that 56% of blacks were registered in 275 southern counties, when the actual proportion is 62%. (These are unweighted averages, which are not usually the "parameters of interest." King displays the posterior distributions of B^b but does not report its posterior mean.) Is this a success or a failure? King does not provide either the Goodman estimates or the aggregate bounds, but I would guess (from inspection of figures 10.3 and 10.4) that his estimates improve slightly over the other methods. Furthermore, King's method will never go completely astray, as conventional ecological regression sometimes does (with negative probability estimates).

Although there is much to like in this book, it is not without its faults. The book is sometimes verbose and disorganized. The entire proof of Appendix C, for instance, could be reduced to a single line: Equation C.1 can be obtained by dividing the last expression on p. 307 by the top expression on p. 308. The derivation of the likelihood function, which could be done in a page or two, is spread out over pages 102–11, 132–8, and 307–8. The notation is a weird mixture of Fraktur, circumflexes, and just about anything else available in the LaTeX fonts. In other places, the technical level is not what it should be. For instance, in section 9.3.2 and Appendix E, King provides a sketchy account of a nonparametric estimation technique. This is a difficult problem, and King does not attempt to justify what he proposes. In any event, what King describes will not work, since equation E.1 does not integrate to one.

None of these criticisms should detract from King's very real contribution. In most circumstances in which conventional ecological regression is appropriate, King's alternative should provide more precise estimates. Is this book an important contribution to the literature on ecological inference? Certainly. Does it solve the problem? Alas, no.

Overcoming Tradition and Modernity: The Search for Islamic Authenticity. By Robert D. Lee. Boulder, CO: Westview Press, 1997. 216p. $62.00 cloth, $18.00 paper.

Roxanne L. Euben, *Wellesley College*

Against the assumption that "only the West has produced political theory," (M. Henningsen, "The New Politics of History," in *The Philosophy of Order,* ed. Peter J. Opitz and Gregor Sebba, 1981; Henningsen invokes this position to criticize it.), Robert Lee raises crucial questions about the nature and extent of common political and philosophical dilemmas across cultures. He does so by tracing the contours of the modern Islamic search for authenticity against the backdrop of analogous pursuits by Western political theorists from Rousseau to Charles Taylor. The book serves as a timely reminder that those who dismiss the pursuit of authenticity as the machinations of wild-eyed Islamists fail to recognize the ways in which "authenticity" is a familiar trope in modern and contemporary Western political thought, and that we need to attend to the nuances and complexity of Islamic attempts to theorize about authenticity. At times the analysis founders in its attempt to push loose parallels among radically different critics of modernity into a transcultural search for authenticity defined not just by common dilemmas but also by consistent themes and "analogous solutions" (p. 192). Yet, overall, this is a worthy and important foray into an underdeveloped field of cross-cultural political thought.

The author begins by identifying four crucial themes in the pursuit of authenticity as articulated by modern Western theorists: a concern for individual and cultural particularity, a radicalism defined by a struggle against both tradition and modernity, an emphasis on human autonomy in the creation of identity and history, and the "unicity" of human experience. The bulk of the book, however, is devoted to an analysis of these themes in terms of four radically different Islamic thinkers of the late 19th and 20th century: Indian "poet-philosopher" Muhammad Iqbal, who sought both to draw upon and transcend Islamic and European ideas in the name of "the true Islam"; Egyptian Islamic "fundamentalist" thinker Sayyid Qutb; Iranian 'Ali Shariati's Marxist-inspired Islamic thought; and Algerian Berber Muhammad Arkoun, professor emeritus of Islamic thought at the Sorbonne, who views the search for authenticity as the attempt to be "at home in the world" along the lines of 19th-century German historicism.

While some may regard the choice to analyze Islamic thinkers in terms of themes derived from Western debates as problematic, the author is right to insist that advocates of Islamic authenticity must be understood, at least in part, in terms of their profound engagement with Western debates. Indeed, the book generally avoids many pitfalls common to the difficult task of cross-cultural analysis. For example, Lee sidesteps the all too common temptation to essentialize what is often called "the Islamic impulse," on the one hand, or assume an undifferentiated, homogeneous "West," on the other. By contrast, he provides admirably clear and nuanced exegeses of significant Islamic thinkers, while allowing "the West" to dissolve properly into a low cacophony of arguments and anxieties that nevertheless reveal common preoccupations with the darker side of modernity. Lee strikes just the right balance between striving to understand what he calls "authentic thought" on its own terms and practicing immanent critique. His sympathies clearly lie with the sophisticated work of Arkoun, but he persuasively argues that the logic of authenticity is unconvincing even if judged in terms of its own objectives.

Framed by these arguments, the book's central ambition is to both explain and widen our understanding of the pursuit of authenticity in modern and contemporary life. While an admirable endeavor, in the course of this analysis, the search for authenticity is made to signify too much and thus, in the end, too little: It is alternately a symptom, a solution, a trope, a method, an epistemology, a strategy, and a common perspective on modernity. That is, it is a symptom of the confusion about values and identities in the modern world; it reflects the failures of liberalism, developmentalism, modernism, modernization, and the neglect of culture; it signifies a struggle against the West, universalistic reason, relativism, subjectivism, tradition/convention, essentialism, and the existing social order; it provides a method for reconstituting personal and cultural identity, epistemological and political foundations; it provides a solution to modern meaninglessness; and it represents a standard whose viability is measured by the extent to which it offers Muslims persuasive ways to cope with modernity (Qutb gets the most votes here).

While explicitly dedicated to illuminating the search for authenticity, the book thus raises another, unintended question: Given this extraordinarily broad definition, what in contemporary political thought does *not* qualify in one way or another as part of the quest for authenticity? The author carefully acknowledges differences among versions of "authentic thought," yet it is difficult to determine what the content of such thought could really be when Qutb and Arkoun, Nietzsche and Taylor are all identified as key expositors. The book is on solid ground when portraying these various theorists as critics united in the extent to which they are responding to and engaged with common dilemmas posed by modernity. But it strains to recast these widely divergent solutions, aims, and commitments as part of a transcultural search for authenticity characterized by similar themes and parallel prescriptions.

Despite the argument that autonomy is a dominant theme common to all these instances of "authentic thought," Islamists such as Qutb, for example, reject the logic of individualism, repudiate the belief in humans as self-creating, and condemn those who arrogantly view the world and history as a product of human will. While Lee is right to emphasize the importance of praxis to Qutb's thought, it is also the case that in this way, much of Islamic thought entails a critique of certain components of the Western idea of authenticity itself. Another case in point: Qutb argues that the antidote to hubristic insistence on human mastery of nature and human nature is an insistence that only God knows the truths necessary for living and living well. By contrast, Taylor is concerned with recapturing the ethic of authenticity prior to what he calls the massive subjectivist turn in recent history; he argues that this ethic was originally actuated by a laudable concern for increased individual responsibility and the insistence, against utilitarian theory, that morality had a voice within. Taylor explicitly sets his project in opposition to those in the Nietzschean tradition who posit a linkage of self-discovery and artistic creation against the constraints of morality, thereby exalting the notion of an agent with radical freedom and untrammeled power to create value.

The insistence that these thinkers are united not only in a critical engagement with modernity but also in a common attempt to reconstruct eroded foundations suggests that this search for authenticity may be but the latest expression of that paradoxical attempt to at once revive and reinvent "the real and true foundations," an attempt common to narratives of historical and political decline throughout the ages. This is in tension with the central premise of the book, that this search for authenticity is constitutively modern or, as the author puts it, "carries no meaning except in a context in which modernity has eroded cultures, values and identities" (p. 191). Moreover, in his attempt to unite them, Lee goes a step farther; he concludes that as the Islamic search for authenticity is less about Islam per se than a reflection of and response to the general "dilemmas of modernity," (p. 191) these thinkers' arguments "are not, in the end, religious arguments" (p. 193). This is a surprising conclusion for a book on Islamic authenticity. While it may be a viable reading of Arkoun, for example, it is substantially less true of the strain of Islamist thought represented by Qutb, where the rejection of Enlightenment rationalism and insistence on the complete sovereignty of God are mutually constitutive.

That said, this is an all too rare exercise in cross-cultural political thought, impressive in both its breadth of knowledge and aspirations. It may raise more questions than it answers, but the questions themselves are essential to the study of Islam and political theory alike.

The Principles of Representative Government. By Bernard Manin. Cambridge: Cambridge University Press, 1997. 243p. $54.95 cloth, $16.95 paper.

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This book provides a conceptual and historical overview of the development of representative democracy in the western world. There is no single argument but rather a series of observations regarding various dilemmas of this trajectory—broadly speaking, from the "direct" democracy of ancient Greece to the representative institutions of the present day.

Particular attention is given to a method of selecting political leaders that has heretofore drawn little remark from academics: selection by lot. Chapter 1 describes the use of this method in Athenian government; succeeding chapters show how it remained common among the city-states of the Italian Renaissance and how it occasionally entered into the ruminations of later English and French political theorists (e.g., Harrington, Montesquieu, and Rousseau). The plot structure of the narrative is one of decline, for by the late eighteenth century the lot seems to have been entirely abandoned as a device for leadership selection (with the notable exception of juries). "Never was it seriously considered during the American and French revolutions" (p. 79).

Manin invests the abandonment of this device with a great deal of theoretical and historical weight. The move from selection by lot to selection by election is considered to have marked the critical move from "direct" to "indirect" methods of government—the birth of representative government. Indeed, it is plausible to regard selection by lot as a way-station between rule directly by the people (collectively assembled) and rule by the people's (elected) representatives. With a system of selection by lot, the people evidently rule themselves *in turn.*

Most readers (myself included) would be tempted to conclude that the disappearance of this principle of selection can be explained by the increasing size of political units (from the city-state to the nation-state) and electorates (through the extension of suffrage). Manin, however, argues that "lot was not totally impracticable" (p. 82); thus, he takes the position that its disappearance cannot be explained with functional arguments.

There is, of course, a larger agenda on the table. Manin seems discontent with that element of representative democracy which decrees representatives be different from those they represent. The thesis—sometimes overt, sometimes covert—of this book might be found in Robert Michels's dictum that "the substance of democracy is permeated with aristocratic elements" (*Political Parties: A Sociological Study of the Oligarchical Tendencies of Modern Democracy,* 1962, p. 50). What actually distinguishes representatives from the represented? Manin concludes only that there will be *some* difference—be it greater virtue, competence, or whatever—between the person chosen to represent a constituency and the electors themselves. More than that, he will not say.

The final chapter departs somewhat from the themes of the earlier chapters. Here, Manin offers a historical typology of representative democracy, which he divides into three ideal-types: parliamentarianism, party democracy, and audience democracy. While I have some qualms about the defining attributes, I do think the typology serves admirably as a shorthand description of developments in the Anglo-European democracies over the past several hundred years. That is no mean feat. In the age of parliamentarianism, as the term implies, parliaments were the operative unit of government. This was the golden age of parliamentary oratory, "independent" MPs, and bitter factional disputes. The age, and its

style of government, is exemplified in the English case by Walpole, Pitt, Fox, Peel, and Disraeli and in the American case by such figures as Madison, Webster, Clay, and J. Q. Adams. By the end of the nineteenth century in England (and the mid-nineteenth century in America), parties had replaced parliamentarians as the principal focus of power and authority, inaugurating a new epoch in representation. The third representative form, audience democracy, can be seen in the postwar era, as parties cede power to opinion polls, political consultants, independent (nonpartisan) media outlets, and strong leaders. (Another term for this stage of democratic representation would be "plebiscitarian.") Doubtless, each country has experienced slightly different incarnations of this three-part historical schema. In the United States, for example, one might argue that the age of party democracy did not evolve to the same extent as it did in parliamentary democracies (due largely to the existence of a directly elected president). Nonetheless, this simple typology (which I have presented in somewhat different terms than Manin's account would indicate) does an admirable job of ordering the main developments of Western democracy over several centuries.

Despite a stimulating attempt to compress and periodize political theory and practice, this book suffers from several difficulties. First, the thesis could be more finely tuned, more clearly stated, and more rigorously followed. Second, there are some dubious claims. (Could selection by lot be used to fill high political and administrative positions in the modern nation-state?) Third, there are some odd normative concerns. (Why, for example, does the "aristocratic" element of representation aggravate Manin, when he understands this element as constituting *any* difference between leaders and masses?) Fourth, the book could do a better job of integrating secondary literature on the various topics addressed. (Michels, the intellectual godfather of the book, is nowhere cited; Pitkin's classic treatise on representation—*The Concept of Representation,* 1967—is relegated to a single footnote on p. 6; and virtually no empirical work on constituent-representative ties is referenced.) Finally, the key terms employed in the study—aristocracy, representation, democracy—are often used in odd and/or ambiguous ways. "Aristocratic," for example, would seem to imply more than *any difference between representatives and the people.* "Representative regimes," to take a second example, are defined as having regular election, decision-making autonomy from the electorate, freedom of expression, and open debate (p. 6). It would seem that ancient Greek city-states possessed all but the second. Does this mean that Athens was, in fact, a *representative* system of government? It is difficult to say.

Carl Schmitt's Critique of Liberalism: Against Politics as Technology. By John P. McCormick. Cambridge: Cambridge University Press, 1997. 352p. $39.95.

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We have recently witnessed a striking rise of interest in the political and legal philosophy of erstwhile Nazi Carl Schmitt. Much of this has focused on Schmitt's friend/enemy criterion of the political as found in *The Concept of the Political.* It is both a strength and weakness of McCormick's fine book that it does not follow this familiar path. Its main claim is that Schmitt's resistance to a liberal politics of individual rights and constitutional guarantees of a limited government can be properly understood only in the context of a broader critique of Weberian rationalization. As McCormick demonstrates, for Schmitt, as for Heidegger, bureaucratic liberalism, legal positivism, the political primacy of the economic, and machine technology itself are all epiphenomena of instrumental reason, or modern metaphysical "technicity." Hence, such often discussed works as *The Concept of the Political* and *Political Theology* are misunderstood if they are considered in isolation from a careful reading of more obscure works, such as *Political Romanticism, Roman Catholicism and Political Form,* "The Age of Neutralizations and Depoliticizations," and *Theodor Daubler's "Northern Lights".* It is in these that Schmitt most clearly delineates what technicity entails and what it opposes. And, McCormick argues, both Schmitt's theoretical and political stances are "extensions, even applications" (p. 4) of this analysis. McCormick does not limit himself to an exposition of Schmitt's thought, but neither does he want to criticize it from a perspective alien to Schmitt's own. Accordingly, his argument is not that Schmitt's political theory fails because it does not ground politics on principles of justice or individual autonomy, à la Rawls or Mill. Instead, he seeks, initially at least, to present an immanent critique of Schmitt by "reading him against himself" (p. 7; but cf. p. 300). This strategy entails demonstrating that Schmitt falls prey to the very sins he denounces in others. McCormick argues quite rightly that only this approach can hope to address adequately the temptations and dangers of totalitarianism.

Schmitt's entanglement with totalitarianism begins with his reaction to Weber. Here we find both poles of the binary which, according to McCormick, structured Weimar even as it structures our own time: the one between procedural neo-Kantianism and neo-Nietzschean Lebensphilosophie (p. 9). As becomes clear in "Politics as a Vocation," Weber stubbornly valued a model of scientific reason that was itself value neutral. This seeming paradox is hardly unique to him. Numerous theorists have pointed out that this model of reason as a means without a given end *requires* the supplement of the very values it denies. But since these values by definition elude rational evaluation, this requirement inevitably places reason in the service of the irrational. As McCormick deftly shows, Schmitt's initial response to this was to break with Weber's heroic or romantic nihilism and to demand a Hegelian mediation of the intellectual poles of modernity. Schmitt was not able to produce such a sublation, however. Instead, in McCormick's account, he fell back into the duality he began opposing and simply privileged a mythic form of one of its two poles, that of the irrational will.

While this reactive collapse is the guiding thread of McCormick's book as a whole (p. 298), the most focused analysis of it is found in the second chapter, "Myth as Antidote to the 'Age of Neutralizations': Nietzsche and Cultural Conflict as Response to Technology." Drawing upon Horkheimer and discussions of myth by Adorno and Cassirer, McCormick argues that Schmitt turns to myth in an attempt to master his fears by naming them. The name that Schmitt purportedly gives to technicity is "Russia." Rather than perform the hard conceptual labor of sublating Weber's binary of reason and value, Schmitt allegedly projects one of its elements outward, onto a more manageable enemy, one that may be confronted and destroyed. This confrontation, however, will entail the reappropriation of the very technology that was projected onto the Russians, for only with its aid can they be defeated. Moreover, as the conflict is a substitute for dialectics, it reduces Schmitt's own position to an irrational one. Schmitt thus falls prey to the same dialectic of enlightenment he attacks so insightfully in *Political Romanticism.*

One weakness of McCormick's analysis here is its reliance upon comparisons with Nietzsche to demonstrate points about Schmitt. McCormick claims (pp. 84–5) a more direct

link between Nietzsche and Schmitt than most of Schmitt's readers have observed. But it remains unclear what that link may be. There is certainly no claim made of a causal influence. Instead, the reader is presented with a list of superficially similar passages from the two authors, after which McCormick concludes that their use of the language of the Antichrist encourages a latent irrationality in their thought (p. 115). Expressed this baldly, and concerning two writers who are notoriously difficult to interpret, this seems to put too much emphasis upon (the surface meaning of) their rhetoric. The comparison with Nietzsche also leads McCormick to downplay the differences between Schmitt's writings before and after his break with the Catholic Church. This is ironic, as in a later chapter on representation McCormick does a first-rate job of delineating how Schmitt understood his Catholicism in a Hegelian light, as offering a more adequate relation between form and matter, and hence between reason and will. When this is taken into account, it no longer seems that myth plays quite the role that McCormick claims, at least not in Schmitt's early work. If the Russians are Europe's proper enemy because they embody technicity (and not, as McCormick himself notes on p. 98, merely the procedural side of this binary), and if Catholicism alone adequately resists technicity, then Europe must first convert to Catholicism and then confront Russia. The threat of war with Russia may (seek to) inspire an hysterical conversion; but the conflict itself will be between two substantively different ways of life. *Pace* McCormick, it will not be "aesthetic."

But what of Schmitt's use of the myth of a demonic Russia when he is no longer in the church? McCormick declares that Schmitt's "attempt to infuse the technologically disenchanted world with meaning through 'the concept of the political'... is Schmitt's own succumbing to the obfuscating dichotomies of the technological modernity he criticizes" (pp. 18–19). But what *is* the concept of the political? More than once McCormick asserts, in a fairly off-handed way, that "the political" for Schmitt is "the transhistorically legitimated human propensity toward violent existential conflict" (pp. 17, 96, 110). This suggests that Schmitt deploys the myth of the Russians in order to foster conflict, the arrival of which—though inevitable—should be hastened, as it will somehow bring a meaning to modern European life (pp. 73, 92, 232). "The political, the postulation of an enemy... serves to distract from the discomfort... of not knowing who oneself [or] one's culture... is in modernity" (p. 233).

Unfortunately, this familiar and even standard understanding of Schmitt's most important category is never demonstrated through a sustained reading of *The Concept of the Political.* The closest McCormick comes to this is in his final two chapters. Here, again, he emphasizes the role played by fear in Schmitt's thought. McCormick argues that Schmitt's friend/enemy criterion is simply an appropriation of Hobbes's state of war: In other words, it is a mythic inversion of the normal and exceptional states, one intended to compel obedience through fear (pp. 14, 132, 247, 250–5). The problem with this claim is that in Schmitt's account it is unclear for what the citizens fear. In Hobbes it is for their physical life. Since this is the basis of political authority, Hobbes cannot sanction the sovereign's demand that citizens sacrifice their life. But Schmitt has no such hesitations: While he embraces the general principle of *protego ergo obligo,* he also insists that the state has the right to demand from its members the readiness to die.

McCormick acknowledges this dilemma (p. 257), only to turn immediately to a discussion of Strauss's reading of Schmitt, which he claims focuses on this problem (p. 262). Strauss argues that, in the end, Schmitt remains within the purview of liberalism. McCormick seems to concur and argues that, because the state transcends the differences and conflicts that animate its subjects, it therefore reinstates the modern "neutrality" that Schmitt explicitly opposes as being antipolitical: Schmitt "defines his 'political' as beyond objective normative standards, by defining it as if it were neutral" (p. 263). But it is unclear why there is any necessary connection between the lack of norms and liberal neutrality. Moreover, Schmitt himself does not emphasize the neutrality of the state. Instead, as McCormick notes, he is concerned with the identification of the citizenry with the state or the constitution that gives the state life (pp. 232, 245). I can only suggest that the relationship between authority and identify lies in the fact that politics offers a form of identity which transcends the physical body and hence can sanction its sacrifice. This may strike many as repugnant, but it is not incoherent, nor does it require the prior positing of an enemy, mythical or otherwise. If this is correct, then Rousseau is a better source than Hobbes for clues to understanding Schmitt's existential politics.

This is not, however, to deny the considerable accomplishments of McCormick's book. Fear and myth may not be the keys to a complete understanding of Schmitt, but they are certainly of real significance. Similarly, whatever the status of its guiding argument, this extremely well-researched work is filled with nuanced and intelligent discussions of Schmitt's legal philosophy, his changing attitude toward commissarial and sovereign dictatorship, his relationship to Lukacs, and a host of other topics. All serious students of Schmitt, Weber, the Frankfurt School, neo-Hegelianism, political representation, and the politics of technology will want to read it.

The Morality of Nationalism. Edited by Robert McKim and Jeff McMahan. Oxford: Oxford University Press, 1997. 371p. $45.00.

Glyn Morgan, *Harvard University*

Nationalism in one form or another constitutes the most potent and ubiquitous political force of the late twentieth century. The nation-state—the parent and child of so many nationalist movements—remains, despite the challenge of an increasingly integrated global economy, the dominant form of political membership of our age. And yet political philosophers have barely discussed either nationalism or the nation-state. To many philosophers, nationalism is nothing but an incoherent expression of group egoism, a nonrational sentiment unworthy of philosophical scrutiny. Fortunately, some political philosophers have adopted more fruitful conceptions of nationalism and political philosophy alike. Thus, in recent years Will Kymlicka, David Miller, and Yael Tamir (among others) have produced sophisticated analyses of the moral dimensions of our attachment to ethnoculturally defined groups and nations. The McMahan and McKim volume on the morality of nationalism forms a welcome and important addition to this literature.

Unlike many edited volumes arising from an earlier conference, the contributions to this book are uniformly first-rate. Rarer still, the authors speak to rather than past one another. The highlights include Jonathan Glover and Charles Taylor on the appeal of nationalism; Jeff McMahan and Thomas Hurka on the moral permissibility of favoring conationals; Michael Walzer and Robert McKim on toleration and nationalist conflicts; Samuel Scheffler and Arthur Ripstein on liberal nationalism; and David Copp and Alan Buchanan on national self-determination.

Perhaps the most valuable feature of this book is its attention to the psychological appeal of nationalism. Charles Taylor notes that this has been neglected by the modernization theories that dominate the field. Like Isaiah Berlin (whose theory of nationalism is discussed in this volume by Avishai Margalit), Taylor seeks to explain the emotive force of nationalism through a story of the injuries to dignity that cultural minorities experience in the face of superior, conquering powers. The only drawback with this argument is that the "refusal of incorporation" (p. 47) constitutes a species of nationalism rather than its genus. The members of dominant nations are often every bit as attached to their distinctive culture and traditions as are national minorities (as Walter Feinberg notes in his insightful essay). How are we to explain the attachment of the French, the Germans, and the Japanese to their respective nations?

In his response to Taylor's essay, Will Kymlicka suggests that the desire to maintain our language and culture is natural and ubiquitous. For Kymlicka, Taylor's invocation of dignity is otiose. But both can be criticized for confusing a species of nationalism with its genus. Nationalist movements sometimes develop among peoples who lack a sense of cultural distinctiveness antecedent to their struggles with their neighbors. Taylor is surely correct to think that we need an explanation of the "will to difference" that grips so many people in the modern age. The problem with his concept of "dignity" lies, I think, elsewhere. The term is morally loaded. While we may legitimately employ it to explain the nationalist struggles of an oppressed minority like the Kurds, we would be rightly reluctant to see "dignity" at stake in the struggles of Serbs and Croats for their own ethnically pure states. Here we arrive at a thorny problem that confronts everyone who sympathizes with at least some forms of nationalism: the problem of distinguishing morally permissible from morally prohibited species of the phenomenon.

This problem is pursued in a section of the book devoted to nationalism and the moral demand of impartiality. The moral injunction to treat all individuals as equals appears to stand in obvious tension with the nationalists' belief that we have some special relationship of solidarity with our conationals. To some philosophers, partiality toward them is no more justifiable than partiality to the fellow members of one's race. Thomas Hurka seeks to rebut this view by defending national solidarity in terms of the nation's history of "reciprocal benefits" and "shared suffering" (p. 151). Hurka's interesting argument permits a distinction between the moral status of "black and aboriginal movements," on the one hand, and "white supremacy movements," on the other (p. 152). Unfortunately, Hurka shirks the more difficult task of locating specific nations along the spectrum suggested by this dichotomy. Moreover, it is far from clear why our obligations to particular groups with histories of "reciprocal benefit" should be any greater than those we owe to the poor and needy with whom we lack such a history. To the poor and needy themselves, the historically based special obligations of conationals in the developed world may appear as nothing more than a self-serving attempt to justify a refusal to live up to the demands of justice.

Notwithstanding the qualified sympathy that many of the authors in this volume adopt toward nationalism, many political philosophers will still harbor a suspicion that national solidarity has produced—and continues to produce—more harm than good. These critics of nationalism will welcome Arthur Ripstein's essay, which directs some penetrating criticisms against those who have sought a rapprochement between liberalism and nationalism. To Ripstein, liberalism focuses upon fairness rather than history, or culture, or group identity. Liberals more sympathetic to nationalism will likely respond that fairness alone rarely motivates in the absence of a sense of national solidarity founded upon a shared history and culture. This response raises some difficult sociological questions concerning issues about which, as Ripstein pointedly observes, "philosophers are notoriously lacking in expertise" (p. 224). Ripstein is perhaps too hard on his philosophical colleagues here, since contemporary social scientists do not themselves know much about the historical, cultural, and ideational prerequisites of mutual aid. Some national communities are generous to their least well-off members, while others are not. Clearly, a lot more thought needs to be devoted to this issue. The essays gathered in this excellent volume provide a useful place to start.

Republicanism: A Theory of Freedom and Government. By Philip Pettit. New York: Oxford University Press, 1997. 304p. $29.95.

Laura J. Scalia, *University of Houston*

For generations, scholars understood classical liberalism as including nonarbitrary governments that were relatively unobtrusive and strongly committed to individual rights and liberties. Intellectual giants such as John Locke and James Madison were considered among the most important spokesmen of this tradition. Sometime in the twentieth century, (at least American) liberalism took on alternative meanings. For some, like Frederick Hayek, it tolerated only a minimalist government. For others, like Herbert Croly, it became the tradition of economic redistribution and big government. For still others, John Stuart Mill became the role model, with liberal government becoming one that accepted any and every alternative life style. In American politics, early classical liberalism became the foundation of conservatism. In scholarship there and abroad, it appears that this early tradition is now taking a different route.

As the subtitle implies, *Republicanism* offers a theory of freedom and government. The author's aim is to reintroduce important features of what Pettit terms republicanism. He does so in the hope of convincing others that this tradition values a freedom which is superior to and more practical than other contemporary notions. With this in mind, he divides the book into two main sections, the first describing the liberty ideal and the second explaining how it would be put into practice. In the first section, Pettit offers a crisp, clear definition of republican liberty: It is freedom from domination and arbitrary interference; it is the antonym of slavery. He contrasts this with Berlin's preferred notion of liberty as freedom from interference, a negative conception that Pettit finds troubling; as he sees it, it proclaims the slave free in cases in which his master likes him and chooses not to interfere with his daily life. Pettit prefers the republican conception of liberty because it supposes that someone who experiences nonarbitrary legal interference is nonetheless free. Anyone who thinks carefully about this issue will no doubt agree that Pettit is trying to recapture an important sense of liberty, different from negative liberty and certainly ignored by libertarians and various scholars of liberty. He rightfully finds his preferred conception a practical and useful way of understanding liberty, for it allows us to think beyond freedom as the absence of impediments, that is, as theoretically antithetical to law and power. If we can agree that liberty can and should include nonarbitrary, legally bounded interferences, then we can agree that there is such a thing as good government. In other words, we can agree that well-

formed states can and do support liberty, provided, of course, that they are able to avoid becoming dominating forces.

The second part of Pettit's book describes features that help government avoid its miserable tendency to dominate and become arbitrary. Here, the author discusses the kind of institutional mechanisms and citizen predispositions which would allow his beloved republican freedom to prosper. To put it briefly, Pettit believes liberty has the potential to thrive in governments that follow established laws, disperse and limit the power of the majority and their elected officials, allow for open contestation and unimpeded political debate, and are supported by a vigilant citizenry. For him, that means establishing a constitutional form of government much like the one in the United States today, albeit with some interesting new twists. For example, he argues that ongoing contestability should be more highly regarded than popular consent. He seeks to extend into the political realm the economic system of rewards and the jury system of screening. Finally, he depicts a vigilant citizenry as one in which individuals are civil to rival interest groups but nonetheless willing to manifest disapproval when necessary for liberty's sake.

Pettit claims that he has put forward a "challenging conception of the ideal" (p. 11). He has. Careful readers will find many interesting and novel ideas in the book, but many will have difficulty wading through the text and finding the great insights. This will especially be the case for readers who are sticklers for good grammar. In an attempt to avoid gender-specific pronouns, Pettit repeatedly matches singular nouns with plural pronouns. (For example, a three-page subsection of chapter 3 includes ten phrases such as: "The dominated . . . person is someone with reason to watch what they say," p. 91). What a pity that the author and Oxford University Press did not seek an alternative gender-neutral strategy, for this bizarre style certainly distracted me.

Those who have steadfast views about what ideas or persons rightfully belong within the liberal or republican tradition also will have difficulty extracting the true pearls in the argument. Pettit says that he recovered his "distinctive" republican ideal from many historical authors, over "very different periods and very different background philosophies" (pp. 11–12). Yet, he never really distinguishes his republican vision of liberty and its accompanying government mechanisms and citizen predilections from the liberal conception of liberty, government, and good citizenship. Nor does he explain why he omits features traditionally associated with the republican tradition. For example, he uses authors such as John Locke (pp. 40, 62, 100, 225, 266), James Madison (pp. 212, 221), Alexander Hamilton (pp. 100–1) and John Stuart Mill (pp. 62, 139) as authoritative spokesmen of his republican ideal. He implies that liberals accept slavery (p. 22) and that republicans do not (pp. 31–5). Historically, both kinds of regimes have tolerated slavery, but only the latter used its own language to justify it (e.g., Aristotle). Moreover, Pettit describes republican virtue as citizen vigilance (chapter 8). He associates that tradition with equality and tolerance (chapter 4). Though there may be no a priori reason to reject the possibility of such connections, clearly, liberalism has long been the language of equality, tolerance of ideas, and jealous virtue, whereas republicanism has endorsed hierarchy; homogeneity, and noble self-sacrifice.

Perhaps the author would not be seriously troubled by these criticisms. He finds the use of masculine pronouns sexist (p. 71). Moreover, he admits he is no "historian of ideas" (p. viii) and that "if historians of ideas find [his historiography] misleading, they should regard the more substantive historical suggestions as simplifications that are justified only by the color they give to [his own] philosophical claims" (p. 11). Thus, for those who can easily accept these premises, the book's compelling features are easier to find. Many readers will find themselves in my position, however. They, too, will applaud Pettit's attempt to redepict liberty as freedom from domination and arbitrary government, and they will agree with him that the best means to achieve such liberty is to create a limited constitutional government and encourage civility and open debate among citizens and leaders. Yet, they will find the book unsatisfying as they wonder why he insists on calling his entire opus of ideas distinctive and republican instead of what it is: a twentieth-century take on the admirable features of classical liberalism and watered down republicanism.

In the end, however, there may be some merit in Pettit's renaming technique. Scholars now have trouble deciding who or what ideas deserve the liberal label. Perhaps the ideals we truly value would gain from a new and distinctive title. Nevertheless, such a move should be made explicit and deserves explanation. Else we will be left in even more confusion, unaware why early liberalism has become republicanism, and why republicanism has lost its earlier affinities to necessary exclusions, self-sacrificing, noble virtue, and homogeneous participatory government. Most important, we will end up masking the real issue which concerns Pettit and many scholars today: Is there a sense of liberty that is both worthy of human dignity and politically viable? On this point, Pettit has much to say and ought to be taken seriously.

Democracy, Bureaucracy, and Character: Founding Thought. By William D. Richardson. Lawrence: University Press of Kansas, 1997. 202p. $29.95 cloth, $16.95 paper.

Brian J. Cook, *Clark University*

In *Democracy, Bureaucracy, and Character*, William Richardson has successfully packed considerable perceptiveness and stimulating analysis within a short compass. Indeed, despite the slenderness of this volume, it is somewhat overpacked, with the same essential arguments showing up several times in slightly different form.

Like a host of other books published in the past decade on the theoretical and normative foundations of American public administration, this one begins with a litany of current attacks on the American administrative state. Beyond the superficial and immediate discontent, however, the author finds several sources of profound challenge to the administrative state. He then enumerates and briefly describes four such challenges and sources of tension: "Rational-legal bureaucracy and democracy are at once inextricably linked and antagonistic" (p. 10); "changes in the American regime and the functions assigned to the state" (p. 11); "severe limits on the degree to which American public administration is able to satisfy the demands placed upon it" (p. 13); and "the essentially elitist or hierarchical nature of the rationale constructed by administrative theorists to legitimate the power and authority of professional managers in both the public and private sectors" (p. 13).

Brevity and succinctness, which are among the book's principal strengths, are also its weakness in this early discussion. All four challenges, although handled deftly, deserve further elaboration, especially the "administrative technologies" and Tocqueville's "equality of conditions," which the author identifies as being among the forces limiting administrative performance. Nevertheless, the discussion enables Richardson to arrive at a clear statement of his aims for the book: "to bridge the admittedly contradictory elements of

American public administration and the regime it is intended to serve by examining the complex interaction of several elements" (p. 15). The "binding thread" for these elements is the author's effort to articulate "a proper understanding of and appreciation for the American character." Richardson then proceeds in the second chapter to describe and explain the regime foundations for American character traits. He follows this with five chapters that represent elaborations on his understanding, focused respectively on formal and informal correctives that complement character, the role of honor and reputation, the education of public officials, the relationship between public policy and character, and administrative ethics.

Richardson's description of the essential elements of the design of the American regime, as expressed in the Constitution and the *Federalist Papers*, is clear, concise, and consistent with other accounts I regard as authoritative. Although quite familiar with this material, particularly the commercial nature of the regime and the expectations for and dependence on individualism, acquisitiveness, and self-interest in its operation, I learned a great deal from Richardson's treatment of it. Again, the character-forming aspects of the design are the author's chief concern, and he concludes his initial presentation of founding thought with a bold statement of what is the core message of the book: Legislative, judicial, and executive officials are duty bound to pursue public policies and administrative arrangements that take account of and reinforce the character traits on which the regime is based. Thus, public administration has "a character-forming responsibility" to the regime (p. 31).

In turning his description of regime fundamentals into a tool for analyzing American administrative thought, Richardson argues that two fundamental questions arise, but they have often been ignored by administrative theorists: "What is the proper place for public administration in the governance of the regime? What outlook and attributes of character are required of these individuals?" (p. 39). Richardson is clearly a careful and astute reader of both Tocqueville and Woodrow Wilson. I found it surprising, therefore, that in this discussion, and its extension to matters of public honor and reputation, he fails to explore Tocqueville's treatment of democratic honor from *Democracy in America*. With respect to Wilson, Richardson relies much too heavily on the 1887 essay, "The Study of Administration," as the definitive expression of Wilson's views. That essay does provide strong signals about themes in Wilson's political thought that endured throughout his academic and political careers. One must look at Wilson's work in its totality, however, including published and unpublished papers, public addresses, university lectures, and political speeches. Such a look reveals that Wilson addressed directly and extensively the two questions Richardson raises, including the matter of proper administrative character and how to promote and preserve it. A more careful and thorough treatment of Wilson would have strengthened and deepened Richardson's analysis.

The highlight of the book is the linkage of founding thought and concerns about the character and role of those who would govern with public policy design. Using his examination of founding thought, Richardson scrutinizes several New Deal policies for their performance in conserving and promoting the character traits on which a regime relies. He finds them successful in expanding the scope of public administration in the regime while remaining safely within the limits set by regime fundamentals. From this analysis, Richardson derives a short list of guidelines for policy design. The guidelines are sensibly conservative, such as promoting voluntary, self-interested citizen participation in policymaking that also encourages people to see the public interest. What makes Richardson's treatment of public policy compelling, however, is his combination of a historically oriented analysis based in regime politics with a concise and concrete expression of the essential job of the public administrator anchored in institutional design for the public interest, that is, the interest of the regime. To my mind, Richardson has succinctly defined what lies at the heart of study and practice in public policy and public administration grounded in politics rather than in economics.

The book concludes on a weak note, with the glaring mistake of identifying Abraham Lincoln's presidential successor as "Samuel" rather than Andrew Johnson! A final, brief assessment of several more recent public policies, such as the all-volunteer army, is somewhat haphazard and not very well grounded in the author's own regime analysis. Nevertheless, overall the book offers valuable argument and insight that can enrich a wide variety of courses in public administration and public policy.

Athenian Democracy: Modern Mythmakers and Ancient Theorists. By Arlene W. Saxonhouse. Notre Dame and London: Notre Dame University Press, 1996. 192p. $26.95.

Harvey C. Mansfield, *Harvard University*

We today have embraced the view that democracy is the only legitimate form of government. Political theorists have elaborated the view in a section of their field known as democratic theory, in which every question is raised and answered according to what makes for more or less democracy. Democratic theorists are mostly on the Left, to be sure, but their premise is not challenged by thinkers or politicians on the Right. No conservative argues on campaign finance, for example, that the rich as rich deserve to have more political influence than the poor. Conservatives do resist the extension of democracy, but they generally advance an allegedly democratic reason as to why democracy should be limited or, in their view, as to why democracy requires such limitation. Rich people, it is said, must not be denied the right of free speech which their money so loudly amplifies.

On the Left there is equal embarrassment, but of a different sort. Liberals say they want to extend democracy everywhere, but in fact they do not. An up-to-date liberal today is against slavery, segregation, sexism, and homophobia; but these are historical enemies in a series of battles that were fought one by one. Liberals often did not know which battle was coming next, and so they fought slavery without opposing segregation, segregation without opposing sexism, and so forth. Liberals cannot say now what monstrous injustice of inequality we are overlooking and will soon be our duty to eradicate. They have to wait anxiously to see what will be their next cause. It seems that both Right and Left look on democracy as a tremendous force we cannot control, to which we can only react. Both have stacked everything on the legitimacy of democracy alone, and yet neither can prove it to be so. We cannot live with pure democracy, and we cannot justify any impurities.

In this situation it makes sense to go back to theorists of ancient democracy, because they did not make the assumption of our democratic theorists that democracy is the only legitimate government. That is the purpose of Arlene Saxonhouse's latest book, which began as the Frank M. Covey, Jr., Lectures in Political Analysis given at Loyola University. In her study of Athenian democracy, Saxonhouse begins with modern mythmakers, some of whom (such as Hobbes and Madison) too quickly dismiss any possibility that there may

be something to be learned from it, and others of whom (like Mill and Arendt) embrace it with irrational exuberance. These authors were all affected by the simplifying modern decision in favor of democracy, even when they did not wholly adopt it. For Saxonhouse, wisdom in political analysis shows "complexity" (p. 29)—in particular that there is something to be said for other governments, in the light of which a clearer view of the virtues and vices of democracy becomes possible.

The four ancient theorists Saxonhouse discusses—Herodotus, Thucydides, Plato, and Aristotle—all hold debates on the merits of the various regimes. Modern theorists no longer do this, not even those with sympathies for undemocratic regimes. Tocqueville, for example, contrasts aristocracy in the past with democracy in the present and future, thus making clear that the debate between them is over. What, then, do we learn from these debates that is useful today?

Saxonhouse presents each ancient theorist as having a useful theme. That of Herodotus is the equality of *isonomia*, an equality made by law, not nature. It seems that for him there is no natural right on which to ground either democracy or aristocracy. Both have to be, as we would say, socially constructed. The consequence is not necessarily favorable to democracy, because equality or *isonomia* can be had, perhaps more dependably, from monarchy. Here is a point for our social constructionists to ponder. What happens when democracy tries to subsist without natural right or natural rights?

Thucydides shows the potential for deliberation in democracy, if not at first then later, as illustrated in the Mytilenian debate when the Athenians changed their minds, rescinding a hasty decision prompted by the demagogue Cleon in favor of a wiser reversal argued by Diodotus. Saxonhouse agrees with the judgment of Leo Strauss that Diodotus, not Pericles, was the best representative of Athenian democracy. But was Diodotus an actual person or an imaginary creation of Thucydides, as Saxonhouse (still in accord with Strauss) suggests? If the latter, then Athens was not merely a democracy in speech and one-man rule in deed, as Thucydides said of Pericles' domination, but one-man rule—by Thucydides—in speech also. Democratic deliberation is assisted, completed, and even replaced by wise speech. Here is a lesson for admirers of "public reason" who think that wisdom can be made to exude from democracy.

Saxonhouse tells us that Plato, no friend of democracy, nonetheless portrays the problematic gentleness of democracy. Democracy is a particular regime; hence it has a particular character, which Plato shows in *Republic* VIII to be gentleness toward the condemned. Athens was hardly nonjudgmental, as we know from the trial of Socrates, but it offered him clemency after the trial (which he refused) and toleration for seventy years before that. Democratic gentleness, we see, is apolitical; through lack of attention more than policy, it allows good men to live and bad men to rise.

"Aristotle, the exponent of a hierarchical worldview, points out the inadequacy of all attempts to put this principle of hierarchy into practice in political life" (p. 120). Let this shrewd conclusion sum up Saxonhouse's analysis of Aristotle on democracy. If a democracy is to be better than a multitude of tyrants, some hierarchical principle is necessary; but how to put it into political practice is quite unclear.

In this helpful meditation, an open-minded book, Saxonhouse wonders how the ancient theorists can be brought to the understanding and aid of modern democracy. The difficulty is that we today can only imagine a politics in which democracy is an open question; the reality for us is that, for good and for ill, democracy is a closed question. We have more to learn when the question is open and less to fear, we believe, when it is closed.

Beyond Neutrality: Perfectionism and Politics. By George Sher. Cambridge: Cambridge University Press, 1997. 251p. $54.95 cloth, $18.95 paper.

Richard Dagger, *Arizona State University*

George Sher begins this important, argument-packed book with the following confession: "I am ambivalent toward contemporary liberalism" (p. ix). On the one hand, "many of today's liberal thinkers are waging a necessary and courageous battle on behalf of certain vital but embattled Enlightenment attitudes," the most important of which is "a confidence in the power of reason to resolve our disagreements. Without such confidence, we are at the mercy of the postmodernist tendency to reduce all political and intellectual disagreement to so much jockeying for power and advantage" (p. ix). On the other hand, Sher is troubled by a "deeply problematic" turn in contemporary liberal thought: "Though still confident about our ability to reach universally applicable conclusions about justice and rightness, these [liberal] thinkers are much less sanguine about the prospects for reaching reasoned conclusions about goodness or value" (p. ix).

Sher thus enters a debate in which the terms are familiar, if not always clear. On one side are those "neutralists" who argue that "the right" is prior to "the good"; on the other are various "communitarians" who insist that "the good" is prior to "the right" as well as "perfectionists" who hold that the only credible form of liberalism is one that aims not for neutrality but for promotion of distinctively liberal virtues. Sher takes his stand with the latter groups, although he also takes pain to distinguish his position from theirs. The liberal perfectionists, among whom Sher includes William Galston, Stephen Macedo, and Joseph Raz, have provided a merely internal criticism of liberal neutralism: "Instead of defending their value-claims, they are often content to remind us that theirs *are* the values of liberalism, and that most neutralists themselves accept those values" (p. 152). To demonstrate that liberals are committed to certain values and necessarily promote certain virtues is "no substitute for a full positive argument" (p. 152) that establishes the worth of these liberal values and virtues. For their part, the communitarians—Charles Taylor, Alisdair MacIntyre, and Michael Sandel—have tried to provide such a positive argument by tracing "the superiority of certain ways of life either directly to the relevant society's history, culture, traditions, and narratives, or else to the connection between these social factors and the identities of individuals" (p. 152). But they have failed to provide a clear and coherent account of "*what* our traditions, narratives, and the like require of us" (p. 152). Sher thus aims for a version of liberal perfectionism that will supply the deficiencies of these other criticisms of liberal neutralism.

Before advancing his "full positive argument," however, Sher devotes five chapters to a careful scrutiny of the neutralists' arguments. Chapter 2 is especially valuable for Sher's analysis of the phrase "conception of the good," which leads him to conclude that the idea is far from straightforward. On the contrary, there seem to be three overlapping intuitions at work, each one leading to a different justification for neutralism. The first leads to various attempts to justify liberal neutralism by arguing that only a state neutral with regard to conceptions of the good is consistent with personal autonomy. The second advances the case for "prophylactic neutrality" with the argument that "the modern state's power is so vast, and those who wield it so flawed, that no govern-

ment can be *trusted* to promote its citizens' good" (p. 106). The third takes the skeptical position that states should remain neutral with regard to conceptions of the good because there is no way to know what truly is good. Sher's arguments against the neutralists who defend these positions—principally John Rawls, Ronald Dworkin, Charles Larmore, and Bruce Ackerman—are too involved and too numerous to summarize here. But it should be noted that Sher is an admirable critic who sets out his opponents' arguments clearly and fairly before lodging powerful objections against them.

Sher's own version of liberal perfectionism, developed in the last four chapters, is both "pluralistic" and "monistic." It is "pluralistic in the sense that it attaches value to a number of irreducibly different activities, traits, and types of relationship" (p. 199). What makes these valuable—and Sher's perfectionism monistic—is that they all grow out of fundamental human capacities "whose exercise is both near-universal and near-inescapable" (p. 202). That is, every person has reason to value "knowledge, rational activity, and close personal relations" (p. 207). Further argument leads to the addition of "moral goodness, the appreciation of true beauty, and the development of one's abilities" (p. 207) and eventually "decency and good taste" (p. 212) to the list of qualities that everyone should value and the state should foster.

Two features of this list are especially noteworthy. The first is that these qualities may seem universally desirable simply because they are so vague. Everyone may agree that knowledge is good, for instance, yet disagree about what kinds of knowledge are worth having. But complaints of this sort will not trouble Sher, whose open-ended brand of perfectionism "does not seek to *ground* the state in any particular conception of the good, but nevertheless holds that a government may legitimately *promote* the good" (p. 1). The job of the state is not to identify the forms of knowledge, rational activity, or personal relations that are truly good but to enable its people to make these discoveries for themselves.

The second noteworthy feature of the list is the inclusion of "decency and good taste," which may raise suspicions about what Sher would have the state do to promote "moral goodness" and "the appreciation of true beauty." Would Sher's perfectionism license attempts by the religious Right, for example, to impose its conception of "traditional morality" and a steady diet of Norman Rockwell paintings on American society? This hardly seems to be Sher's intent. Indeed, he is more concerned "that an endless diet of punk rock or sitcoms leaves one ill-equipped to appreciate the music of a Mozart or the prose of a Jane Austen" (p. 213). What exactly he would have the state do to turn people away from sitcoms and toward *Sense and Sensibility*, and how he would prevent the state from forcing sectarian or majoritarian views on everyone, is a subject Sher does not address.

I wish that he had, just as I wish that he had addressed some of the cases raised by fundamentalists of various religions who apparently value autonomy much less than do the philosophers on both sides of the neutralist-perfectionist debate. How, for instance, would Sher's perfectionism settle the case of *Wisconsin* v. *Yoder*, in which the Old Order Amish maintained that the salvation of their children and their community was of greater importance than the goods of knowledge and the development of one's abilities?

Sher would respond, I suspect, that a perfectionist theory of politics need not settle every issue. For the open-ended, pluralistic version of perfectionism that he defends so ably in this book, in fact, the complete absence of public controversy would be a sign of failure. To which I can only respond, books as good as this one always leave the reader wanting more.

And this is a very good book indeed. It brings much-needed clarity and philosophical depth to the neutralist-perfectionist debate, and it presents a challenge to the neutralists that they cannot ignore. If *Beyond Neutrality* is not the last word on the subject, the reason may be that so many other words will be written in response to it.

Vindicating the Founders: Race, Sex, Class and Justice in the Origins of America. By Thomas G. West. Lanham, MD: Rowman & Littlefield, 1997. 218p. $24.95.

James H. Read, *College of St. Benedict*

The aim of this book is to defend the founders against the charge that they were racist, sexist, and biased toward the interests of the wealthy. But its purposes go far beyond a mere defense: West seeks to turn the tables on the critics and demonstrate that the founders' understanding of racial equality, gender relations, property rights, and wealth was superior in every respect to that of modern liberalism and feminism. He targets not only those who directly charge the founders with racism, sexism, and so on, but also scholars who claim the founders' understanding of racial and sexual equality was progressive for its day but limited by the circumstances of the age. For West, the founders' philosophy is true for all times, and our most serious contemporary problems result from forgetting or turning away from it.

West's "vindication" thus takes two somewhat different forms. The first is a defense of what the founders actually said about matters of race, sex, class, and justice. The second is a description of what he thinks they would have said about an enormous range of modern-day issues, including environmental regulations, women entering the work force, AFDC, and single-parent households. The author displays no doubts about the founders' views on current issues. All of them—Madison, Jefferson, Hamilton, Adams, Wilson, Franklin, Washington—would be political, economic, and social conservatives opposed to modern liberalism and modern feminism.

West's argument can be summarized as follows. (1) The founders held a single, coherent philosophy on matters of race, sex, property, and justice (no significant differences among the various founders are described). (2) This philosophy is true. (3) Nothing that has occurred in the last 200 years—no change in polity, economy, or society—alters in the slightest the truth and relevance of this philosophy.

The most convincing and historically focused chapter in the book deals with slavery and race. In answer to the frequently repeated charge that the founders did not consider persons of African descent to be "men" at all, West marshals an impressive body of evidence in support of the claim that they considered them to be "men" entitled to the natural rights announced in the Declaration of Independence. He argues that it was the logic of the Declaration that led to the abolition of slavery in the northern states and to a wave of manumissions in the South.

If the founders knew that blacks were equally entitled to natural rights and that slavery was wrong, why did they not—especially Jefferson—do more to abolish it? Here West runs into difficulties. He is on fairly solid ground in claiming that abolition could not have been accomplished at the Constitutional Convention. But why was the cause not taken up seriously afterward? Benjamin Franklin's last public act was to bring an antislavery petition to Congress. Why did Jefferson and others not follow Franklin's example? West

takes at face value every one of Jefferson's excuses for postponing action against slavery. Would abolition have ranked higher on Jefferson's list of priorities if those held in slavery had been white?

The chapters on women and the family are the least convincing in the book. West argues that the founders did consider women to be entitled to the same natural rights as men. But they did not believe voting rights for women were especially important, because they saw no divergence of interest between women and men: "Americans of both sexes thought women were well represented by their men whether they voted or not" (p. 78). West also points out that women wholly dependent for their support on their husbands were "suspected to have no will of their own" (p. 80) and thus, it was argued, could not cast a free vote. He sees no contradiction between this notion and the true equality and common interest he claims existed between the men and women of the age.

West's principal quarrel with modern feminism concerns its denial that the traditional sexual division of labor is founded in nature. "Nature points most women toward, and most men away from, the care of small children," he insists, and "by nature, men are more likely than women to gravitate toward technical expertise and full-time careers" (p. 89). To deny these natural differences is to reject the ideal of the Declaration of Independence, "which recognizes and protects the equal rights, and therefore the natural differences, of all" (p. 88).

On property rights and wealth West argues that the founders' intention was not to protect the wealth of an elite but to create economic opportunities for the middle class and poor. The founders favored public relief for the poor who were unable to work. But for the able-bodied they believed the best poverty-reduction measure was a dynamic economy stripped of all feudal limitations on the use of one's property, such as entail and primogeniture. This leads West into an attack on modern-day restrictions on property rights. He claims that the founders would have considered modern environmental regulations regarding wetlands and endangered species as "unjust (because uncompensated) seizure of private property" (p. 55). Environmental regulations also hurt the poor by making it difficult for them to start a business.

It is perfectly legitimate to make a contemporary political argument inspired and shaped by a reading of the political thought of the founders. It is another thing for West to claim that his own arguments are really those of the founders, even on issues and problems they could not have foreseen. This is to abuse the founders' authority. And it will "vindicate" the founders only among those who already share the author's political and social views. The book is likely to make many readers—unfairly, in my view—more suspicious of the founders than they were before.

A Trumpet of Sedition: Political Theory and the Rise of Capitalism, 1509–1688. By Ellen Meiksins Wood and Neal Wood. New York: New York University Press, 1997. 150p. $30.00.

Edward J. Harpham, *University of Texas at Dallas*

Over the past twenty years, our understanding of sixteenth- and seventeenth-century political thought has undergone something of a revolution. Studies that traced out the rise of liberalism or the possessive individualist paradigm in light of emerging capitalism have been replaced by complex histories of civic humanism or natural jurisprudence. These newer texts explore the evolution of various languages of discourse in different historical contexts. While insightful, these studies all too often suffer from a complexity that is difficult to read, much less understand. The themes integrating a particular episode in the history of political thought often get lost amid a wealth of details and convoluted arguments.

One pleasure of *A Trumpet of Sedition* is the way in which the story behind sixteenth- and seventeenth-century English political thought is made intelligible to the reader. Abandoning much recent scholarship that situates ideas in a largely intellectual context, the Woods unabashedly adopt a Marxist approach to the study of ideas. Ideas are not simply free-floating abstractions that evolve from thinker to thinker. They are responses to and reflections of the social, political, and economic environment of which they are a part. In the hands of some, such an approach often deteriorates into a simplistic can that traces out the rise of capitalism and the triumph of the bourgeoisie. In the hands of the Woods, however, the approach sheds new light on an old story.

According to the authors, three historical factors define the context within which political thought should be understood in the sixteenth and seventeenth century. Agrarian capitalism laid the groundwork for market-oriented economic development and the accompanying social dislocation. The centralized state built by the Tudors bequeathed to the Stuarts state institutions that were unified but vulnerable in important ways. In response to these turbulent times, there emerged a number of important radical thinkers who helped change the way in which political and economic life was understood. Even when defeated, the legacy of these radicals lived on in popular movements and in the thought of their adversaries, who were forced to confront their ideas. Taken from a passage in Hobbes's *De Cive*, the book's title captures the important role that the Woods want to grant radical theorists in the history of ideas.

The Woods are most creative in their treatment of sixteenth-century thought, which draws heavily upon the earlier work of each. Through a succinct analysis of largely unheralded theorists, they explain how the revival of classical humanism in England gave rise to a new "mode of social criticism" that moved beyond classicism. This new social criticism was followed by a number of conceptual innovations. Thomas Smith (1513–77) envisioned the state in terms of individuals, not corporate entities, and helped to lay the foundations for political economy by arguing that avarice could be a positive good conducive to civil order. John Ponet (1516–56), the first advocate of violent resistance to government by private individuals, articulated a secular notion of government as a trust. Discussions of Smith and Ponet, as well as the more well-known Sir Thomas More (1478–1535) and Richard Hooker (1554–1600), provide the backdrop for creative analyses of the Levelers, Hobbes, and Locke.

Although brevity and clarity may be virtues, particularly for writers, even virtues have their limits. *A Trumpet of Sedition* has only 137 pages of text, and it cries out for more extended discussions of certain topics. Many influential figures are left out of the story, such as the Dutchman Hugo Grotius and the Englishmen John Selden and John Milton. No real attempt is made to engage other scholars in a dialogue over how one should read sixteenth- and seventeenth-century English political thought. Although criticism is made of the work of Richard Tuck, who published a considerably longer book on the same period in 1993, the Woods make no serious effort to explain how their accounts differ from his or how they agree. With only seven extended footnotes in the whole, relatively few embedded references, and a brief annotated bibliogra-

phy, this book is largely inadequate for anything beyond an introductory course for graduate students.

The lack of a more scholarly orientation is unfortunate for another reason. It begs the question of the limits to a traditional Marxist approach to the history of ideas. What is gained by adopting such an approach? What is lost? Must the insights of scholars such as Quentin Skinner or J. G. A. Pocock be ignored because they work out of different intellectual traditions? More to the point, must readers adopt the value orientation built into a Marxist framework if they are to profit from reading books such as this? In the last few pages, the Woods make clear their own political orientation. They see modern capitalism as standing in the way of an "emancipatory project" begun by early modern radicals like the Levelers and the Diggers in the seventeenth century. All readers may not agree with the authors' starting assumptions or conclusions. As the twentieth century has demonstrated time and again, being radical is not tantamount to being right. Much political tragedy has resulted from the misplaced utopian dreams and emancipatory projects of radicals. But it would be unfortunate if different value premises to the study of ideas or to politics in general kept readers away from this short book. *A Trumpet of Sedition* deserves a broad audience and a close read.

Postmodern Platos: Nietzsche, Heidegger, Gadamer, Strauss, Derrida. By Catherine H. Zuckert, Chicago: University of Chicago Press, 1996. 351p. $56.00 cloth, $19.95 paper.

Bernard Yack, *University of Wisconsin, Madison*

Catherine Zuckert's new book is a comparative study of the interpretations of Plato's thought and legacy offered by Nietzsche, Heidegger, Gadamer, Strauss, and Derrida. That said, one may begin to wonder whether the title of the book, *Postmodern Platos*, represents anything more than a marketing maneuver. Such suspicions, however, are unwarranted. Zuckert has good reason to describe these five encounters with Plato as "postmodern," for they are all inspired by a diagnostic interest in identifying and moving us beyond the characteristic defects of modern life and thought. All five of the philosophers studied in this book think of themselves as, to use Nietzsche's expression, "doctors of sick culture." And all five come to the conclusion that developments in Western philosophy play a crucial role in generating modernity's ills. Accordingly, they all turn to the study of Plato, the founder of the Western philosophic tradition, as a form of therapy—whether, as in the case of Nietzsche, Heidegger, and Derrida, in order to figure out exactly where we went wrong or, as in the case of Gadamer and Strauss, in order to recover the original and superior understanding of philosophizing that modern developments have obscured from view.

At first glance, Strauss may appear to be the odd man out in this philosophic gang of five. After all, while he stubbornly defends both classical rationalism and the possibility of understanding texts as their authors intended them to be understood, the others rank among the most important critics of classical rationalism and defenders of the hermeneutic or interpretive turn in continental philosophy. But Zuckert does a good job of showing how Strauss fits in with this group. In particular, she makes a strong case that his turn to Platonic rationalism is inspired by many of the same Nietzschean and Heideggerian questions that lead Gadamer toward hermeneutics and Derrida toward deconstruction.

One of the nicest things about Zuckert's book is the way in which it brings to life the dialogical character of these thinkers' approach to philosophy. All five treat their encounters with the great minds of the past as a form of philosophizing which, when done properly, can lead to surprising insights about oneself, rather than just information about past beliefs. By bringing their interpretations of Plato together in one book, Zuckert impresses us not only with the wealth of insight and intellectual vitality that this dialogic approach can produce but also with the variety of ways in which it can be carried out. (I would add, though this is not a point Zuckert makes, that viewing these five different versions of dialogic philosophizing together also helps us better locate its inherent limitations.) Moreover, by engaging these thinkers in a continuing dialogue with one another, Zuckert gives her book some of the dialogic sensibility that they all prized.

In order to bring these thinkers together in this way, Zuckert has to exaggerate a little the importance of Plato in the work of Gadamer and Derrida. For them, I suspect, the study of Plato deepened and confirmed, rather than inspired, their most important insights. In any case, their accounts of Plato lack the sense of mystery, the sense that there is a secret waiting to be discovered, that hovers around the encounters of Nietzsche, Heidegger, and Strauss with Platonic thought. Nevertheless, this exaggeration is not very great and does not prevent us from benefiting immensely from the comparisons that it allows Zuckert to construct.

One final point. Readers will quickly notice that Strauss receives two or three times more space in this volume than any of the others. This allocation reflects both the author's sympathies and her accurate assumption that readers will be least familiar with the philosophic background of Strauss's work. While this disproportionate attention may disappoint those looking for fuller accounts of the other thinkers, I do not believe it is a serious flaw. The accounts of Nietzsche, Heidegger, Gadamer, and Derrida are all fair, accurate, and insightful, even if more might be said about their encounters with Plato. Moreover, by studying Strauss in the context of the philosophic questions that he shared with these other thinkers, Zuckert corrects the unfortunate tendency of many of his friends and enemies to treat his thought as sui generis, as philosophically, if not politically, rootless. As a result, Zuckert's book offers us not only a fine comparative study of different interpretations of Plato but also a very helpful introduction to Strauss's philosophical thought.

The Natural Rights Republic. By Michael Zuckert. Notre Dame, IN: University of Notre Dame Press, 1997. 304p. $32.95.

Robert Webking, *University of Texas at El Paso*

The republic of which Zuckert writes is the United States as created in the final quarter of the eighteenth century. The title makes clear Zuckert's fundamental argument: Although the Americans did not create the philosophical theory of natural rights, they made the most explicit and thoroughgoing commitment to it that had yet been practiced anywhere. Zuckert works both to explain the use of this philosophy as the foundation of the American regime and to defend the argument, put forward by other students of the American founding, that the belief in natural rights was not the core concept of what is advantageous and just that shapes American political life. Zuckert focuses on the Declaration of Independence as the most clear and important document stating the Americans' adherence to the natural rights position as well as their "political creed" during and since the founding generation. In exploring its meaning, Zuckert relies

heavily on the political thought of its author, Thomas Jefferson.

Part I is devoted to an analysis of the Declaration and an exploration of the philosophy it expounds. The first chapter confronts the word themselves, seeking the meaning of the equality and rights mentioned in the most well-known passage. The Declaration asserts that its principles are "self-evident" truths, and Zuckert takes the assertion seriously, focusing first upon the language and structure of the document itself. The reading which results shows clearly that liberalism is at the core of the Declaration and casts doubt on the validity of the classical republican and communitarian approaches to understanding the foundations of American politics. It also points to the problems with the interpretations offered by other commentators, especially Garry Wills, Paul Eidelberg, and Morton White. The second chapter reflects on the asserted self-evidence of the truths. Zuckert notes not only the meaning of "self-evidence" but also the importance of the citizenry "holding" truths to be self-evident. He next moves to an effort to elucidate the deeper philosophical meaning of the Declaration's principled statements in a way that the short and powerful phrases of the document itself cannot. With the comment that the founders apparently believed the ground of the truths they affirmed in the Declaration, Zuckert examines that ground especially through analysis of Jefferson's political thought. He turns particularly to "Notes on the State of Virginia" to explore Jefferson's understanding of nature and human nature, and on the basis of that discussion Zuckert develops the meaning of and relationships among the natural rights. He clearly shows Jefferson's adherence to the position that there is an important natural right to property, along with rights to life, liberty, and the pursuit of happiness.

Zuckert uses Part II to consider three competing visions of the thought behind the founding. He maintains and, indeed, further clarifies the basic importance of natural rights for Americans while suggesting that three alternative explanations of America may contribute something to the way Americans see themselves and political life. Zuckert's discussion of the view that the American Revolution was not based on novel principles but on a continuation of themes from the "old Whigs" of the British Glorious Revolution takes the form of comparing the principles embraced in the Declaration with those of the English Declaration of Rights, a document written to explain the Glorious Revolution. The comparison shows powerfully the American departure from traditional views on human beings and politics toward the new Lockean understanding.

Moving to the view that the American founding is best understood in terms of the Puritan religious principles that animated the immigrants in the seventeenth century, Zuckert compares the philosophical position of the Declaration to that taken in the Mayflower Compact and in a John Winthrop sermon. The clear and striking differences show plainly the movement away from Puritan principles to the natural rights philosophy. That movement can be seen in the views of Protestant ministers themselves, as Zuckert shows in the chapter on the "Lockeanization of Protestant Political Thought."

The final chapter discusses the view that "classical republicanism" is the perspective on human beings and politics that lies at the base of the American founding. Again, Zuckert shows the incompatibility of that view with the natural rights philosophy of the Declaration of Independence. He then continues to explore the role of a kind of republicanism in Jefferson's political thought. He discovers there, more than in others of the founding generation, a new kind of republicanism, but one still focused on the importance of individual natural rights.

It is often difficult for people to consider the meaning of words so familiar as those in the Declaration of Independence. The difficulty is further complicated when the words have led the reader to see human beings and politics in a particular, usually unexamined, way. Zuckert's book helps open up to examination the meaning of the Declaration and, through it, of the American founding. The direct analysis of the Declaration is clear and compelling. The meaning and implications of the philosophy are made still more clear by the analysis of Jefferson's thought (which, of course, cannot be taken simply as the thought of the American founding) and especially by the comparisons to the English Declaration of Rights and Protestant thought of the seventeenth and eighteenth century. In leading readers to work with and reflect upon these writings in the way he does, Zuckert helps them not only to a clear view of the natural rights republic but also to consideration of the meaning and sources of that republic, and to thought about the relationship between philosophy and political life.

American Politics

Democracy and the Policy Sciences. By Peter DeLeon. Albany: State University of New York Press, 1997. 160p. $18.95.

Policy Design for Democracy. By Anne Larason Schneider and Helen Ingram. Lawrence: University Press of Kansas, 1997. 241p. $35.00 cloth, $15.95 paper.

Matthew A. Cahn, *California State University, Northridge*

Policy studies have emerged as an increasingly important subfield within the discipline over the past decade, with the policy sciences literature taking an increasingly central place in that emergence. The subfield is torn among competing paradigms and methods. Area studies, policy theory, process analyses, strategic analyses, and evaluative research each offer insight into policies and their implications. *Policy Design for Democracy* and *Democracy and the Policy Sciences* stake out what may become the most important niche in the subfield: the linkage of policy tools (tools for making "better" policies) with the normative concern for increasing civic participation. Each book argues persuasively that policy design is a purposeful effort reflecting specific values and interests, not merely the result of systematic political processes. If this is true, then surely policy designs with democracy as a central value are an appropriate and worthy end.

Schneider and Ingram begin with a statement of their central contention: The structure and design of contemporary public policy discourages active citizen participation. "Rather than provide institutions and symbols to ensure that the self-correcting mechanisms of pluralist democracy will be operative, the policies deceive, confuse, and in other ways discourage active citizenship" (p. 5). The book begins with a review of the four dominant theories of public policy: pluralist democracy; policy sciences; public choice; and critical theory. Pluralist theory contributes insights into the impor-

tance of participation, coalition building, and procedural justice. The policy sciences contribute an applied concern for linking policy instruments to specified goals. Public choice contributes a meaningful language of the marketplace of ideas, goods, and services, and it challenges us to limit governmental interference into any of them. Critical theory contributes both a substantive normative foundation for democracy as well as a method for unmasking degenerative social constructionism. More important, the book reviews the evolution of policy design theory, allowing the authors to assess the role of social constructionism implicit in public policy, both in defining policy targets (those who will benefit or be punished) and in defining policy knowledge (interpretations of reality). The book then assesses each prominent theory for its contributions and limitations. Based on this discussion, the authors develop a series of observations that can be applied to real-world policies. They conclude by suggesting that these observations can be used as general principles in democracy-based policy design.

Are they correct? The role of social construction in policy design is of critical importance, but all too often completely ignored, as if the world were perceived in precisely the same way by every person in every circumstance. In this Schneider and Ingram make their most useful contribution. Drawing on deconstructionism to demonstrate that traditional policies have always *implicitly* constructed a policy environment, but one that actively discouraged civic participation, the authors demystify the social engineering that characterizes any public policy. If social perception varies, then policy narratives, as well as policy language, must implicitly define the context of social environs. As a consequence, the general principles that Schneider and Ingram suggest may allow policymakers to construct a social environment within which the institutions that foster civic participation are not merely encouraged but mandated.

There is one enduring problem with the logic of *Policy Design for Democracy*. Schneider and Ingram accurately point out that policy designed by scientists and professionals will consistently reflect the values and interests of the sciences and professions—elites by any meaningful definition. Is it possible for policy scholars to suggest a set of neutral principles that would democratize the policy process? Alternately, if policy elites were able to engineer policy designs such that they would construct a more open process, would this suggest that policy professionals were in fact able to exceed their narrow values in the interest of democracy generally? This catch-22 is not merely semantic. It goes to the core of policy scholarship. If policy professionals cannot design interest-neutral policies, then Schneider's and Ingram's general principles themselves may be suspect. If, at least on occasion, policy professionals can offer design principles that reflect the broader interests of democracy, then is it not possible that the policy scientists and professionals may be able to design efficient and interest-neutral policies? Since Lindblohm, policy scholars have recognized the often tense relationship between efficiency (policy success) and democracy. In many technical policy areas, more participation may inhibit rational policy design. The current environmental justice literature provides ample illustration of this: The NIMBYism surrounding many environmental questions has actually exacerbated unequal distribution of policy benefits. In this context the tension between efficiency—even equity—and democracy can be understood with greater complexity. Is it always true that more participation is better?

This tension is by no means detrimental to the quality of the argument. In fact, it enhances the intellectual utility of the book. Through engaging this tension in this argument, the authors force readers to engage the tension in the discipline generally. Specifically, is it the function of public policy to resolve narrow public problems in a value-neutral way, as a mechanic might approach a rattle in a car, or is public policy responsible to the broader structures of maintaining democratic values and civic participation? The book is cogent and well written, offering clear discussion in a logical format with just enough restatement of the central thesis in each chapter to allow the reader to follow the discussion clearly and substantively. In reviewing the major themes of the policy subfield, and in offering an analytic framework for improving both the understanding of the policy literature and, indeed, the policy design approach, the book is useful for graduate courses, policy professionals, and the discipline generally, as well as policy scholars in search of a central paradigm.

In *Democracy and the Policy Sciences*, Peter DeLeon confronts the role of the policy sciences specifically in inhibiting democratic participation. He begins with a discussion outlining what he believes to be the problematic nature of American democracy. In so doing he presents a wide discussion on democratic theory as it is manifested in the American national psyche. He then applies the democratic discourse to the foundational literature of the policy sciences in an effort to excavate its democratic foundations. Finally, DeLeon assesses the efforts of the policy sciences to get beyond the positivist quagmire of economics-influenced analysis. In the end, he integrates these discussions in an effort to build a policy science capable of rescuing participatory democracy. In short, Americans are increasingly frustrated with political gridlock, impersonal government, and bureaucracies that appear Byzantine to anyone outside the organizational structure, all of which is reflected in low levels of traditional participation (e.g., low voter turnout) and increasingly harsh displays of political cynicism (e.g., "bureaucrat bashing"). Rather than inadvertently encourage this frustration, DeLeon asks what the policy sciences can do to convert that passion into positive participation.

The core problem, DeLeon reminds us, is that the sheer size of the citizenry makes traditional notions of civic participation impossible. Direct participation in policy decisions is impractical, and participation in policy hearings is at best inconvenient for those outside policy circles. Whether or not voluntary associations ever played the dominant role suggested by Bellah and others, they are increasingly difficult to maintain in our fractured global economy. In such an environment, the Madisonian accommodation of interest group democracy may be a viable—if not desirable—compromise. The problem with much of the traditional literature on democracy and participation is that it assumes a world that no longer exists—and perhaps never did: moderate-sized communities of civic minded, knowledgeable citizens with the time and desire to follow politics closely. DeLeon's argument, then, that a rational science of policy ought to explore alternate mechanisms for maximizing civic participation, is a refreshing change. *Democracy and the Policy Sciences* is an excellent read, perfectly suited for use in graduate seminars and for policy scholars with an interest in the normative concerns of building and maintaining democratic institutions. DeLeon offers a useful review of democratic theory and the evolution of the policy sciences, and he ultimately offers a "tapestry" (p. 99) of approaches that are reflective of participatory policy analysis in an effort to "advance the policy sciences in ways that they will contribute positively to . . . democracy" (p. 11).

In summation, both these books have an engaging style and initiate normative contemplation that ultimately links democratic theory to the applied concerns of the policy literature.

In this, Schneider, Ingram, and DeLeon persuasively remind policy scholars and professionals alike that we are not merely mechanics responding to the pervasive but ambiguous rattling of the body politic. Rather, public policy manifests the tools of a living civilization and therefore must incorporate the organic values and extant expectations of a still evolving civic democracy. Should the policy sciences abdicate their responsibility to encourage, prod, and facilitate democratic institutions and civic participation, we are doomed to the tyranny of technocrats.

Gender Power, Leadership, and Governance. Edited by Georgia Duerst-Lahti and Rita Mae Kelly. Ann Arbor: University of Michigan Press, 1995. 305p. $47.50 cloth, $17.95 paper.

Sue Thomas, *Georgetown University*

The literature on women officeholders—elected and appointed—has evolved tremendously over the past 25 years. During the first stage of research on this topic, scholars focused on sociological differences between women and men who held office. Attention was paid to educational and occupational backgrounds, ambition levels, points of entry into politics, and family status. The second research stage illuminated questions of differences in voting record, ideological proclivities, issue support, sense of women's status in the political realm, and activity levels on routine legislative tasks. The third stage, moving toward issues of women's influence in legislative life, concentrated on differences between women and men in policy priorities, success levels of bill introduction and passage, and proportionality—or whether a higher concentration of women was likely to mean greater focus on women-centered legislation.

Rapidly expanding is the fourth stage, which questions the gendered nature of institutions themselves and the sort of effect women can have in places and with processes that are masculinized. Central to this debate is *Gender Power, Leadership, and Governance*, edited by Georgia Duerst-Lahti and Rita Mae Kelly. The editors offer five purposes for the volume, chief among them that concepts of gender power are central to analysis of government and governance and that institutional sexism leads to differential gender power and to differential political power. Underlying these aims are the fundamental arguments that feminism and masculinism are more than ways of being; they are ideologies. Because masculinism is considered the norm, men and masculinity are given the privileged positions in society and politics.

To illustrate their reasoning, the editors offer chapters organized into various sections. Part Two explores the important difference between sex and gender, the former being a biological construction, and the latter a social one. Two empirical questions are raised in these chapters: why there are so few women in top political positions, and how women's influence in legislatures can be measured. According to Cheryl King, answers lie in notions of the compulsory masculinity that operate in political institutions, regardless of the sex of the officeholder. Karin Tamerius argues for the need to move beyond roll-call voting scales to assess the effect of women. Rather, indicators should include operations at all phases of the legislative process and analysis of the differential resources that women and men bring to bear on the process.

Institutional sexism and structural barriers to women's equal participation are the focus of Part Three. The chapter by Noelle Norton on the importance of committee positions proposes an augmentation to existing literature on women legislators' influence on policy outputs. Her point is that agenda control is related to issues of institutional sexism and that, at a minimum, women need to hold enough positions of power to be able to put the issues most important to them in the foreground. Meredith Newmann analyzes conventional political science wisdom about the work of policy agencies and suggests that typologies such as Lowi's are deeply imbued with gendered assumptions which affect both clients and those in leadership positions in the bureaucracy.

Another long-neglected aspect of gendered institutions is what Duerst-Lahti and Kelly call the "interpersonal dimension." How and how much can and do women participate when psychological power in political institutions still resides with men? Whether the issue is verbal communication patterns and practices or sexual harassment, the silencing effects can be dramatic. Lyn Kathlene's chapter highlights how communication styles in legislatures are dominated by men, even when women are committee chairs. Her work once again calls into question the influence of women in legislatures, given the underlying and unquestioned norms of masculine hegemony. More overt is the psychological warfare of sexual harassment. Rita Mae Kelly illustrates how leadership by women can be undermined by instances of workplace intimidation and harassment. That direct link between performance and safety provides yet another example of what has long been called "the chilly climate for women" in the public sphere.

Part Five offers analyses of the symbolic meaning accorded women's presence in politics. Georgia Duerst-Lahti and Dayna Verstegen discuss the much heralded "Year of the Woman," and Mary Ellen Guy writes about Hillary Clinton's foray into the health care debate. The underlying issue in both chapters is boundaries. Are women overstepping them, in the case of Hillary? Or are women so excluded that the spectacle of an all-male Senate Judiciary Committee debating allegations of sexual harassment brings the lack of descriptive representation of women in Congress squarely into the 1992 election cycle?

Whether talking about legislators, bureaucrats, or executives, the contributors to this volume ground analyses of women's place in politics in the metaideology of masculinism and its effect on women's presence, actions, influence, and future. The editors conclude by asserting two claims. The first is the need for gender-balanced, not just sex-balanced, political institutions. Without it, the perspectives, influences, and contributions of some of our citizens are muted or excluded. The resulting price is a high one and is paid both by those with privilege and those without it. A final, highly relevant point for political scientists concerns the need for researchers to analyze politics through a gendered lens, whether or not their research concentrates directly on issues of women in politics. A full understanding of the nature of power and the realm of politics is not possible until gender is part of the fundamental basis for all exploration. These points are so well taken that they cannot be made often or insistently enough. Because of them, *Gender Power, Leadership, and Governance* is an important addition to the literature.

Women's Political Voice: How Women Are Transforming the Practice and Study of Politics. By Janet A. Flammang. Philadelphia, PA: Temple University Press, 1997. 419p. $69.95 cloth, $24.95 paper.

Mary Hawkesworth, *University of Louisville*

In characterizing our scholarly investigations as a mode of scientific inquiry, political scientists claim to generate objec-

tive accounts of the political world. Sophisticated methods, replicability, and falsifiability are relied upon to ensure the objective validity of the discipline's scientific claims. The critical, nondogmatic, disinterested pursuit of truth is the standard set for well-trained political scientists, and the constant examination and testing of competing hypotheses constitute the means to achieve the standard.

How well does the discipline of political science live up to this regulative ideal? In *Women's Political Voice*, Janet Flammang provides compelling evidence that it falls far short of the mark. Her comprehensive overview of thirty years of feminist scholarship on American women's political behavior demonstrates that mainstream political science is riddled with erroneous claims about women and about American politics, that androcentric bias permeates supposedly "neutral" methods of inquiry, surfacing in problem selection, research design, definition of key concepts, decisions concerning relevant evidence and counterevidence, data collection and analysis, and interpretations of results. Flammang carefully documents feminist scholars' refutations of the discipline's conventional views and shows how research on the political experiences of women and people of color, the majority of American citizens, changes the categories of political analysis and the substantive understanding of American political life.

The book is structured to engage the discipline's dominant paradigms in the areas of mass politics (political mobilization and political participation studies), elite politics (political recruitment, policy preferences, and political styles of male and female elected and appointed officials), and elite-mass interaction (agenda setting and the development and maintenance of political coalitions). Within each area of investigation, Flammang illuminates the means by which deeply entrenched disciplinary assumptions preclude political scientists from attaining an objective account of women's political involvement. She demonstrates, for example, how the state-centered paradigm of proponents of an institutional definition of politics and behaviorists' restrictive focus on interest groups, parties, and elites converged to render invisible 150 years of political mobilization in the women's movement. She shows how gender stereotypes substituted for empirical analysis in early studies of female voting behavior, generating unsubstantiated claims that wives followed their husband's lead in casting their ballots. She points out that survey researchers' accounts of male feelings about politics sustained claims about men's greater political participation in years when females were devoting more hours to the work of political parties and were casting more ballots than men. She demonstrates how reliance on roll-call analysis as an indicator of legislative performance underestimates the difference female legislators make with respect to the substantive representation of women's interests.

In addition to refuting erroneous claims, Flammang sifts through hundreds of feminist studies of women and politics to generate a portrait of female political activism and a systematic account of the obstacles to equality women still confront in American politics. In so doing, she suggests that women's understanding of their political action raises important challenges to the received view of politics in the discipline. In contrast to the instrumental assumptions of the self-interested maximizer that undergird rational choice and pluralist approaches to politics, women continue to explain their political behavior in terms of efforts to achieve the public good, social justice, and meaningful equality. In contrast to the trustee/delegate/politico and broker/ritualist/inventor typologies of the legislator's role, studies of women legislators suggest that a more complex set of categories are necessary to capture their behavior. And in contrast to the discipline's emphasis upon interest groups and policy entrepreneurs, or the "national mood" and elections as keys to agenda setting, the complex interaction between grassroots activists in the women's movement and feminist policy networks suggests a markedly different model of agenda setting.

It is impossible to do justice to the richness of Flammang's account in a short review. But it is important to note that *Women's Political Voice* succeeds admirably on a number of different levels. It provides a sophisticated account of the central epistemological and methodological issues that divide mainstream and feminist approaches to political science. Indeed, Flammang suggests that these issues help explain the failure of the discipline to engage the refutations and counterevidence advanced by feminist scholars. The positivist presuppositions that inform mainstream approaches in political science provide a form of methodological immunity against the postpositivist arguments of feminist scholars.

The book also examines two markedly different feminist approaches to the study of women and politics. Extrapolating from a framework developed by Sandra Harding in the *Science Question in Feminism* (1986), Flammang contrasts the works of feminist "accommodationists" (who believe the discipline requires only marginal adjustments to accommodate women's political practices) and feminist "transformationalists" (who believe the discipline must undergo major epistemological and methodological changes to capture the full range of women's political experience). Although Flammang makes every effort to be fair in her treatment of "accommodationists" and to demonstrate the important contributions they have made to an understanding of women and politics, she builds a consistent case for the superior merits of the transformationalist arguments.

Flammang also does an excellent job in bridging the gulf that too frequently divides feminist empiricists from feminist theorists. She lucidly demonstrates the emergence of debates in feminist theory from the complex political struggles within the women's movement. In turn, she uses arguments drawn from feminist theorists to illuminate policy stances and political strategies of feminist political activists. She provides the best discussion of feminists' continuing struggle to come to grips with issues of race, class, and ethnicity that I have seen. Her sensitive treatment of the legitimate claims of feminists of color includes important insights drawn from the works of African American, Asian American, Latina, Chicana, and Native American women activists and theorists. The singular omission from her comprehensive analysis of feminist coalitions and the "hard work of sisterhood" is a sustained discussion of lesbian feminist struggles within the women's movement.

Throughout the work, Flammang also develops a case study of Santa Clara County, California, "the feminist capitol of the nation," in order to show how transformationalist feminist political research illuminates problems for investigation occluded by the dominant disciplinary paradigms and affords richer explanations of women's political practices than mainstream accounts.

Women's Political Voice is a model of postpositivist scientific inquiry, employing theoretical insights and a range of methods to structure empirical investigations of contemporary politics. The account of political life it affords seriously challenges the adequacy of numerous disciplinary paradigms. In depicting alternative conceptions of and possibilities for political life drawn from women's political experiences, it raises the question of the discipline's complicity in producing and enforcing an overly narrow construction of the political world. In her even-handed examination of a wide range of

evidence and argument, Janet Flammang embodies the highest standards of scholarly investigation, and she affords the discipline of political science an opportunity to reexamine its dated presuppositions and test its status as a scientific endeavor.

***Everson* Revisited: Religion, Education, and Law at the Crossroads.** Edited by Jo Renée Formicola and Hubert Morken. Lanham, MD: Rowman & Littlefield, 1997. 242p. $63.00 cloth, $23.95 paper.

Lief H. Carter, *Colorado College*

In 1947 the U.S. Supreme Court announced, in *Everson* v. *Board of Education*, that the Constitution's Establishment Clause obligated the states to maintain the metaphorical wall separating church and state. In the same majority opinion, however, Justice Black concluded that state funding of bus transportation of students to and from Catholic schools did not breach the wall. Just as *Korematsu* (1944) had prefaced with a stern warning against racism its approval of the blatantly racist internment of Japanese-Americans during World War II, so many feared that *Everson* had produced a rhetorically camouflaged defense of public support for private sectarian schooling.

1997 marked the 50th anniversary of *Everson* and occasioned the ten original essays published here. In the cases both of racial discrimination and church-state relations, we now know that the rhetorics of *Korematsu* and *Everson* have displaced the precedential authority of their factual holdings, but neither area of constitutional law has settled into unquestioned public policy. For those who seek to formulate workable policies regarding the role of religion in public and private schooling, these essays are useful stimulants to rethinking creatively the major policy problems in that field, and especially policies concerning school vouchers.

These essays range very widely, from Daryl Fair's engaging if journalistic recounting of the New Jersey political forces that produced and shaped the actual *Everson* litigation, to attorney William Bentley Ball's memoir on his uses of the *Everson* precedent when he litigated subsequent Establishment Clause cases, to Robert Booth Fowler's provocative "skeptical postmodern defense" of government aid to religious schools. These essays do, however, have a central tendency. Five of the ten authors teach at schools with specific religious affiliations, and their writing pushes toward more accommodationist and less separationist readings of church-state relations.

Thus, Stephen Monsma of Pepperdine describes the *Everson* rhetoric of separation as "the wrong road taken." Jo Formicola of Seaton Hall provides a balanced and (for this non-Catholic) fascinating insight into "Catholic jurisprudence on education." Formicola both describes the efforts of the Catholic Church to develop a coherent position on Establishment law and the practical choices that the church must make, such as the obligation to honor diversity requirements when a Catholic school participates in student-loan programs. Hubert Morken, a biographer of Pat Robertson and founder of the Religion and Politics section of the APSA, traces Evangelicals' rejection of current separationist doctrine back to Horace Mann and the importance of nonsectarian religion in the programs of the old "common schools." Angela Carmella, a professor at Seaton Hall law school, and Mary Seegers, of Rutgers-Newark, provide legal, political, and theological background for understanding several proposed versions, particularly that of Rep. Dick Armey, of the Religious Equality Amendment.

These essays by no means tilt toward a fundamentalist campaign for "getting religion back in the schools." While Daniel Dreisbach's essay bemoaning the Supreme Court's failure to read the history of the Establishment Clause accurately has a Bork-ish slant, Derek Davis's argument rejecting mandatory moments of silence in public schools (a practice which Establishment doctrine now presumably permits) balances Driesbach's tone. The editors judge *Everson* as a desirable political event, if only because its ambiguity keeps alive debate over the many different practical applications of separationist and accommodationist views of religion in public life, but they also call for "a reorientation of the relationships between public education and religion and between private education and the state" (p. xii).

While these essays may well help educators who confront church-state political controversies in schools to think more clearly and creatively about their options, I have some difficulty imagining other academic audiences for which they will be useful. The problems with this collection may be endemic to any collection of essays that tries to focus on a "loaded" political topic from varying perspectives. The essays must be short, but that means neglect of interesting academic treatments of such matters, for example, LaRue's analysis of *Everson* in *Constitutional Law as Fiction* (1995). And there are the redundancies, the repeated descriptions of *Everson*, *Lemon*, and the familiar march of Establishment cases in the last 50 years (described not unlike biblical sequences of begattings). We read several times that Justice Jackson, dissenting in *Everson*, equated the majority opinion with Byron's Julia, who, "whispering 'I will ne'er consent,'—consented." Brevity obligates many of these essays to presume that readers have prior knowledge of the key cases in the field, but readers with no prior training in constitutional law will have to struggle to follow some arguments precisely.

Readers who do bring a background in constitutional law and practice to this collection will find some conceptual confusions and omissions frustrating. The central confusion arises from the strain to make the collection "about" *Everson*. Thus, the preface asks "whether *Everson* will continue to create problems within American society, or whether it will solve them" (p. xi). Some essays, such as those by Morken and Monsma, suggest that *Everson* deserves some share of blame for the current state of schooling and of legal policy about religion in schools. But cases in isolation have no such linear power and influence. In hornbook legal reasoning, stare decisis plays virtually no role in constitutional law. In constitutional practice, particularly in unsettled areas like this one, decisions flow from an immense complex of political and psychological influences, not the mechanistic application of precedent.

Many of the essays thus take the form not of political or policy analysis but of legal advocacy, a form with its own deeply biblical roots. With the exception of essays by Davis and Fowler, there is little if any acknowledgment of the political realities and power dynamics in public schools and on school boards when sectarian religious impulses hold power. Were these essays to analyze politically where Establishment law may go in the next decade (the "crossroads" in the book's subtitle), they would look at what the Court says and does now. Unfortunately, books cannot keep up with "now." Since this publication, the Supreme Court in the 1997 term overturned its rigidly separationist position in *Aguilar* v. *Felton*. The Court also overturned Congress's Religious Freedom Restoration Act. These cases will provide better clues for where Establishment and free-exercise policy may go than will railings about the failure of the Court to get First Amendment history "right" 50 years ago, especially in a case

that, on its facts, permitted state assistance to students in religious schools.

Because the bulk of these essays advocate legal positions, they omit much of scholarly literature. Dreisbach's criticism that the Court has fictionalized history (p. 33) cries out for references to arguments, such as those of LaRue (1995), that constitutional law inevitably does, and should, fictionalize. As legal arguments, these essays assume a clause-bound definition of doctrine, but other writers, including Ronald Dworkin and Sotirios Barber, offer strong readings of the Constitution as a whole that lead in very different directions than do debates over the *Lemon* test's interpretation of the Establishment Clause. For that matter, E.D Hirsch's recent writings, particularly *The Schools We Need* (1996), omitted from Morken's essay, would greatly strengthen his case.

My vote for the best read in this collection goes to Robert Booth Fowler's "Skeptical Postmodern Defense of Multiestablishment." He wisely sets aside the cases as piecemeal political events and turns to the critical question: Does not a commitment to a pluralistic and multicultural society necessarily entail a tolerance for religious groups? If so, does not this tolerance entitle religious groups to no less governmental support than the support other groups receive? But, he concedes, skeptical postmoderns reject religious explanations and world views out of hand. Why should they support the very groups that oppose them? His answer is compelling: If postmodern skeptics at least embrace the catholic respect for persons, then acceptance and support of those with whom we disagree must extend to the religious right. This may be the "essential test of whether we really do respect persons" (p. 186). It is to the credit of this volume's editors and authors that they maintain a tone of tolerance for and acceptance of those with whom they disagree.

Picking Federal Judges: Lower Court Selection from Roosevelt through Reagan. By Sheldon Goldman. New Haven, CT: Yale University Press, 1997. 488p. $45.00.

Susan B. Haire, *University of Georgia*

The delay in filling the record number of federal judicial vacancies in the 1990s has led many scholars to reexamine the influences that shape the selection process. Fortunately, with the publication of Sheldon Goldman's book, political scientists now have available what will become the definitive work in this area. Well-written and meticulously researched, it pulls together many disparate pieces to provide a thorough knowledge base detailing the politics of selection. More important, this study offers a theoretical framework for understanding the dynamics of the appointment process.

Central to this framework is a straightforward conceptualization of presidential strategies in making the appointments. According to Goldman, the process of identifying a nominee to the lower courts can be characterized as advancing policy, partisan, or personal agendas. In the first case, judicial selection is used to support the president's policy goals. In the second, the appointments are vehicles for shoring up political support for the president or his party. In the third, personal friends or associates of the president are appointed. These considerations need not be mutually exclusive; the difference lies in the primary emphasis underlying the motives of the president and administration officials. Critics may challenge this framework as one that does not recognize the "uniqueness" of each administration's judicial selection process. Goldman's analysis addresses this issue by analyzing appointments at the individual judge level and recognizing that a president may advance different agendas at different times, depending on the circumstances surrounding the appointment. The study makes clear, however, that in most administrations, a particular agenda dominates the procedures for identifying nominees.

After introducing the historical and theoretical perspective, Goldman presents a fascinating account of appointments made by presidents Roosevelt through Reagan. His research is exemplary in part because of his rigorous analysis of presidential papers, Department of Justice files, and other historical documents. Readers are given details on individual appointments as well as an understanding of the agendas adopted by the appointing president. For example, Goldman traces Roosevelt's appointment of Peter Woodbury to the U.S. Court of Appeals, First Circuit. Union leaders and officials in the administration, including Secretary Perkins, were opposed to the nomination, as they believed Woodbury was "antilabor." Unwilling to drop the nomination, officials in the Department of Justice analyzed 31 economic and labor decisions in which Woodbury participated as a state supreme court justice. (He was eventually confirmed.) Overall, Goldman finds that 24 of Roosevelt's 50 appointments to the appeals courts were in the policy agenda category, most of which occurred when the New Deal was being challenged. The book provides similar analyses of judicial selection processes of other administrations. Some readers will be surprised at what the analyses uncover, particularly with regard to varying levels of presidential involvement in the process. For example, contrary to the emphasis given by candidate Nixon to appointing "law-and-order" types to the federal bench, as president he paid almost no attention to lower court appointments and delegated much of the responsibility to John Ehrlichman.

In the analysis of each presidency, Goldman assesses the influence of participants outside of the administration who may affect the judicial selection process. Again, his careful analysis of historical documents provides a refined appraisal of the role played by home state senators and members of the Senate Judiciary Committee. Goldman's research documents a shift over time as administrations instituted procedures designed to strengthen the president's role in the selection of nominees at the expense of home state senators. The book also sheds light on the role of party leaders in judicial selection—an area previously unexplored by scholars. Goldman's work suggests a limited but important role here, particularly when administration officials attempt to make cross-party appointments. His research also chronicles the rise and fall (and rise and fall . . .) of the American Bar Association as a major player in the selection process.

Any research by Goldman would not be complete without profiles on the background characteristics of the judges on the lower federal bench. Perhaps what may interest scholars more, however, is his research on the actions of administration officials behind the scenes. For example, he documents several instances in which the names of women and minority candidates were suggested to officials in the Department of Justice and White House during the Eisenhower administration, but officials did not pursue these nontraditional candidates. Although a few female and minority judges were appointed during the Kennedy-Johnson years, Goldman's work clearly suggests that such candidates were not even given serious consideration until the Carter administration.

Overall, this research indicates that presidents generally do not use the policy agenda approach. With the exception of Roosevelt and Reagan, most presidents adopt partisan agendas in judicial selection. Goldman suggests these findings are related to the stability of the party system. He argues that during periods of party stability, presidents adopt partisan or

personal agendas. When the policies of the courts are seen as being out of step with an emerging new party system, presidents will adopt policy agendas. While these arguments are supported somewhat in his research, they are tenuous. Goldman himself acknowledges that the election of Nixon and his subsequent failure to adopt a policy agenda for lower court appointments suggest the limited utility of this explanation. Scholars in other fields of American politics also have debated when "party stability" exists and other issues in realignment (and dealignment), including the recalcitrant problem of where southern democrats in the 1950s and 1960s "fit" in a two-party system. Moreover, the relationship between party stability and judicial agendas may have changed beginning with the Reagan era, as judicial selection has become more contentious during periods of divided government. Additional studies will need to build on this, then, to develop our understanding of the appointment process as the dynamics that shape the selection and confirmation of nominees continue to shift in the 1990s.

Finally, Goldman's research also contributes to the scholarly literature because it highlights how much still needs to be done to develop our understanding of the links between selection influences and judicial behavior in the lower courts. Clearly, the conservatism of Reagan judges is not surprising given Goldman's description of the policy agenda adopted by administration officials. Yet, previous research also establishes the liberalism of Carter appointees in the lower courts, even though, according to Goldman, the selection process was not driven by a policy agenda. As scholars build models of judicial decision making, Goldman's work should be a constant reminder of the need to be more rigorous in accounting for the meaning underlying those "presidential dummy" variables.

The New Majority: Toward a Popular Progressive Politics. Edited by Stanley B. Greenberg and Theda Skocpol. New Haven, CT: Yale University Press, 1997. 333p. $30.00.

L. Sandy Maisel, *Colby College*

This book emerged from more than a year of discussions among a distinguished group of scholars and activists—all progressive Democrats—who are frustrated and dismayed by the course their party has taken in recent elections. Former Clinton pollster Stanley Greenberg and Harvard political sociologist Theda Skocpol were the catalysts for a group of more than a dozen who sought ways for their party to respond to the political environment of the current era and to achieve electoral success without abandoning the fundamental principles that they feel define the Democratic Party.

The theme of the volume is that a new majority in American national politics can be built by "championing the needs and values of American families striving for a better life in the face of unsettling changes. . . . People want to tame and channel changes that threaten the family, exacerbate racial conflicts, and separate the rich from all others" (p. 1). The theme is developed in an opening chapter by Skocpol and Greenberg and then in thirteen individual pieces, each of which examines a different aspect of the political agenda suggested. These articles are divided into three sections: The first contains five chapters that look at developing a new social contract between citizens and the government; the second has four chapters on how people can be brought back into the political process; and the third offers four discussions of different aspects of how this agenda will build a political majority. Quite an impressive undertaking!

The question is what to make of these suggestions, and the answer rests on a decision about what criteria one should use to judge them. By any standard, they raise the level of political dialogue from the drivel that dominates the airwaves and print media to a far more lofty plane. These are serious pieces, based on careful analysis of historical trends and current problems. In this regard, Alan Brinkley's chapter on the history of public investment in economic development and Michael Sandel's on the American tradition of civic virtue and the conflict among progressives—between those who fear the "curse of bigness" and those who feel the national government must intervene to counter the consequences of concentrated economic power—stand out. Similarly, Ira Katznelson's effort to demonstrate flaws in the conventional wisdom concerning the dire future for the Democratic Party in the South deserves careful attention.

The chapter by William Julius Wilson, on recasting affirmative action into a program for affirmative opportunity for those who are socially and economically disadvantaged, and the one by Theodore R. Marmor and Jerry L. Mashaw, who examine the need for continuing programs of social insurance, each show how traditional Democratic programs have lost their popularity because of the ways in which they have been presented to the American public and how they can be redefined so that more will see their appeal. These and most of the other contributions add significantly to the public debate over the policies our national government should follow.

But in another sense, Marmor and Mashaw point to the flaw in this book as well. As they state in discussing social insurance programs, "the real issue here is not economics but ideology" (p. 88). And the real issue with this book is not whether the authors are correct; that I happen to agree with them is beside the point. The real issue is not whether they present a progressive agenda in well-reasoned terms (which they most frequently do) but, rather, that their difference with Will Marshall (*Building the Bridge: Ten Big Ideas to Transform America*, 1997) or even, I suppose, with David Frum (*What's Right: The New Conservative Majority and the Remaking of America*, 1996) or Newt Gingrich (*To Renew America*, 1995) is one of what one believes, not of what one can demonstrate to be conclusively right. The arguments for and against the merits of the positions they espouse rest on one's values and assumptions, not on social scientific analysis.

In addition, the editors suggest another criterion on which the positions should be judged: Is this a blueprint that the Democratic Party could adopt in order to form a progressive electoral majority? The chapter by Margaret Weir and Marshall Ganz, on successful patterns of how to reintroduce civic engagement, and the one by Miles Rapoport and Marc Caplan, on successful efforts at democraticizing reform (based on their Connecticut experience), point to aspects of the blueprint that can meet their desired goal. Too much of what is suggested, however, seems like wishful thinking to the reader anxious for a game plan that will lead liberals back to victory.

In the final analysis, this book fails to deal with the distinction between the message and the messenger. The book is dedicated to Robert F. Kennedy, whose brand of politics inspired many of the contributors. As we approach 2000, they want their party to stand for what he represented in the late 1960s. They demonstrate well how progressive ideas can be reframed in the contemporary context. What they lack is a successor to Senator Kennedy who is willing to carry this message forcefully to the American people. I am afraid it is often the messenger, not the message, that the people hear.

Organizing for Foreign Policy Crises: Presidents, Advisers, and the Management of Decision Making. By Patrick J. Haney. Ann Arbor: University of Michigan Press, 1997. 183p. $39.50.

James M. Scott, *University of Nebraska at Kearney*

Patrick J. Haney examines "the ways that U.S. presidents since World War II have organized and managed advisory groups during foreign policy crises and the ways that these groups, so structured, performed the tasks of providing information, advice, and analysis" (p. 1). Through this examination, Haney seeks to explore the links between structure and process, guided by the proposition that the "institutional model" established by the president "ought to have empirically discernible effects on the political process of decision making" (p. 47). To undertake this highly worthwhile effort, Haney uses a "case survey" method, which employs a set of questions to "interrogate" case studies of the Berlin airlift (1948), the Korea decision (1950), Dien Bien Phu (1954), Suez (1956), the Tonkin Gulf (1964), the Tet offensive (1968), civil war in Jordan (1970), the 1973 October War, and the Panama invasion (1989). The first group of questions is designed to reveal the organizational structure used by the president, relying on the "models" described by Richard Johnson (*Managing the White House*, 1974) and Alexander George (*Presidential Decisionmaking in Foreign Policy*, 1980). The second group of questions focuses on how well or poorly the advisory groups complete what Haney conceptualizes as the "generic tasks" of decision making (surveying objectives; canvassing alternatives; searching for information; processing new information; evaluating costs, risks, and implications of the preferred choice; and developing implementation, monitoring, and contingency plans).

Haney achieves his main purposes quite well—describing how crisis decision-making groups were structured and how they performed their decision-making tasks. In the third chapter, Haney concludes, for instance, that Truman and Nixon established a formalistic model, Eisenhower established a competitive-formalistic hybrid (largely formal in structure, but competitive in informal operations), and Johnson and Bush established a collegial-formalistic hybrid (collegial in the small group of top advisors, but formalistic below this level). In the fourth chapter, Haney reports that Eisenhower's advisory group generally performed well; that Nixon's advisory structure performed well in the 1973 October War, but had mixed results in the case of the 1970 civil war in Jordan; that the Truman and Bush structures displayed a mixed performance; and that Johnson's advisory group performed poorly (until its decisions after the Tet offensive). While this descriptive information is effectively presented in the main (Haney draws on a wide range of material from the numerous case studies on which he bases his analysis), *Organizing for Foreign Policy Crises* would be even stronger if Haney had included more in the way of description and detail about the nature and dynamics of the structures and processes in these crises, perhaps in place of some of the extensive literature and methodological reviews.

In addition, Haney advances the conceptualization of organizational structure and process, raises a number of interesting implications and suggestions for further research into crisis decision making, and offers a methodological contribution. Through his examination of structures, he provides insights on the need to go beyond the formal, competitive, and collegial ideal types, suggesting refinement and further specification to include additional, hybrid types. His conceptualization of procedural tasks establishes a useful benchmark for examining decisions and the performance of advisory groups and offers an interesting and potentially fruitful avenue to expand our understanding of success and failure in decision making. Among the several implications and suggestions Haney derives from his analysis, his calls for study into the issues of crisis definition and construction are particularly useful. Methodologically, Haney's case survey approach represents a constructive way to harness existing, often atheoretical, single case studies to comparative, theoretical analysis. While his method is necessarily limited by the quality and focus of existing cases, Haney relies on multiple cases for each crisis, which strengthens his implementation considerably.

Haney is more tentative in his discussion of the link between (certain) structures and (certain) processes, which is probably the most theoretically significant aspect of the book. In general, Haney concludes that there is "no obvious, direct relationship between a particular advisory structure and the decision process that results" (p. 125). He does find, however, that the more collegial structures (Johnson and Bush) seem to be less effective in completing the procedural tasks well, and that the actual results for each organizational structure frequently fail to match what might have been expected, given the nature of each different model.

Here, some limitations to the analysis may be noted. As Haney indicates, all the presidents in question used the formalistic structure or some variation. Clearly, one area for future work would be to expand this limited variation by including crises from presidents who employed more purely collegial or competitive models. This might help identify a more obvious pattern in the relations between structure and process. Even in the evidence Haney has collected, the relationship may be more patterned than he indicates. The author presents a clear and cogent discussion of the results for each of the six procedural tasks in each individual crisis (see his summary tables 4, p. 86, and 6, p. 126), exploring how each advisory group performed. Organizing this data by organizational structure instead of crisis (i.e., procedural tasks—performed well or poorly—according to organizational structure used) would have allowed him to suggest that, among other things: (1) the formal and competitive-formal models seem more likely to complete the tasks well, and the competitive-formal hybrid seems much less likely to fail at these tasks; (2) the formal model does the first three tasks well more often than poorly, and the last three tasks poorly more often than well; and (3) the collegial-formal hybrid seems just as likely to complete the procedural tasks poorly as well. Hence, Haney may be too modest in his discussion of the links between structure and process that emerge from his analysis.

This notwithstanding, *Organizing for Foreign Policy Crises* suggests that factors other than structure are at least as important, and probably more so, in determining how well or poorly advisory groups perform certain tasks. For instance, Johnson purportedly used the same model in each of the crises examined, but it worked in the decisions after the Tet offensive and "failed" in those before. In addition, Haney's analysis shows that the formal model used by Nixon worked better when Nixon was not engaged and attentive (October War) than when he was (civil war in Jordan). The formal model also seems to have worked better under Nixon (or Kissinger) than under Truman, and the formal collegial model worked better under Bush than under Johnson. In the end, one is left with the unavoidable conclusion that such factors as the personality, style, and interest of the president and individual advisors matter most in the outcomes. As Haney concludes, presidential attention to management and process may be the critical factor (p. 130). Given this overall

finding, Haney could have devoted more attention to these explanations for the variation in the way procedural tasks were performed.

In the end, *Organizing for Foreign Policy Crises* is an interesting, useful, and provocative work that makes solid methodological, descriptive, and conceptual contributions, even if its contribution on the theoretical link between structures and processes is more limited. As Haney suggests, the presidency is "an institution that is intimately linked with its inhabitant" (p. 145), which perhaps offers direction to future explorations of crisis, structure, and process.

Executive Governance: Presidential Administrations and Policy Change in the Federal Bureaucracy. By Cornell G. Hooton. Armonk, NY: M.E. Sharpe, 1997. 274p. $63.95.

Francis E. Rourke, *Johns Hopkins University*

This book challenges the principal explanations offered in the past for the rift that commonly divides a new president and his political aides from the career officials in the federal bureaucracy, as the two groups go about their joint efforts to put the nuts and bolts of policy together in the wake of a national election. Cornell Hooton believes too much of the earlier research on this conflict has a "top-down" perspective. It has examined this source of friction within the American policymaking system from the point of view of the president and those he has selected for service either at the White House or in one of the executive agencies. It has paid very little attention to the attitudes and activities of career employees in the ordinary bureaus of government, upon whose energy and skill the success of all presidential programs ultimately depends.

In an effort to correct this oversight, Hooton investigates the role of career bureaucrats in the development of transportation policy from the mid-1970s to the early 1980s. This was an area in which the incumbent president, Jimmy Carter, had strong personal preferences, and his views did not always coincide with those of senior career officials in the two transportation agencies on which Hooton's research focuses: the Urban Mass Transit Administration and the Federal Highway Administration. (Eventually, the study includes a third bureau within its orbit, the Food and Nutrition Service at the Department of Agriculture.)

A strong feature of the book is the light it throws on the operational styles or working procedures that prevail in government bureaus as they go about the difficult task of converting the plans proposed by political leaders into the programs needed to achieve these objectives. Hooton contends that much of what may appear to be defiance by bureau personnel of directives from the president or his political appointees may simply reflect the fact that the bureau's standard operating procedures do not easily lend themselves to pursuit of the policy objectives the administration is proposing.

As Hooton himself puts it: "This study argues that many of the difficulties of changing practice are attributable not simply or even primarily to the personal dispositions of career officials but rather to the normal characteristics of organizational behavior.... The existing training, expertise, and task orientations within a bureau favor certain policy options and slow the adoption of others" (p. 160). He contends that "a variety of attitudes, anecdotes and testimony suggests a willingness among career officials to support presidential administrations' policy directions" (p. 4). Moreover, it may be difficult "for an agency to change policy even when the career staff approves of the change" (p. 5).

Hooton believes that the attitude of the bureaucracies he examined toward administration policy was to some extent shaped by the compatibility of the proposals with the agency's standard operating procedures. This analysis is reminiscent of Graham Allison's celebrated study of the Cuban missile crisis in 1962, which held that policy decisions cannot be fully understood without taking into account the operational routines of the organizations principally involved in their formulation or execution. More recent events, such as the tactics of the FBI when it sought to arrest leaders of a religious cult at Waco, Texas, have given new life to the utility of this kind of analysis in explaining some types of agency behavior. The standard procedures of the FBI seemed ill-suited to attain even the agency's objectives in the Waco case.

Hooton's analysis of bureaucratic behavior is certainly a very useful supplement to other explanations offered for the troubled relationship between political appointees and career officials in the development of national policy. It does not, however, discredit alternative paradigms. It is clear, for example, that the differing roles played by these two sets of officials in the policymaking process can also engender considerable friction in their relationship, even if the bureau's standard operating procedures are in perfect harmony with the president's policy proposals.

For example, the very different time frames within which career bureaucrats and political appointees work in dealing with policy issues has often been a source of irritation for both sides. Presidents and their aides are constantly in a hurry, given the deadlines imposed by the political calendar. At the beginning of their tenure, a fast and successful start is needed to take advantage of the honeymoon period. At that point the White House can normally expect to benefit from a great deal of public good-will. This is also a time when constituencies whose support helped elect the president are pressing for quick action on policies they favor. Meanwhile, career bureaucrats may still be struggling with unsolved problems from the past.

Another source of division between the two groups is the fact that the presidential aides with whom career bureaucrats ordinarily deal report to one principal, the president himself, while bureaucrats also are responsible to numerous other principals, including the courts, the clientele groups that have a stake in the agency's decisions, and the congressional committees that oversee an agency's activities and supply it with appropriations. Hooton provides evidence that the obstacles to a harmonious relationship between the bureaucratic and the political sectors of government in this country are even more formidable than previously imagined.

The Manipulation of the American Voter: Political Campaign Commercials. By Karen S. Johnson-Cartee and Gary A. Copeland. Westport, CT: Praeger. 1997. 232p. $55.00.

Karen M. Kedrowski, *Winthrop University*

Johnson-Cartee and Copeland attempt an important task. Their purpose is to bring together the disparate literatures of political science, political communication, and production to analyze political campaign commercials. Their point of departure is that each of these disciplines has unique insights that should be brought together in one volume for the use of interested scholars in each field. Interestingly, both authors are academics who have also worked as professional political consultants—making some of the very "polispots" (their term for political campaign commercials) that they analyze here.

I looked forward to reading the book, especially given the practical experience that the authors brought to the table, but

I was disappointed. The authors state that their book provides a "narrative analysis" (p. xxi). "Narrative" is accurate. Each chapter draws upon an exhaustive amount of literature. There is endless discussion of the different types of political ads and their intended purpose. Several chapters have elaborate typologies of ads. For example, the authors developed six types of positive ads; three modes of negative ads; two types of inoculation ads and eight types of negative reactive responses; fifteen types of "American myths" used in ads; twenty-one types of "persuasive appeals"; and sixteen different styles. There are transcripts of ninety-seven polispots to illustrate the categories.

The problem enters with the definition of "analysis." Certainly, constructing typologies is a form of analysis, but there is no attempt to go beyond these elaborate, lengthy, and at times complex constructions. For instance, the authors provide no systematic analysis of ads to determine which types are used most frequently and which are most effective.

Similarly, Johnson-Cartee and Copeland repeatedly discuss the intended purpose of the various polispots and televisual techniques but do not indicate how they reached these conclusions; presumably they are the authors' own impressions, rather than the product of focus groups, systematic content analysis, or some other method. Likewise, the book contains no interviews with the political consultants who produced the polispots to see if their intended purpose was the same as the authors' conclusions. It is certainly possible that a political consultant meant to send a message that was not perceived in the intended way by the audience, or by the authors. In one instance, the authors even intimate such a possibility (p. 132). There is no attempt to analyze the ads from this perspective.

There is very little discussion of the extensive political science literature on whether exposure to political campaign commercials makes a difference in vote choice or the decision to vote. Given the title of the book, a chapter demonstrating that ads do indeed "manipulate" seems necessary. Instead, only occasionally do Johnson-Cartee and Copeland remark in passing, and often without citation, that airing a particular ad may have affected public opinion polls or increased candidate name recognition.

The book lacks any sort of overarching conclusions about the diverse and extensive literature surveyed. The narrative analysis produces a lot of trees, but the authors do not help the reader visualize a forest. Similarly, a concluding chapter could have been used to make some sort of normative statement about the implications of manipulating voters through political ads. Obviously, Johnson-Cartee and Copeland (and many candidates for office) believe that polispots have some effect on voters. In fact, many politicians believe that the enormous expense of producing commercials and purchasing advertising time is a major factor in the high cost of today's political campaigns, which arguably keeps many qualified potential candidates from running. Yet, the authors fail to offer any opinions or conclusions about the appropriateness of polispots in today's political discourse. Instead, this book stops rather abruptly at the end of the last substantive chapter.

Nonetheless, the book is not without merit. In the chapter on negative ads, Johnson-Cartee and Copeland make the point that these have become synonymous with "mudslinging" in the mind of the public and academics. They argue that critical attack of opponents is a legitimate political strategy that can educate voters about important facts. Thus, "negative" ads should not be considered illegitimate or misleading. Their point is well taken, and their discussion is well worth reading. Similarly, the chapter on the power of recorded visual images, which focuses on camera angles and compositional techniques used by political consultants to create different effects is excellent. Political scientists are unlikely to be familiar with this literature, and the chapter summarizes the material nicely.

In sum, the book has some value as an exhaustive literature review, but it makes no new contributions and does not attempt to draw any conclusions from existing information.

Budgeting Democracy: State Building and Citizenship in America, 1890–1928. By Jonathan Kahn. Ithaca, NY: Cornell University Press, 1997. 222p. $42.50.

Charles Stewart III, *Massachusetts Institute of Technology*

For the same reasons a fish does not know it is wet, the modern American political observer is probably unaware that politics at all levels is enveloped in the budgetary reform baggage left over from the turn of the century. The value of Jonathan Kahn's *Budgeting Democracy* is its careful recounting of the process that transformed how Americans perceived government taxing and spending in the early twentieth century, which in turn altered how policy conflicts large and small have played out over the past century.

The general story of how the federal government got a budget is fairly well known among students of American national institutions: The Taft Commission begat a congressional reform effort, which begat the Budget and Accounting Act of 1921, which begat the president's budget. Before 1921, federal agencies made their spending appeals directly to congressional legislative committees, bypassing the president (and often the Appropriations Committee, too). Taxes were not even mentioned when appropriations requests were made. After 1921, all appropriations requests were channeled through the president (who actually had the power to change them), who made a set of revenue estimates that were reported to a single committee in each chamber of Congress. Both the Congress and the executive branch received new auditing capacities. An appropriations process was replaced with the budget process. Presumably, appropriations politics were replaced with budget politics.

Kahn's examination of turn-of-the-century budget reform pushes the empirical case back before the well-known national reform effort by focusing on the earlier movement to reform municipal budgeting. Here, the history is longer, more varied, and in many ways more interesting. Young reformers, imbued with a moral dislike of machine politics, armed with knowledge of the modern management practices that were overtaking large industrial concerns, and confident in an ideology that focused accountability in the hands of a few well-trained experts, whittled away at the standard government practices of the big cities, especially New York.

The focal point of this reform effort was the New York Bureau of Municipal Research, which served as the holding company of some of the best-known personalities of early-twentieth-century reform, such as Frederick Cleveland, William Allen, and Henry Bruere. Within the orbit of the bureau were such important academics as E.R.A. Seligman and Charles Beard. The Bureau of Municipal Research was largely successful in altering the fiscal institutions of the city through a combination of sheer energy, managerial talent, and public relations savvy. On this latter point, we have some of the most entertaining reading in the book. The bureau began sponsoring public exhibitions to publicize and illus-

trate the issues at stake in the annual New York City budget consideration. When the city took over the enterprise in 1911 (learning from successes of previous bureau exhibitions), it rented an old dry-goods warehouse and filled it with charts, models, graphs, multicolored cubes, and pictures to bring the city budget to life. Seeking to include the whole family, Brentwood, "a twenty-one-year-old horse that had served the fire department faithfully (and, of course, efficiently) for seventeen years... was tethered outside, in front of the building" (p. 108), to the delight of city children.

It was the experience with budgeting in New York—both the politics of creating a budget for the first time and the mechanics of implementation—that made the leap from New York to Washington inevitable. Keep in mind that during the early years of the century, the scale of New York City's budget and the federal budget were not wildly different, so success in one realm was an obvious predictor of success in the other. Grover Cleveland, particularly, was detailed to the Taft Commission, which provided the intellectual ammunition for the federal reform effort of the 1910s and, ultimately, the momentum for the passage of the federal Budget and Accounting Act.

Political scientists trying to understand American "state-building" will find a plethora of information to help round out the account of how the U.S. government acquired the trappings of the modern state early in this century. The book provides a much-needed corrective to the current literature, which has focused on the building of governing capacity in national institutions. Kahn makes a convincing demonstration that if you were a politically aware American in 1910, you would be much more interested in developing capacity at the local level, where things actually happened, rather than in sleepy old Washington. Tip O'Neill's aphorism, "all politics is local," turns out to apply to national budget reform as well.

This is a helpful book, though it is not as thorough as it might have been. Two important arguments remain underdeveloped. First, running throughout the book is an explicit criticism of the model of representation implied in the reformers' efforts, coupled with a claim that the real import of municipal reform was the shifting of government benefits away from the working class and recent immigrants toward the urban middle and upper classes. While this latter claim may be true, it is weakly supported by the evidence in the book. Certainly, the New York City budget reforms had real distributional consequences for residents and interests residing in the city, but no systematic evidence is adduced to examine these assumed distributional consequences.

Second, a not-so-subtle conspiratorial ingredient is added to spice up the narrative stew. Budgets are claimed to have "eased the crisis of representative democracy by creating a new myth of representation which secured the acquiescence of citizens in the increased exercise of governmental power over their lives without supplying them with any more real control over that government" (p. 5). Americans have acquired a "fixation" on budgets, which has created a new crisis of representation today.

However one may feel about these normative claims, very little in the book sustains the point one way or the other. Representation is a complex thing, and simply showing that one set of reformers succeeded in pushing the emphasis along one dimension (and may have had designs to push it even farther) is a long way from demonstrating that democracy is undermined when the capacity for synoptic planning is expanded.

A major theme in the literature on American state-building is that the case for U.S. exceptionalism rests on the highly fragmentary nature of American politics. The fault line usually examined is the one that separates national governing institutions. The contribution of *Budgeting Democracy* comes in exposing the fault line that emerges because of federalism. That alone makes this book a valuable addition to the canon of American reform politics.

Liberty, Property, and Privacy: Toward a Jurisprudence of Substantive Due Process. By Edward Keynes. University Park: Pennsylvania State University Press, 1996. 238p. $40.00 cloth, $16.95 paper.

Keith E. Whittington, *Princeton University*

This is a work of advocacy, but advocacy of a peculiar kind. The author is ultimately concerned with defending a version of substantive due process and with using that reconstituted notion of due process to criticize and reformulate recent decisions by the Supreme Court. Keynes pursues this point, however, through an extended history of historical reasoning, legislative debate, and jurisprudential rulings on the meaning of the due process clause and its precursors. This approach provides a welcome relief from competing polemics attacking and defending recent judicial rulings, and it is a useful history that will be of interest to many with no particular stake in current doctrinal jousting. The book's argumentative goals emerge and recede again as the historical evolution of due process understandings is traced. The result is an interesting and highly readable history of substantive due process in the national government, but one that may not be very likely to convince readers to change their views on how the clause should be interpreted in the current context.

The book traces the evolution of thinking about the cluster of concerns surrounding due process from the colonial period through the Court's recent abortion decisions. Keynes hopes to show that "due process includes a substantive as well as a procedural component" and that the substantive component is not confined to a narrow set of "specific liberty or property interests" (p. x). In developing this substantive component, Keynes argues that those rights are best conceptualized as conditional rather than absolute and as rooted in fairly concrete historical sources and not simply in abstract moral reasoning. He finds support for these contentions in a variety of sources, ranging from historical legal commentaries, to constitutional texts, to judicial doctrines, to the debates of constitutional drafters. Keynes works through several chronological periods—from before the Civil War, through Reconstruction and the drafting of the Fourteenth Amendment, through the rise, fall, and second rise of substantive due process jurisprudence. In overviewing each period, Keynes is concerned both with establishing his central concerns using the historical evidence and with arbitrating among the numerous scholars who have examined this material.

Keynes breaks little new ground in examining the historical material, but his discussions are clear, full, and well written. As an introduction to substantive due process and the debates over due process, the books succeeds admirably. Because this is ultimately a work of advocacy, however, Keynes sometimes seems to stretch to make his points, often unnecessarily. For example, due process language is too often identified with constitutionalism more broadly. Keynes blurs important distinctions in claiming that, because early American constitutions both recognized "substantive limits on the power of government" and included due process language, the due process clauses were therefore substantive guarantees and not simply procedural (pp. 18–19). That the

founders undoubtedly intended to protect life, liberty, and property did not necessarily mean that they regarded the due process clause as more than a procedural guarantee. Similarly, his post–Civil War analysis elides distinctions between equal protection and due process concerns, often underestimating the independent significance of the former in order to establish or emphasize the substantive component of the latter.

Such quibbles with the historical presentation are not crucial to evaluating the normative goals of the book. The motivating concern of the work is with contemporary jurisprudence, and Keynes hopes to persuade his readers that the modern Court has adopted a "fundamentally flawed" approach to thinking about rights and the powers of the state (p. xii). Central to those modern flaws is the fact that judges are forced "to act like legislators" in employing modern doctrine, while often producing incoherent or misleading opinions to rationalize outcomes (p. xii). The original substantive due process logic of the Reconstruction Republicans and the *Lochner*-era judiciary provides a compelling alternative, in Keynes's account. As many recent revisionist studies have concluded, the *Lochner* Court was far more principled and far more restrained than is commonly believed. The multifaceted reasoning of the first wave of substantive due process jurisprudence, rooted in a common law tradition and the concrete historical intentions of the framers of the Fourteenth Amendment, provided an exemplar of principled, coherent, and sustainable judicial protection of individual liberty.

The historical approach is not well suited to establishing this normative case, and the necessary normative arguments are never extensively developed. Readers are left to their own devices in answering key questions regarding the viability and desirability of Keynes's normative project. Why should we be particularly concerned with the state of recent due process jurisprudence? Even if the framers of the Fourteenth Amendment had substantive liberty commitments derived from common law, natural law, and social contract theorizing, why should we be either guided by or constrained by their particular understandings of either the sources or extent of individual liberty? Such fundamental questions about the status of judicial review and constitutional interpretation could perhaps be laid aside in order to follow Keynes's interpretive recommendations, but even here the argument is frustratingly partial. What should we make of the fact that substantive due process doctrine was being developed in dissenting opinions in the immediate postbellum era and was only embraced by the Court decades after the Reconstruction-era thinking that presumably determined its meaning? Given that the substantive due process commitments during Reconstruction were specifically economic, how should that logic be applied in the modern context? What survives of the animus against "class" legislation, and how should that idea be applied in the context of government regulation of sexual activities? How much deference should be shown to legislation that has some connection to a legitimate state power but serves primarily to advance the interests of some citizens at the expense of others? The author's discussion is not helpful in answering such questions, not only because they are largely left unexamined but also because they are often implicitly waved away. The book is filled with assertions that various pieces of legislation were simply "irrational" or "arbitrary" (e.g., pp. 168, 173, 178). Not only are such claims usually far from obvious, but also they hide whatever reasoning might go into rendering such decisions. Labels of that sort often substitute for analysis and do not build confidence in how such a jurisprudence would improve either the Court's reasoning or its political standing.

This book is a useful introduction to the evolution of the due process clause and its interpretation over the course of American history. It will take quite a bit more work, however, to move us "toward a jurisprudence of substantive due process," as the book's subtitle promises.

Turf Wars: How Congressional Committees Claim Jurisdiction. By David C. King. Chicago: University of Chicago Press, 1997. 207p. $34.00 cloth, $15.95 paper.

Bryan D. Jones, *University of Washington*

In graduate school, I swore that I would NEVER, but NEVER, study two topics: urban services ("count manhole covers") and congressional committees. Having done both, I plead that things changed: Both were far more interesting than I thought, and both illuminated fundamental processes of governance while still remaining empirically tractable. David C. King's book will ensure that no graduate student will again swear not to study congressional committees, at least. He not only has produced a seminal work on the committee system but also has done it in a way that makes the whole process exciting and interesting to anyone with any interest at all in Congress.

I want to argue, however, that King's book is worthy of study not just by students of Congress or American national institutions. It ought to be examined generally by political scientists because of the processes of democratic governance that it illuminates. To make this case, I point to four reasons this is a critically important book on Congress.

First, *Turf Wars* is above all a study of the dynamics of jurisdictions. King argues that jurisdictions have both a statutory base (assigned by chamber rules—Rule X in the case of the House) and a common-law base (how the Office of the Parliamentarian refers bills to committees). Common law jurisdictions are necessary because the issues that face Congress are not fixed, and committee entrepreneurs have motive to claim new "property rights" as they emerge. As a consequence, committee jurisdictions are not fixed; they evolve in response to the emergence of issues and the activities of congressional entrepreneurs. Most jurisdictional differences, King claims, occur when new issues emerge; there is very little conflict over established jurisdictions. But, viewing jurisdictional rules as "dynamic or adaptive documents" (p. 4) over extended periods means that great changes in jurisdictional arrangements occur. Moreover, statutory changes generally ratify already accomplished common-law alterations.

King goes on to detail the strategies that are more successful in influencing the referral of bills to committees. The discussion is not only a how-to manual for members of Congress but also illuminates the criteria used by the Parliamentarian in the referral process. The most successful strategies are directed at establishing either expertise (including interest and the willingness to commit resources) or the issue's jurisdictional proximity to the committee.

Second, *Turf Wars* is a historical and empirical study of the Office of the Parliamentarian. King's careful depiction of the House moving toward a nonpartisan officer making judgments about committee jurisdictions can give no aid and comfort to the party cartel model of Cox and McCubbins.

Third, King speaks to a major fault line in theorizing about Congress. Not content to stop with a superb analysis of jurisdictional dynamics among committees and the role of the Parliamentarian, King comments on current debate about

the function of the committee system. Does it provide an "informationally efficient" mechanism for chamber majorities to understand the effect of policies, or does it establish a mechanism for reelection-driven legislators to enjoy "gains from trade" among policy specialist committees? King sees the committee system as a collective good that can be undermined through member defection, which leads him to favor the informational viewpoint. While King's careful empirical study of the precursors of entrepreneurial activity on the new issue of mag-lev trains (chapter 6) supports the distributional model, the assignment of bills to committees seems more to conform to an informational efficiency ideal.

Fourth, the book is more than a study of Congress. It is an implicit (and occasionally explicit) argument about the evolution of governing institutions. Regardless of the "three theories of Congress" notion that permeates recent discussions, I claim that political scientists can no more have a theory of Congress than economists can have a theory of General Motors or astronomers can have a theory of Jupiter. Surely, theory is more than this. What would a general theory of governance look like? King has laid out many of the elements of an evolutionary theory of institutional change, one whose context is the tractable problem of congressional committee jurisdictions. He is not as explicit here as on the other points I have summarized, but a reader does not have to strain to find it.

The key is King's view that jurisdictional rules are "adaptive documents" (p. 4). The following are some of his key findings that speak to an evolutionary approach.

- Reforms follow, rather than proceed, the incremental evolutionary process of jurisdictional change.
- Jurisdictions are path dependent.
- The office of the Parliamentarian evolved historically to "solve" the collective goods problem of maintaining a committee system; yet, the Senate was unable or unwilling to use the same depoliticized approach.
- Members of Congress are entrepreneurial when it comes to jurisdictional expansion; they have motive to use trial-and-error strategies to claim jurisdictional advantages.
- The Parliamentarian selects among strategies that fit the "weight of the bill" rule, which refers according to jurisdictional proximity and committee expertise.

If these findings are only slightly rephrased, the following picture emerges. Most change in social institutions is through a trial-and-error with selectivity process, resulting in considerable incrementalism and little intended comprehensive change. Once a system adopts a best strategy, from among the rivals it will not move to maximize—its solutions are path dependent. While some solutions are so attractive that all social institutions will converge on them (specialization and division of labor), others are not so uniformly attractive that convergence is so uniform (the House and Senate find different solutions to the bill referral problem). There must exist in evolutionary theory a source of variation (entrepreneurial legislators). There must exist a selection mechanism (the referral system).

The only problem I have with *Turf Wars* is that there is no need to try to distinguish between distributional or informational or party cartel models of committees (although a great deal of good comes from discussing the problem in these terms). Each of these depictions represents something important about Congress—because our national legislature is an adaptive organization facing competing demands. So long as an adaptive, evolving organization faces multiple demands, its solutions, as represented by its committee structure, will be an evolving compromise—albeit with different salient aspects in different eras (score one for Rhode).

David C. King has written the seminal work on the committee structure in Congress, a book that will be consulted as the standard work for years to come. It should be evident from this review that I think it is far more: *Turf Wars* is a template for the study of formal institutions in a complex society.

The Days of Wine and Roses Are Over. By Daniel C. Kramer. Lanham, MD, and London: University Press of America, 1997. 340p. $64.50 cloth, $37.50 paper.

Jeffrey Kraus, *Wagner College*

Kramer offers us a biography of Hugh Carey, a governor who had a significant influence on the direction taken by New York City and state in the last quarter-century, but whose administration was overshadowed by that of his successor, Mario Cuomo. As Kramer notes in the preface, only one other book has been written about the Carey era (1975–83) in the fifteen years since the governor left office.

The book's central thesis is aptly summarized by the title of the final chapter, "A Rolls Royce Engine in a Studebaker Body." As Kramer notes, Carey "did more than anyone else to save the City of New York from bankruptcy in 1975" (p. 316). Yet, this was the same man who responded to concerns about PCB contamination in a state office building by offering to drink the water there. While Kramer concludes that Carey was an outstanding governor, his stewardship was affected by what Gerald Benjamin describes as his "disregard of public image" ("The Carey Governorship," in *Making Experience Count: Managing Modern New York in the Carey Era*, ed. Gerald Benjamin and Norman Hurd, 1985, p. 251) and by Carey's lack of interpersonal skills.

The book begins with a brief discussion of Carey's formative years. We learn of the family troubles during the Great Depression and how those experiences later helped convince the governor that New York City had to remain solvent during its fiscal crisis in the mid-1970s. As a young officer during World War II, Carey helped liberate the Nordausen concentration camp. According to Kramer, this experience "bred in him a loathing of the use of violence and killing to solve political and social problems that has lasted to this day" (p. 7). This may help explain Carey's opposition to the death penalty.

Sixteen chapters then cover Carey's first campaign for governor and various aspects of his administration. Four of these are devoted to the administration's efforts to keep the city and state from insolvency between 1975 and 1978. Much of this material has been examined elsewhere in both the popular and scholarly press (Ken Auletta, *The Streets Were Paved with Gold*, 1979; Robert Bailey, *The Crisis Regime*, 1984; Fred Ferretti, *The Year the Big Apple Went Bust*, 1976; Charles Morris, *The Cost of Good Intentions*, 1980; Martin Shefter, *Political Crisis/Fiscal Crisis*, 1987; and William K. Tabb, *The Long Default*, 1982). Kramer's analysis is distinguished by the significance he places on Carey's years in Congress (1961–75).

Kramer presents the position that Carey was particularly successful in securing federal support for a New York bailout (despite the blaring *Daily News* headline: "Ford to City: Drop Dead") because of his ability to present New York's case in Washington (in testimony before various congressional committees, Carey explained that the city's fiscal crisis was threatening the state's solvency and impairing the efforts of other states to secure money in capital markets). Carey's personal relationship with many members also proved important. As Kramer ex-

plains, "the Governor's activities in the House and Senate did not consist merely of speaking before committees. He made use of his network of friendships in the House among members of both parties, and among conservatives as well as liberals to try to develop a majority in that body for loan insurance" (pp. 69–70). Carey ultimately prevailed, and New York City did emerge from its fiscal crisis.

Various chapters deal with Carey's relationship to the Democratic Party (not very good) and the state legislature (at times even worse); his reelection campaign; and economic development, transportation, environmental, health, and criminal justice policies.

One of the last chapters covers with what Kramer calls Carey's "Long Winter of Discontent" (fall 1980–fall 1981), when "several episodes convinced many New Yorkers that their Governor was a flake" (p. 276). Some of these mistakes were political, such as his support for Bess Myerson for the U.S. Senate in 1980 because he regarded the seat as the "Jewish" seat (p. 276) or his lukewarm support for President Carter's reelection. Other errors were more personal in nature. His attempt to condemn the property of a Shelter Island neighbor for "security reasons" failed but resulted in Carey being savaged in the press. His sudden decision to dye his hair and his marriage to Evangeline Gouletas, about whom there was considerable confusion concerning the number and whereabouts of her previous spouses, brought derision. A chapter on the Governor's management skills and a concluding chapter round out the work.

The book's title comes from Carey's first State of the State Address. After two decades of profligate spending under Nelson Rockefeller, New York would have to live within it means. In view of what would happen in the months and years to come, Carey's words were prescient.

Hugh Carey would be the first chief executive to grapple with the role of government in a time of diminishing resources. In contrast with his Republican predecessors, he proved to be something of a fiscal conservative, presiding over a cut in personal income taxes and slowing the growth rate of state spending. Carey would also play a significant part in changing the state's mental health policy; the practice of "warehousing" the mentally retarded in large congregate facilities ended when Geraldo Rivera exposed abuses at the Willowbrook School. Carey moved the state toward the more humane practice of placing only the most seriously ill in psychiatric centers and locating the overwhelming majority of the state's charges in group residences and other less restrictive environments. Under Carey, New York became the national leader in the deinstitutionalization movement.

Kramer's work benefits from interviews with Carey and more than 125 of his contemporaries. The book sheds a great deal of light on a man obscured by his lieutenant governor and successor, Mario Cuomo.

The President's Call: Executive Leadership from FDR to George Bush. By Judith E. Michaels. Pittsburgh, PA: University of Pittsburgh Press, 1997. 348p. $50.00 cloth, $22.95 paper.

FDR and the Modern Presidency: Leadership and Legacy. Edited by Mark J. Rozell and William D. Pederson. Westport, CT: Praeger, 1997. 242p. $59.95.

M. Stephen Weatherford, *University of California, Santa Barbara*

Among the president's greatest and most inscrutable challenges is shaping the bureaucracy to carry out the administration's goals. Many heads of large organizations face similar challenges, but the context of public accountability, media coverage, and especially the competition inherent in the separation of powers multiplies the difficulty—and arguably transforms it qualitatively—for the world's most visible leader. These books offer perspectives on presidential control of the bureaucracy from two promising vantages, at the beginning of the modern presidency's growth and at the end of the Reagan-Bush era, which marked the interment of the New Deal belief that activist government could make things better.

FDR and the Modern Presidency brings together papers from a conference held at Louisiana State University, Shreveport, in 1995, on the fiftieth anniversary of Franklin Roosevelt's death. Although the modern presidency did not begin with the New Deal, Roosevelt's task was less to control an established bureaucracy than to develop the government's capacities, its sources of information and levers of implementation. The normative issues were clearer and the opportunities for experimentation freer than for later presidents, and the ideas and arguments—over what the government should be able to do, what sorts of institutions should be constructed, and what qualities of individuals should be recruited—had a clarity and open-endedness that they would never have again.

The authors of the eleven papers are historians and sociologists as well as political scientists. They offer a wide variety of vignettes from the Roosevelt years. Like snapshots chosen randomly from a very large album, most capture some telling little truth that leads one to want to find out more about the characters in the picture and the events that brought them together. For instance, Martin Halpern tells the fascinating story of the meeting, in the depths of the 1938 recession, between FDR and Henry Ford, icons in an explosive national debate in which one side feared that the government was bent on socialism and the other believed that the propertied classes were planning to withdraw finance capital from the economy. William Ellis traces the troubled ambassadorship of Robert Worth Bingham in England, showing that Bingham's internationalism allowed the administration to express positions for which the isolationist American public was not yet ready. And yet, Ellis argues, the relation between FDR and Bingham was much more complex than the conventional view of a president who manipulated subordinates for his own purposes. Allida Black's chapter on Eleanor Roosevelt's efforts—inside the White House and out in the country—to advance the cause of civil rights brings out both the limits to Mrs. Roosevelt's influence and her own ambivalence when it came to applying the same principles to the Japanese internment that she advocated for Negroes in the South.

If these chapters lead toward the wider literature on the people around Franklin Roosevelt, others whet the appetite for more extended expositions of the events and their interpretation. For example, Margaret Rung looks at the way legitimate claims to bureaucratic authority developed as government grew, highlighting the tension between college-based expertise (with its strong class bias in the 1930s) and the promise of upward mobility for long-time civil servants who had worked hard for promotion. By the late 1930s, personnel specialists had developed a body of professional knowledge that helped to transform this "political" debate into an administrative one, first toward technical questions about job classification and then toward issues of human relations and program development. Michael Lewis places the early history of public works during the New Deal into the perspective of state autonomy, arguing that the initial administrative vehicle, the Civil Works Administration, was scuttled within a year of its formation, in essence because of its

success as an autonomous innovation independent of societal groups. The mandate and organization of the successor agency, the WPA, not only was circumscribed by the influence of business interests and state governments but also reflected the delicate balance of forces within the developing profession of social work, between case workers focusing on the needs of individuals and social engineers who saw unemployment as the inefficient use of economic resources. Matthew Dickinson describes FDR's advisory system of "competitive adhocracy," arguing that it is vitally relevant to current debates over the administrative presidency, because the system is at heart antithetical to the operation of a "presidential branch" narrowly centered on the White House.

The entries inevitably vary in quality, but the collection as a whole will stimulate students' interest in the Roosevelt years. At the same time, experts on the New Deal will find few of the chapters completely convincing interpretations: Their brief, staccato pace allows little opportunity for nuance, a dangerous failing in writing on the richly historied New Deal.

The President's Call carries forward the story of the administrative presidency, placing a survey of President Bush's Senate-confirmed political appointees (PASs) into the context of postwar presidents' use of the appointment power, and focusing especially on the similarities and differences between those of Bush and Reagan. Although Michaels initially intended to replicate earlier polls of upper-level bureaucrats carried out by the National Academy of Public Administration, the study itself ran into such a flurry of melodramatic perils that it could serve as a cautionary tale for those who suppose that a questionnaire is all one needs to get at the truth. In spite of these problems, the descriptive portrait is of considerable interest, and the historical summary, especially of initiatives under Nixon and Reagan, provides a telling perspective.

Momentum, as candidate Bush found, is an unreliable ally; Michaels ran into similar reversals. She was awarded a doctoral research fellowship at the General Accounting Office, and with the cooperation of NAPA, her project appeared to be well underway. The bureaucracy being a place of many vetoes and no imprimaturs, however, each step needed to be negotiated, and each negotiation cost a few hostages. The GAO was unhappy with NAPA's questions, and modifying them undermined the goal of replication. The White House was skeptical of GAO's intentions and refused to sanction the survey, leaving an atmosphere of suspiciousness even when the list of PASs was finally released. And the list was out of date anyway, making it hard not only to locate all 600 or so current PASs but also to compare the respondents (response rate ≅ 38%) to the population. Michaels followed up the mail questionnaire with personal interviews of about three dozen respondents, and this lends additional detail. It is difficult, however, not to be skeptical of the validity and generalizability of the results, and the analysis does little to dispell such concern. Between what appears to have been GAO's principled opposition to anything more revealing than running the marginals, and Michaels's own aversion to moving beyond description, neither validity analysis nor causal hypotheses come in for sustained attention. For instance, the criteria for including or deleting individual respondents are unclear; response categories do not appear to be unbiased samplings of the universe of content, and opinion responses are not anchored in any uniform standard, especially on questions regarding job satisfaction or reasons for leaving government service; and the absence of multivariate analysis invites mistaken attributions of causal influence.

Much can be learned from good description, however, and this is especially the case with the political stratum of the bureaucracy, which remains an aspect of the American political system about which our understanding is sorely incomplete. Michaels confirms what previous studies (four NAPA reports during the 1980s, the Volcker Commission report in 1989) have revealed: Morale is low among both political appointees and careerists, and the proliferation of political appointees and the increasing emphasis on ideological loyalty have slighted both administrative talent and subject-area expertise. The confirmation process and the budget process continue to be foci of complaint by PASs, and suggestions for reform are constructive and sensitive to the public, political context; these are not simply business leaders bent on transposing private sector hierarchies to public administration. At the same time, there are some notable surprises. In spite of Bush's rhetorical commitment to the ideal of unbiased public service, his appointees did not differ much from those of his predecessor: They were Bush loyalists rather than Reagan loyalists, but their background of government service, organizational experience, or substantive expertise was equally light. At the same time, once PASs gained some experience or substantive expertise was equally light. At the same time, once PASs gained some experience of working with senior civil servants, they evaluated the careerists as skilled and reliable, not at all the political obstructionists they had expected. And in contrast to Heclo's famous aphorism, after ten years or so of Republican presidents, the bureaucracy was no longer a government of strangers but a coterie of colleagues with similar backgrounds and political beliefs.

The wealth of descriptive data on Bush appointees is placed into perspective by the author's review of the history of attempts to craft an effective administrative presidency. Previous researchers—Nathan, Pfiffner, Aberbach, and Rockman, among others—have told the key episodes of this story, but Michaels interweaves historical developments in light of the dynamic tension between politics and administration, between centralizing control and overloading White House decision making. In the end, the book may not quite measure up to its subtitle, but it amounts to more than a small survey study wrapped in a large box of quotations from previous studies. Indeed, Michaels makes a good case for an unbroken thread of institutional development from Nixon through Reagan and Bush, and scholars who see a secular "thickening" of the medium in which presidential power is exercised will find in this book valuable evidence for various models of presidential cycles and political time.

These two new books on public administration at the highest level of government were published at about the same time as James March's Gaus lecture to the 1997 APSA convention ("Administrative Politics, Organization Theory, and Political Philosophy: Ruminations on the *Reflections* of John M. Gaus," *PS* 30[December 1997]:689–98), and the juxtaposition suggests the context for judging their contribution. At a time when significant new theoretical approaches are contending in the larger discipline, when popular mistrust of government signals the need for political scientists to help deepen our understanding of democratic administration, March urges three "scholarly commitments." On the first, understanding the detailed context of administrative practice, these texts rate high praise. On the other two, linking the study of public administration with organization theory and with democratic political philosophy, they fall lower on the scale. We find little guidance here on questions of how public organizations deal with asymmetric information, adapt to changing environments, or develop networks into decision-making patterns. Nor do the authors grapple with questions

of accountability or legitimacy, the quality of civic discourse, or the refurbishment of social capital—more distant from daily practice to be sure, but issues to which the administration of government is surely vitally relevant.

Consider, for instance, the potential for systematic comparison along the continuum from neutral to responsive competence, where the variation across the presidencies from FDR to Bush is dramatic. How does responsive competence systematically alter the relationship between principals and agents? How does it change the conditions for successful organizational learning or adaptation? What light can now be shed on the trade-off between responsiveness to the cycle of presidential elections and the role of bureaucracies to ease the difficulty of committing to long-term goals? These questions come from what March calls the "lost tradition" of John Gaus. The research in these two books provides the sort of data needed to confront such questions, but other scholars will have to put the data to use.

Federalism and Environmental Policy: Trust and the Politics of Implementation. By Denise Scheberle. Washington, DC: Georgetown University Press, 1997. 224p. $58.00 cloth, $20.95 paper.

Susan J. Buck, *University of North Carolina, Greensboro*

This book is one in a series edited by Barry Rabe and John Tierney, American Governance and Public Policy. The series is targeted at both academic and professional audiences, and this volume fits that audience well. It addresses the increasingly important question of implementation design for environmental programs that inextricably link federal mandates, federal oversight, and state agencies. Especially noteworthy is Denise Scheberle's discussion of the perceptions and roles of federal regional officers.

The book explores working relationships between state and federal administrators in five environmental programs. Four are administered by the Environmental Protection Agency (EPA): the state drinking water and wellhead protection programs (authorized under the Safe Drinking Water Act, as amended) and the radon and asbestos programs (authorized under the Toxic Substances Control Act). The fifth is the surface mining program, authorized by 1977 legislation and administered by the Office of Surface Mining Reclamation and Enforcement in the Department of the Interior. The case studies of the five programs are thoroughly researched. Scheberle's descriptions of the enabling legislation and the development of the programs are helpful in setting the political context for ensuing federal-state relationships; her presentation of the surface mining program is particularly good.

Scheberle surveyed state directors for all five programs as well as the appropriate EPA regional officers and OSM field officers. She also conducted telephone interviews with state and federal officials. In the opening chapters, Scheberle presents two analytic models to organize her analyses: a typology of federal-state working relationships based on trust and extent of involvement, and a systems model of extrinsic and intrinsic factors that affect state implementation. The second framework is similar to the well-known implementation model of Daniel Mazmanian and Paul Sabatier (*Implementation and Public Policy*, 1983). It adds factors specific to state implementation, such as role orientations of federal oversight agency personnel and national-level political support, although it is necessarily less detailed than the Mazmanian and Sabatier model. It provides a useful set of guidelines for discussions in the case study chapters. The first model is more problematic, however.

In the first model, *trust* is defined as the intensity of the belief by one set of officials (state or federal) that their counterparts are dedicated to implementing policy effectively. *Involvement* includes formal and informal communication between the two sets of officials, the frequency and nature of oversight activities, funding, and resource sharing. Scheberle then combines these attributes to describe four types of relationships between state and federal officials. The type characterized by high trust and high involvement, described as "pulling together and synergistic," is identified as the obviously preferred relationship.

I spent a fair amount of time puzzling over this typology. First, it is not clear to me that *trust* is solely or even predominantly based on one's beliefs about another official's dedication to the task. It is much more likely to be based on one's perception of the likelihood that she will deliver what has been promised, such as funding, technical assistance, or political intervention. The most dedicated person may be unable to follow through on agreements, and an official who is indifferent to policy outcomes may have professional or practical reasons for being utterly reliable. Second, I am not convinced that the most preferred quadrant of this topology is always high trust and high involvement. I can easily envision a program in which the federal and state officials have little direct programmatic involvement with one another but still produce successful state programs; an example that comes to mind is state wildlife management programs funded through the Federal Aid to Wildlife Restoration Act (1937).

The real meat of the book comes in the final two chapters, with the respective discussions of federal regional officers and Scheberle's conclusions. The author correctly notes that the roles and perceptions of federal regional officials have been largely ignored by policy implementation scholars. Scheberle's interesting findings may well spark a new interest in regional and field officers. Bless 'em—they must frequently be miserable, caught between state officials who view them as the primary contact with the national government and its maze of mandates, and the home office, which often delivers new policies like Jove from Olympus but then retains fundamental control of funding. Scheberle's research shows that while regional staff have a lower level of confidence in state program effectiveness than do state officials, they also lack confidence in federal efforts and headquarters staff. For example, only 47% of federal regional staff think that federal implementation efforts have been effective (Table 5-3, p. 172). This dissatisfaction can be even more striking when broken out by programs: Zero percent of federal wellhead protection staff thought the federal program was effective (Table 5-4, p. 173). What might be done to improve these relationships and, presumably, the program effectiveness?

Scheberle suggests that solutions lie in increasing levels of involvement and trust. Involvement might be improved by increasing the frequency and quality of interagency (state, regional, and national) communication. Trust is more difficult to establish, but Scheberle notes that encouraging open dialogue, discouraging sub rosa communication, and focusing on policy outcomes rather than implementation means may be helpful. Unfortunately, such recommendations ignore the bureaucratic imperatives that often control agency personnel regardless of the policy being implemented. I think it is more likely that low levels of trust are a symptom of poor institutional design. Rather than seeking to increase involvement and trust, policymakers might be better served by examining the structures of incentives and disincentives for state agencies with an eye to institutional redesign.

This volume is useful in three significant areas. First, it is a scholarly examination of the important issue of federal-state relationships in environmental policy implementation. Second, the five programs chosen as case studies cover a sufficiently wide range of policy types to allow plausible generalizations about the perceptions of agency personnel at the various levels of government. Third, the discussion of regional federal officials provides a nucleus for future research on a vital area in implementation studies. This book should be of interest to administrators at the state, regional, and national level as well as to scholars concerned with policy implementation processes.

Ambition and Accommodation: How Women View Gender Relations. By Roberta S. Sigel. Chicago: University of Chicago Press, 1996. 240p. $48.00 cloth, $16.95 paper.

Linda L. Fowler, *Dartmouth College*

Roberta Sigel examines the consequences of the women's movement for the daily lives and perceptions of women. Changes in gender roles and the rapid movement of women into the work force led Sigel to ask a simple question: How do women perceive their current status in society, and what further actions are they prepared to take to realize their goals of equality? Sigel asked this question in 1985, when she conducted focus groups and opinion surveys with 650 New Jersey women and men, but her recently published analysis of the data seems both fresh and timely.

In the roughly two decades that political scientists have been polling public attitudes regarding women, they have documented changes in stereotypical thinking about women generally, probed differences in the way men and women approach political issues, and examined the propensity of the electorate to support female candidates. To my knowledge, however, no one has systematically looked at the question of discrimination toward women and asked how they feel about it.

The mid-1980s were a particularly interesting period in which to explore these attitudes, because women "no longer accepted the dominant group's conception of themselves" (p. 19); they also were experiencing a backlash orchestrated by newly mobilized religious conservatives. The rich and nuanced picture Sigel constructs from both focus groups and telephone interviews not only is compelling in its own right but also offers insight into public opinion during a key transitional period in American politics.

Using the theory of relative deprivation, Sigel looks first for a sense of grievance and then for signs that women's dissatisfaction with their lot leads to group consciousness and collective action. Her research indicates a high level of awareness on the part of women and men in her study about the "second-class status" of women and a considerable degree of discomfort among members of both sexes over its persistence in American society. Yet, the respondents show little inclination to take political action to remedy the situation.

Women as well as men, Sigel reports, are inclined to blame men rather than society for the unequal treatment they experience, but neither group seems to put much faith in collective challenges to the social order or in governmental intervention. Indeed, women appear to think it is up to them as individuals to rise above whatever biases they encounter, and they also are likely to claim that they have not personally experienced the discrimination they readily acknowledge around them. Men, not surprisingly, reveal less intensity about gender inequality, but they, too, seem to think that discrimination is the way of the world and appear more indifferent than hostile to political action on behalf of women.

Clearly, Sigel's findings suggest that awareness of inequality is not a sufficient condition for political action. She offers the view that women's participation in intimate relationships with men distinguishes them from other aggrieved groups, such as African Americans, who demonstrate much higher levels of mutual solidarity and obligation. The cost of conflict is so high, she argues, when the oppressor is also a parent, husband, lover, or father of one's children that women choose to devote their energies to pragmatic adaptation rather than challenge perceived injustice through protest or political demands. Similarly, the resistance of men in the private sphere to their perceived loss of dominance conflicts with their embrace in the public sphere of abstract principles of merit and equality. A sizable majority of both women and men, therefore, react to the trend toward greater opportunity for women with acceptance and approval but display considerable ambivalence about the next stage.

That something so deeply political as the women's movement in the 1980s should elicit so little in the way of an agenda for collective action is stunning. But Sigel is handicapped in fully exploring this seeming paradox by her decision to rely so heavily on focus group data as a source of evidence—which is relatively common now but was unusual at the time of the study's initiation. The interweaving of the two makes the book accessible to a wider audience than might otherwise be the case, but it does limit the scope of the analysis. By the time Sigel lays out her observations of the groups and presents the frequency distributions of the key survey items, she has little room left to model the statistical relationships among the variables. Given the fascinating array of attitudes she has uncovered, the reader is eager to obtain a clearer picture of how the various pieces fit together.

It is difficult to generalize from analysis of a single state. In some states the population probably would have been less supportive of women's equality than was the case in New Jersey, and in others women may have been more receptive to political mobilization. Yet, Sigel's study vividly demonstrates why political entrepreneurs are so critical in American politics in setting the national agenda. Transforming a *condition*, which one accepts as an unpleasant fact of life, into a *problem*, which one endeavors to correct, is the work of party activists and interest group advocates. Left to their own devices, women and men are inclined to make do and muddle along, regardless of their education and occupation. What Sigel documents so thoroughly is not a rational calculation among citizens to play the free rider but an absence of political imagination.

The Farrakhan Phenomenon: Race, Reaction, and the Paranoid Style in American Politics. By Robert Singh. Washington, DC: Georgetown University Press, 1997. 352p. $60.00 cloth, $17.95 paper.

Richard A. Keiser, *Carleton College*

Robert Singh has written a peculiar book. It makes lengthy and determined, but flawed, arguments for two theses for much of its first four chapters; then, the final two substantive chapters convincingly refute these two theses. The central question is how a man like Louis Farrakhan has risen to prominence and leadership in black America. Put another way, what factors transformed the leader of an obscure religious group, the Nation of Islam, into one of the most widely recognized and powerful black leaders in the country?

Singh's first thesis is that media coverage did it. In a chapter entitled "From the Margins to the Mainstream: The Rise of Louis Farrakhan," Singh argues that the first step in Farrakhan's rise was the media attention he received during Jesse Jackson's 1984 campaign for the Democratic presidential nomination. Prior to that according to the author, Farrakhan occupied a position of national obscurity and near total anonymity among whites, but after the campaign he was widely recognized across the country Farrakhan's death threats against the black journalist who reported on Jackson's "Hymietown" remark and Farrakhan's description of Adolf Hitler as a "wickedly great man" created a national controversy.

Another major step toward national prominence as a black leader, according to Singh, was Farrakhan's invitation in 1993 from the esteemed and mainstream Congressional Black Caucus (CBC) to participate in the panel "Race in America." The event was covered live by C-SPAN and received considerable attention from the news media. Undoubtedly, the coverage and the respect of the CBC helped to elevate Farrakhan's stature in the white community. The final key event was, of course, the Million Man March.

Having established the course of Farrakhan's rise Singh then asks what kind of leader he is. Singh turns to Richard Hofstadter's *The Paranoid Style in American Politics* and argues that Farrakhan demonstrates the six distinctive characteristics of this "mental disorder" (p. 152). The most noteworthy is the perception of an immense conspiracy to destroy an entire cultural way of life or political group. Farrakhan's disorder, according to Singh, emerges from his delusion that a diffuse antiblack conspiracy is responsible for the devastation of many black communities across America.

Two-thirds of the way through the book, scholars familiar with the Nation of Islam (NOI) and schooled in the literature on subnational black politics will be unsatisfied by these arguments. Undoubtedly, the Jackson campaign, the CBC invitation, and the Million Man March did bring Farrakhan onto the radar screen of white Americans and made them see him as a preeminent black leader. But that status is not achieved because of what whites think; rather, it depends on the opinion of blacks. The question that begs for an answer here is why Jackson, the CBC, and the marchers treated Farrakhan not as a marginal, eccentric, and extremist purveyor of bizarre views (as NOI leader Elijah Muhammad and his disciple Louis Farrakhan had long been seen by the vast majority of black Americans) but as a leader with support that went far beyond the NOI membership, and thereby gave the media a spectacle.

Why Jackson asked Farrakhan to join his campaign is not explained; when Farrakhan's anti-Semitism redounds negatively on Jackson, we learn only that "intrablack electoral imperatives" (p. 47) precluded Jackson from denouncing Farrakhan. Similarly, no explanation is offered for the CBC invitation or why the CBC signed an agreement with the NOI that gave it the same consultative role on legislation recently accorded to the venerated NAACP. Jackson had learned from Harold Washington's stunning victory in Chicago in 1983 that the balance was shifting in the black community; black nationalists were gradually gaining the political allegiance of an increasingly frustrated cross-class electorate, whose disillusionment with black elected officials was growing. CBC members—such as John Lewis, Kweisi Mfume, Charles Rangel, John Conyers, and Bill Gray—as well as black mayors like Andrew Young, Maynard Jackson, Tom Bradley, Wilson Goode, and even Harold Washington recognized that if they repudiated and marginalized Farrakhan they risked losing a large segment of their electoral base, not just a few misguided religious zealots.

Singh sharply shifts his argument in the last third of the book and largely rejects the thesis that media coverage catapulted Farrakhan from obscurity. At the same time, he also undermines the idea that there is something paranoid or delusional about blacks believing that whites have conspired to destroy their communities. Singh cites statistics on household income, percentage of population below the poverty line, unemployment, welfare receipt, and prison incarceration that demonstrate a lack of progress since the 1960s, and he argues that there are empirically rooted reasons for large numbers of poor Blacks agreeing with Farrakhan about a conspiracy while, middle-class blacks are drawn to the fiery leader, according to Singh, because he champions economic individualism and free-market principles. The author seems to conclude that Farrakhan is no more paranoid or deluded about conspiracies than a majority of black Americans, and he has become a prominent leader not because of the media but because his message resonates with the black community.

This book is a good introduction to Louis Farrakhan's public statements and his relationship with the more mainstream black political leadership. Yet, the Nation of Islam is not discussed in the kind of detail available elsewhere (e.g., Claude Andrew Clegg III, *An Original Man*, 1997), and the analysis of Farrakhan does not take us far beyond the insights offered by Adolph Reed ("False Prophet: The Rise of Louis Farrakhan," *The Nation* 252 (1991):51–56, 86–92).

The Rise of Baptist Republicanism. By Oran P. Smith. New York: New York University Press, 1997. 320p. $38.50.

Mark J. Rozell, *American University*

This study, a revision of the author's doctoral dissertation, is a worthy addition to the rapidly growing literature on religion and politics. Smith traces in detail the move toward fundamentalism and Republicanism among the fifteen million members of the Southern Baptist Convention (SBC) and its implications for the politics of the South and the nation. His very thorough documentation and the breadth of coverage add to the value of the study. This book is a very useful source for scholars researching just about any aspect of the subject, even though there is no deeply theoretical analysis in the end.

Smith covers a great deal of territory. He traces the evolution of the SBC through three conservative eras in this century (1920s, 1950s, 1980s), documents the internal and external events that changed the SBC culturally and politically, examines the SBC's role in the modern "culture wars" through a single-state case study, profiles the membership of the organization, and evaluates the political implications of his findings for the South and nation.

Smith is attempting to make his analysis appeal to a multidisciplinary audience, particularly in the fields of political science, sociology, and religious studies. It is a difficult feat to accomplish to the satisfaction of scholars in all three disciplines, but in fairness the topic requires one to span traditional academic boundaries and try to find some synthesis. Smith is telling the story not only of the transformation of the SBC but also of the South and the Republican Party—and the intersections among them. The book largely succeeds in finding the right balance while not reducing the coverage in any one area to a superficial level. That may be the book's strongest achievement.

In brief, Smith makes it clear that the growing conservatism of the SBC leadership and the rise of the increasingly Republican South are deeply connected, a linkage that

perhaps had not received ample attention from other writers at the time this book was published. He portrays a certain discomfort among many SBC members and, not surprisingly, former members with SBC leadership policies ranging from promotion of GOP activism to boycott of the Disney Corporation. Although Smith counsels against the organization moving too far to the right, it is difficult to escape the conclusion that the SBC became an increasingly powerful political and cultural force once it adopted a more militant and activist posture.

At times Smith writes as though the old solid Democratic South already has become the solid Republican South. Given continued but admittedly weakened Democratic control of most southern legislatures, I am tempted to say "not so fast." But electoral trends may validate his characterization in the near future. In Smith's coverage of southern politics and religion, there is a heavy emphasis on his home state of South Carolina (including an entire chapter), which has the benefit of offering a more detailed case analysis, but it is a little disappointing that the author did not make use of some other state-specific works to provide both validity for and telling contrasts to his own findings.

Such a well-researched study begs for a somewhat deeper theoretical analysis than Smith provides. He offers, for example, a list of advice for the political survival of Baptist Republicanism—also applicable to the GOP—and much of what he says is obvious. Essentially: avoid extremism and harsh rhetoric, be wary of alliances with theological/political strangers, show that movement activism brings about real achievement, and so forth. None of this is arguable, but political scientists will wish for more analysis steeped in disciplinary theory.

These problems aside, Smith has written a richly detailed, valuable study that clearly deserves a place on the shelves of scholars of southern politics and of religion and politics. The writing style is ongoing, although there is a bit too much of the thesis-style sign-posting that usually does not end up in academic books. Smith documents his work meticulously: in fact, the appendices, notes, bibliography, and index total more than 100 pages, about one-third of the book, perhaps a little too much of a good thing. He nicely illustrates findings throughout with helpful figures and tables. The type size in several tables is so absurdly small that the publisher should either enlarge them in any future printings or offer buyers a magnifying glass.

America in Black and White: One Nation, Indivisible. By Stephan Thernstrom and Abigail Thernstrom. New York: Simon & Schuster, 1997. 704p. $32.50.

Edith J. Barrett, *University of Texas at Arlington*

Much of the scholarly literature on race and race relations over the past thirty years has examined the successes and failures of the civil rights movement, the Civil Rights Act of 1965, and subsequent antidiscrimination and affirmative action legislation. Some authors have been critical of these developments, but much of the criticism has been tempered by the acceptance that some type of social policy is necessary to remedy past discrimination. In recent years the shape of the discussions has taken a different turn. Many Americans—both the intellectual elite and nonelite—are vocally turning against affirmative action, evidenced most recently by the passage of California's Proposition 209. *America in Black and White* is a polemic against all race-conscious social policy, made by two well-credentialled authors, one a professor of history at Harvard University, the other a senior fellow at the Manhattan Institute and an award-winning writer on race relations. In more than 500 pages of historical details, Stephen and Abigail Thernstrom outline why they believe America must turn away from race-targeted social policy.

According to the Thernstroms—and undoubtedly nearly everyone, black or white, agrees—race relations (read: black-white relations) in America have come a long way since Gunnar Myrdal wrote his infamous work in the 1940s. No longer must blacks drink from separate water fountains, sit at the back of the bus, or bow their heads in subservience to whites who call them by their first name. Blacks are more than three times more likely to graduate from high school now than in 1960; they are five times more likely to attend a four-year college; and they are three times more likely to consider themselves members of the middle class. Life for African Americans has improved immeasurably over the past five decades, and the Thernstroms try to make this very clear. More important to the authors, however, is their belief that racist attitudes among white Americans have diminished. Because racism is no longer a guiding force in the attitudes of whites, and because blacks are now so much better off economically, socially, and politically, the Thernstroms argue, the time for race-based public policy—if ever there was one—is no longer. The authors hold earnestly to the belief that white and black Americans can live together as a single nation, but until race is removed from our policy lexicon, it cannot happen.

American in Black and White provides a comprehensive history of racism in America from the Jim Crow South of the 1940s to the crime-ridden urban ghettos of the 1990s. The book is divided into three parts. Part I provides a history of racism in the South, offering a detailed summary of black life under Jim Crow; the changes brought about by Roosevelt's federal policies; the strides made in integration by the military during World War II; the strength and patience of civil rights activists and of Martin Luther King, Jr., in particular; and finally, according to the authors, the unraveling of the civil rights movement, epitomized by urban riots, the Black Panthers, and the separatist Nation of Islam. The purpose of this first section is to show how much has changed for African Americans and, according to the authors, how quickly it all came about. Furthermore, through their historical account, the Thernstroms wish to make clear that in only two decades many white Americans came to comprehend the backwardness of racism, and with that realization, their attitudes changed dramatically.

Part II is a summary of statistics on the economic, social, and political well-being of blacks today and in the past. The chapters present imposing evidence of the improvements that have come to African Americans. The Thernstroms contend, as well, that the significance of race has declined dramatically in much of American life. They show evidence that education and whether or not there is a male present in the household matters more than race in determining household income. White and black criminals face the same justice system, they maintain; sentences tend to be more severe for blacks because they commit more serious crimes. Black political candidates need not worry about running only in majority-minority districts, the Thernstroms contend, because minority candidates can now win in majority-white districts. While this optimistic view of race relations emerges from seemingly compelling statistical evidence, one is reminded of the duplicity of statistics. Education and marital status do explain income, but what does that tell us, when it is also true that blacks achieve lower educational attainment and are much more likely than whites to live in female-headed households? Does race still not matter? Part II is useful for the data it offers; few other single books provide so much information. The Thernstroms offer their interpretation of the statistics,

but, fortunately, readers are given enough information to be able to draw their own explanation.

Part III, the crux of the book, outlines the role of social and political institutions in shaping black America today. Three chapters are devoted to education, including past and current school desegregation policies, the use of performance testing for students and teachers, and affirmative action admissions in colleges and universities. One chapter focuses on affirmative action in the workplace and another on voting rights and gerrymandering of electoral districts. The final chapter examines the attitudes of whites and blacks toward race. Once again, the book provides useful information, albeit with the authors' particular bent.

Although it does not offer new information to the literature, the book nonetheless provides a thorough review of the historical evidence. On the surface, the Thernstroms seem committed to presenting all evidence, rather than only that which supports their argument. Some times this works, and some times it does not. For example, the authors use the improved rates in high school graduation, college enrollment, income, and unemployment to argue that life is getting better for African Americans. Yet, when they find that the rate of unwed births has been rising more quickly for whites than blacks, they comment that "the rate of change is surely not the most important point" (p. 240). Or, when expressing their disapproval of affirmative action plans in hiring and promotion, they want us to believe that the U.S. Supreme Court had no justification whatsoever for its rulings in favor of hiring blacks or women; at least in the case of *Johnson* v. *Transportation Agency, Santa Clara County,* there was "absolutely no evidence of past discrimination" (p. 435). While an argument that the Court moved in the wrong direction from a legal, moral, or even political standpoint could be convincing, to ask the reader to accept that the nine judges were grossly negligent in their decisions, seeing discrimination when none existed, stretches the credibility of the point. To their credit, the Thernstroms do not hide their political bent.

If there is one piece truly missing from this book, however, it is insight on what comes next. If America were to abandon completely affirmative action and other race-guided policies, as the Thernstroms recommend, how will black America fare? The authors make it sound so easy. Get rid of color in our policy, and it will no longer exist in our society. If the first post-*Hopwood* class at the University of Texas Law School, virtually devoid of minority students, is any indication, American will be taking a giant step away from racial equality. The authors of *America in Black and White* offer a well-argued prescription for addressing America's racial tensions, but they neglect to mention the potential side-effects of their remedy.

No doubt the debates for and against race-conscious policies will continue in the months and years to come, and no doubt the evidence presented by the Thernstroms will be cited in the discussions. For this reason, any serious scholars of race will want to read *America in Black and White*. As America seeks the best direction for social policy, it is imperative that we examine all points of view and consider all options. We cannot afford to be one-sided, and for this reason the Thernstroms have provided a valuable book. Clearly, *America in Black and White* is written for a nonacademic market; the evidence presented is neither new nor rigorously analyzed. Yet, for its purpose, which is presumably to enter the debate on the next direction of race-based policies, the book has its merits. Given the current controversial state of the political discussion, there is plenty of room for new views. While many readers will not agree with the Thernstroms, they will nonetheless finish the book with a sounder sense of a popular take on affirmative action.

The Politics of Social Welfare: The Collapse of the Center and the Rise of the Right. By Alex Waddan. Cheltenham, UK, and Brookfield, MA: Edward Elgar, 1997. 204p. $70.00.

Stathis N. Kalyvas, *New York University*

How did the Right win the battle of ideas surrounding the welfare state? How did liberals allow the debate to shift from social justice and poverty to dependency? In other words, how did important parts of New Right ideology permeate the liberal consciousness and become established parts of the conventional wisdom? This book discusses the rise and crisis of the welfare state in the United States and offers an argument explaining the ideological defeat of pro-welfare liberals by the Right. It provides a detailed overview of welfare state-building, beginning with President Johnson's War on Poverty (particularly the Economic Opportunity Act) and including numerous defeated initiatives, such as President Nixon's negative income tax and family assistance plan, and President Carter's Program for Better Jobs and Income. The book's twin focus is on social policy and the ideological context in which it is conceived and implemented.

Waddan is particularly keen on explaining "the open and agonizing nature of the collapse of faith in the capacity of social welfare liberalism to provide solutions" (p. 46). The central argument is that the way in which the welfare state was built in the 1960s undermined its future: the model underpinning it was not viable in the long term; whatever its merits, it could not last. Overly optimistic and naive liberal reformers defined poverty in a self-defeating way conducive to political and ideological confusion. Overall expectations surrounding the War on Poverty were unrealistic. A philosophical ambiguity and an ad hoc nature seemed to permeate the whole effort. The demise of the welfare state was inherent from the very beginning and in its very core. Even when the ideas enjoyed a political hegemony, Waddan argues, there was little consideration of the long-term implications of welfare policies. While governments engaged in the correction of identifiable social wrongs, there was either an unwillingness or an inability to think through the possible implications of each incremental measure, an absence of reflection about the societal costs imposed by welfare policies, and a reluctance to assume these costs and implement adequate reforms.

An example of how the unrealistic expectations of liberal reformers hurt their long-term goals is the way in which they sabotaged the Nixon-era reforms, particularly the family assistance plan, thus missing an opportunity to fortify a philosophy committing government to help the poor under virtually any circumstances. Welfare measures slugged along in a haphazard way: No real integrated structure was created, while the continued expansion of welfare rolls was not followed by a rationalization of the system. The fragmented nature of the welfare structure, particularly its residual-type elements, made it an easy target for opponents. As a result, when welfare policies met with the fiscal pressures stemming from the economic dislocations of the 1970s, particularly the recession of 1979–80, they were promptly defeated on the ideological front. It was an easy task for the New Right to connect the issue of taxation with the notion of the undeserving poor, turn poverty into dependency on welfare, and drive home its point that welfare programs undermined both the health of the overall economy and the dignity of the individual.

Although this is an interesting and plausible argument, it is just that: an argument. No competing explanations are offered, no alternative hypotheses examined. Indeed,

Waddan readily acknowledges that his book is "essentially a commentary" on the crisis of the welfare state in the United States (p. 9). He does not make any particular effort to convince the reader why his argument is more valid than others that have been (or can be) advanced to explain the same phenomenon. Moreover, even the causal mechanisms underpinning his argument are never specified; they are vague and shifting. A comparative framework could have helped in this respect. (In fact, the book's title is misleading; its subject is the politics of social welfare in the United States.) Although Waddan claims that the American case is a valuable guide to the problems encountered by the European welfare states, he takes a stand in favor of U.S. exceptionalism: "Clearly developments in the US cannot be divorced from the country's particularly idiosyncratic historical approach to social welfare issues; and obviously this tradition helps to explain why American liberalism has suffered such a spectacular and painful crisis of confidence" (p. 3).

Waddan then points out (p. 172), however, that the "relatively weak collectivist traditions" of the United States do "not fully explain quite why liberal advocates were so badly beaten." This vagueness underlines the absence of a rigorous specification of the central hypothesis. A related but more serious problem is the absence of theory from the analysis. Even though Waddan refers to welfare state theories (such as Offe's and Esping-Andersen's) at the beginning and end of the book, he makes no systematic attempt to connect his argument to these. On a more general level, while he makes several implicit assumptions about the role of ideas in the success or failure of social policies, there is no explicit theoretical discussion of either the role of ideas in policy-making or the interaction among ideas, interests, and structures; Waddan seems to ignore the thriving relevant debates.

The absence of original data, a research design, and a theoretical framework turns this book into a purely descriptive account of the politics of social welfare in the United States. It is certainly possible to argue that descriptive works do not require a full-fledged research design and theoretical framework but the presence of many implicit and explicit causal claims in the analysis interferes with the description. Although this book could serve as both a factual reference and a commentary on welfare politics in the United States, it is hardly the only one in the market. After all, the author himself suggests (p. 29) a book which, he claims, provides a lengthier and more comprehensive account than his of interpretations about the origins and long-term development of the welfare state.

"We Ain't What We Was": Civil Rights in the New South. By Frederick M. Wirt. Durham, NC: Duke University Press, 1997. 286p. $49.95 cloth, $16.95 paper.

Richard L. Engstrom, *University of New Orleans*

In 1970 Frederick Wirt published *The Politics of Southern Equality*, a case study of the initial effect of civil rights laws on Panola County, Mississippi. In 1989 he returned these to begin examining the effect roughly 25 years later. The results are reported in "*We Ain't What We Was.*"

Panola County is a small black-belt county in northwestern Mississippi. It had a population of just under 30,000 in 1990, 48% of which was African American. Its economy has evolved from primarily agriculture to small industries. The black population remains very poor, however, with 46% classified as below the poverty line, compared to only 13% of the whites.

The title of Wirt's volume is taken from a saying among the county's African American residents: "We ain't what we should be, and we ain't what we can be, but by God, we ain't what we was!" Improvements in the political, educational, and economic opportunities of African Americans since Wirt's first study are documented. Changes in federal civil rights laws are identified, not surprisingly, as having been necessary for these improvements to have occurred. But the changes to which Wirt devotes most of his attention are in the attitudes and behavior of the county's whites. It is their reactions to the changes induced by civil rights laws that is the focus of this volume.

Wirt describes his method as "primarily qualitative, with a heavy lashing of quantitative data and measures" (p. 6). He relies on more than 100 interviews with "key actors" in the political, educational, and/or economic life of the county (p. 264). These are supplemented by a content analysis of two weekly newspapers in selected years and a survey administered in 1990 or 1991 (both dates are provided) to around 1,200 students, public and private, in the 6th, 9th, and 12th grades.

The general conclusion that Wirt reaches, based almost exclusively on the interviews, is that the white residents have retreated from the "bitter recalcitrance" he found pervasive in the 1960s. They now display instead a "pragmatic acceptance" of the changes induced by the civil rights legislation. This pragmatism is attributed to an understanding that the cost of noncompliance with these laws now clearly exceeds the benefits. Whites in Panola County still do not endorse the objectives behind these laws.

The analysis concentrates on the county's politics, education system, and economic development activities. The latter two dimensions are covered best. Wirt identifies the effect that two distinct cultures in the county, the Delta culture in the north and Hill culture in the south, had on white reactions to school desegregation and economic diversification. Once the "freedom of choice" subterfuge was disallowed, the whites in the southern part of the county kept their children in public schools and supported integrated education. Those in the northern part chose the private academy option for their children and opposed additional funding for the financially strapped public system. Whites in southern Panola County were also quicker to recognize that good race relations were important to attracting the businesses that would provide economic growth and diversification, resulting in more economic gains in that part of the county.

The analysis is weaker with respect to changes in county politics. As a result of the protections provided by the Voting Rights Act, majority black districts were created for county and municipal legislative bodies, and African Americans were subsequently elected in these districts. The consequences are not examined with nearly the detail one would expect in a single-county case study. For example, changes in public employment and service delivery are noted, but not very systematically and almost exclusively at the municipal level. The county government receives very little attention in this respect, an especially curious omission given that the county is identified as the more important and powerful level of government.

Political change in the county is said to be highlighted by the election of an African American, Leonard Morris, to the state legislature in 1992. While his district has a black majority and is dominated by most of Panola County, it is not as heavily black as those from which African Americans previously had been elected. (Wirt never provides the actual racial composition of the district, but other sources place it at 54% black in total population and 51% in black population of voting age.) This election receives special attention because Morris, according to Wirt, received an astonishing (by county standards) "*40.8 percent of the white vote*" (p. 81, emphasis in

original), which Wirt for some reason repeatedly identifies as a "plurality" of the white vote, despite only one opponent for Morris. This crossover figure, it turns out, is based on the vote cast in the five precincts identified by "local knowledgeables" as white. No other basis for this designation is reported, however. No figures are provided, for example, as to the relative presence of blacks and whites in these precincts. The designations are said to be "confirmed," however, because Morris failed to carry these precincts, unlike those designated as black. The 40.8% figure is simply the median percentage of votes received by Morris among these five precincts. This is hardly a valid basis for providing, let alone emphasizing, a specific point estimate of crossover voting. Far more information about the precincts on which the estimate is based could be given in a case study such as this, especially for an election so critical to the author's argument.

There is little doubt that great racial change has come to Panola County, as Wirt documents. But he also stresses that the change has been limited to that induced by law. When it comes to aspects of life that the law does not reach, such as friendships and religious worship, Wirt tells us that the county remains divided by color. Indeed, the church repeatedly serves as Wirt's "counterexample" to the other changes brought by the law. Segregation apparently remains most intense in Panola County, as in most of this country, on Sunday morning.

Latino Politics in California. Edited by Aníbal Yáñez-Chávez. San Diego: Center for U.S.-Mexican Studies, University of California, San Diego, 1996. 142p. $12.95.

Christine Marie Sierra, *University of New Mexico*

The Latino presence in California is formidable and growing. Numbering more than 10 million, Latinos make up nearly one-third of the state's population. Demographers estimate that Latinos will account for almost half of California's residents by 2040. To be sure, the Latino presence in California is not new but embedded in the very origins of the state (and territory) itself. The importance of the Mexican-origin population to California's history is multifaceted. Yet, it is in large part the burgeoning numbers of Latinos—still predominantly Mexican in origin—that have drawn increased attention toward Latinos and their role in Californian society.

A scholarly assessment of Latino politics in California is both important and timely. The Center for U.S.-Mexican Studies has produced a volume of essays that attempts such an assessment. Aníbal Yáñez-Chávez, a professor of geography, brings together an interdisciplinary array of scholars to address important aspects of Latino political participation. Although the focus is on California, important theoretical and empirical observations relate to Latino politics at the national level as well.

As the volume makes clear, the politics of numbers does not translate easily into the politics of power for Latinos in California. Voting potential is decidedly diminished by the large number of noncitizens—estimated at 54% of Latino adults in California. Moreover, such socioeconomic characteristics as youthfulness and relatively lower levels of income and education suppress the Latino propensity to register and vote. Nevertheless, Latinos do participate in electoral politics in California as voters, political party supporters, community activists, and elected officials. Public policy debates in the state, especially in the areas of education and immigration, involve Latino group interests directly. The contributions to this volume address such topics, albeit with varying degrees of success.

Rodolfo O. de la Garza takes to task two prevalent "myths" that dominate public discourse on Latino voting: (1) Dramatic increases in the Latino population will necessarily affect the political system in significant ways, and (2) Latinos can and will leverage their potential as a "swing vote" in critical elections in order to command more attention from political elites and political parties. Drawing primarily from previously published reports and scholarship, de la Garza identifies constraints on Latino political influence in national and state politics to counter the first myth. He draws attention to the increasing heterogeneity of the Latino population, for example, with regard to nativity, citizenship status, and ethnic identification, to show how increasing numbers—and diversification—may undercut Latino political power. At the same time he underscores how, as loyal partisans rather than switchers or swingers, Latino voters and activists are more likely to wield influence within their respective parties.

Focusing specifically on California, Carole J. Uhlaner synthesizes a number of studies to provide an overview of Latino political participation and party and issue preferences. Although Uhlaner taps data sources that are familiar to students of race and ethnic politics, namely, the 1984 Caltech study of ethnic minorities in California and the 1989 Latino National Political Survey (LNPS), she draws some new comparisons from both studies that offer a fresh look at these data. In addition, she presents results from an October 1994 poll to discuss Californians' preferences on Proposition 187, the statewide initiative denying public services, public health care, and public education to undocumented immigrants. The latter discussion is particularly revealing of how ethnicity, citizenship status, and political mobilization figure into highly contested debates regarding immigrants.

A third essay, by demographer Leo F. Estrada, is a rather weak complement to the contributions by de la Garza and Uhlaner. Although Estrada extends the discussion of demographic limitations to San Diego County's Latino population, his essay lacks breadth and depth, contributing primarily basic data tables drawn from Census and *Current Survey of Population* reports.

The final chapters of the volume address education and immigration policy, two exceedingly contentious issues in California. Education specialist Patricia Gándara identifies the particularly acute problems in California, where students of Mexican origin are fast becoming the most numerous segment in the state's public schools. She recommends specific policies for their solution, such as bilingual education and school desegregation. Her essay, however, is disappointing and appears somewhat misplaced in this volume. A lengthy review of educational theories on Latino underachievement supersedes further discussion of more relevant topics, such as California's political environment and its effect on Latino education and reform initiatives. Noticeable as well is the omission of work by political scientists, such as Kenneth Meier and Joseph Stewart, dealing with Hispanic education.

Historian David Gutiérrez provides an excellent discussion of Mexican migration to California in the context of the political economy of the Southwest. While theoretically instructive, his essay makes the case for reframing the debate over immigration. Gutiérrez persuasively argues that transnational migration networks, in this case between Mexico and the United States, have "reconfigured" the boundaries of the American political community in ways that require new understanding of the migration experience, community building among newcomers, and the very nature of citizenship itself. He draws out important political implications for policymaking and political organizing that could lessen the demagoguery and divisiveness so characteristic of public debate on immigration and immigrants.

The essays in this volume were originally prepared as papers for a conference held at the Center for U.S.-Mexican Studies. Ironically, San Diego and its surrounding communities do not receive much attention in this book. Indeed, regional analyses of Latino politics within California would have been useful. At the same time, the center has produced a volume that is useful for public education and university and college courses. But because of its rather introductory nature, *Latino Politics in California* may be of limited use to scholars in the field.

Interestingly, the editor faults the volume's conventional emphasis on electoral politics (especially voting behavior) to account for Latino politics in California. Although more on Latinos in electoral politics could have been included (e.g., as candidates for political office and as public officials), the point is well taken. Additional arenas and expressions of contested politics in California that involve Latinos—from the elite to the community level—warrant further and fuller investigation. *Latino Politics in California* is just a start.

Comparative Politics

Determinants of Economic Growth: A Cross-Country Empirical Study. By Robert J. Barro. Cambridge, MA: MIT Press, 1997. 145p. $22.50.

Robert W. Jackman, *University of California, Davis*

The study of economic growth has enjoyed a considerable resurgence over the past decade, inspired in good measure by Robert Barro's empirical work. This brief volume contains three essays that originated as the 1995–96 Lionel Robbins Memorial Lectures at the London School of Economics. Together, the essays constitute an excellent and accessible synthesis of recent empirical research on growth.

The first essay, "Economic Growth and Convergence," begins with a nice discussion of neoclassical and endogenous growth theories. As Barro points out, original neoclassical arguments imply that there are ceilings on growth, absent continuing technological innovations, but the evidence suggests otherwise. At the same time, more recent endogenous growth approaches fail to incorporate typically observed patterns of conditional convergence; that is, other things equal, the poorest economies grow most rapidly. Barro develops an empirical framework that corrects for these two lacunae.

Drawing on estimates for a panel of approximately 100 countries from 1965 to 1990, Barro reports strong evidence for conditional convergence. Among other things, he shows that growth is fostered by higher initial schooling and life expectancy, lower fertility and government consumption, better maintenance of the rule of law, lower inflation, and gains in the terms of trade. At given levels of such variables, growth is a negative function of the initial level of per capita GDP, as conditional convergence would lead us to expect. Barro concludes the first essay with growth projections for 1996–2000. The average overall projection is for an annual growth rate of 2.4%, while regional average growth forecasts range from 0.5% for sub-Saharan Africa to 3.7% for Asia. Remarkably, this verisimilitude is achieved without invoking "Confucian" or any other norms!

The second essay centers on connections between economic development and democracy. Barro finds evidence of a weak, nonlinear democracy effect on growth. Expanding political rights where they had previously been absent fosters growth. Given an intermediate level of democracy, however, further expansion inhibits growth. The pattern is modest at most.

Perhaps of more interest to political scientists, given recent debates, Barro finds strong evidence that prosperity enhances democracy, a relationship he labels the "Lipset hypothesis" after Seymour Lipset's influential paper that appeared in this *Review* (1959). Even in a panel regression framework that includes a lagged measure of democracy (five years), democratic performance is enhanced by per capita GDP, life expectancy, and a smaller gap between male and female schooling. In other words, economic development affects both levels of democracy *and* rates of democratization. The essay concludes with an analysis of actual and projected long-run values of democracy, by country, for 1975 and 1994. The comparisons are especially informative in view of the third wave of democratization.

The final essay examines the effects of inflation on growth. Again, this issue has received a good deal of attention and has clear policy relevance, but much of the evidence adduced to date has been inconclusive. Barro finds strong evidence that high inflation does inhibit growth. He further shows that the adverse effect is pronounced. Indeed, high inflation has dramatic consequences over longer periods (say, 20–30 years) on the standard of living.

The crispness of these essays does not come at the expense of methodological rigor. Barro deftly addresses problems of estimation throughout, and his treatment of potential endogeneity problems is especially noteworthy. In this respect, his analyses also serve as a useful methodological corrective to many recent efforts to advance our knowledge of the sources of economic growth.

In all, this small book packs a big punch. To be sure, it does not contain all the detail that one would find in a full-blown text (such as *Economic Growth*, 1995, which Barro wrote with Xavier Sala-i-Martin). But that is not Barro's purpose. *Determinants of Economic Growth* is instead intended as an accessible synopsis of the considerable recent body of empirical work on growth. The book succeeds admirably in achieving this goal. Indeed, it has an unusually high signal-to-noise ratio. And those who may be inclined to the view that economists are insensitive to the *politics* of growth will also be pleasantly surprised.

This is must reading for social scientists interested in understanding patterns of economic growth and in learning about recent contributions to the field. Barro lays out a clear and well-defended position, and his analysis has many implications for those with research and teaching interests in the area. I also recommend his book highly to those looking for an example of an informative and effective approach to data analysis that addresses a central policy issue.

Voting and Political Attitudes in Denmark: A Study of the 1994 Election. By Ole Borre and Jørgen Goul Andersen. Aarhus: Aarhus University Press and Oakville, CT: David Brown, 1997. 352p. $33.00.

Peter Nannestad, *Aarhus University*

Outside the ranks of country specialists, knowledge of voting behavior in Denmark is most likely quite limited. Possible exceptions are the "landslide" election of 1973, when the

electorate suddenly turned the Danish party system upside down, and the referendum on the Treaty of Maastricht in 1992, when a majority voted against the treaty in defiance of the endorsement of all major parties. The book by Borre and Andersen—both leading scholars in the field of Danish electoral behavior—can be viewed as a first attempt to redress this situation.

The book takes its point of departure in the Danish general election of 1994, which was covered by a very extensive survey, but its principal aim is not just to explain the outcome. Through systematic comparisons with survey data from earlier general elections (generally reaching back to 1971), dynamic aspects of Danish voting behavior during the last two decades are highlighted throughout the study. Furthermore, some of the topics covered in the second half of the book, such as legitimacy of the welfare state (chap. 8) or mass attitudes toward unemployment policies (chap. 9), go beyond the confines of electoral studies in a narrow sense, in that they are not treated in relation to electoral choices.

Through eleven chapters the authors draw a richly faceted picture of Danish voters and their political environment. In doing so they present a wealth of detailed analyses, and they test a broad selection of theoretical propositions on their Danish data, especially in the first half of the book. Examples include theses about the demise of parties (chap. 1), the decline of class voting (chap. 5), "old politics"/"new politics" (chap. 2), proximity versus directional models of voting (chap. 3), and economic voting (chap. 4). The basic result is that "Danish voting behavior can be understood as plausible (if not always wise) responses to actual and proposed policies, to outcomes and to political events" (p. 16). In that sense the Danish system can be characterized as a bottom-up democracy. But the book also pinpoints the possible weakness of such a system. Generally, the voters display a rather limited understanding of the world around them. Therefore, external shocks to the system can easily lead to destabilizing conflicts between the electorate and the political elite.

Comprehensiveness is undoubtedly one of the main qualities of the book. It contains a wealth of information and results on different aspects of voting behavior and political attitudes. This quality comes at a price, however.

In the first place, the authors at times seem to push their analyses to the limits of what the data can support, or even beyond. This is particularly obvious in chapter 4, in which an attempt is made to explain support for government and opposition parties based on their competence ratings in various policy areas. The analysis is marred by simultaneity problems, and hence the estimated coefficients from the OLS regressions reported are most probably both biased and inconsistent. Consequently, some of the conclusions drawn—for instance, that concerns for personal economic well-being and for the macroeconomic situation of the country affect the vote equally (p. 112)—ought to be interpreted with greater caution. Furthermore, some causal models in chapter 5, on the effect of changing class cleavages on the vote (p. 145), most probably also suffer for the same reason. In some instances, more advanced econometric techniques might have remedied some of the problems, but the crux of the matter is that sometimes the authors want their cross-sectional data to provide answers to questions that require longitudinal observations. This was pointed out by Kramer as early as 1983 in the case of economic voting (Gerald G. Kramer, "The Ecological Fallacy Revisited: Aggregate- versus Individual-level Findings on Economics and Elections, and Sociotropic Voting," *American Political Science Review* 77[1983]:92–111).

In the second place, the theoretical framework of the book is not very precise. The authors profess to work from the "policy voting model," which assumes that "Danish voters indeed have opinions corresponding to policy positions in a number of policy areas, and that these positions are consistently related to their evaluation and choice of parties and alternative governments" (p. 12). In order to offset this model from "formal public choice theories prescribing what the voter should do [sic]," it is labeled "semi-rational or value-rational" (p. 12). But throughout the book different theoretical perspectives and concepts are quite liberally applied. Thus, the key concept is party identification in chapter 1 and class membership (in a Marxist sense) in chapter 5. How these concepts and perspectives relate to what is declared to be the basic theoretical perspective of the book is not always obvious. To put it differently: The vagueness inherent in the term "semi-rational" allows a great deal of what looks like ad hoc reasoning. The authors deliberately abstain from formalizing their model. This is certainly advantageous in some respects, but it seems to me that the constraints provided by the need to formalize might have done their theoretical work a lot of good in terms of enforcing clarity and consistency.

Despite these shortcomings (to which may be added the sometimes sloppy language editing and proofreading), the book is a must for anyone remotely interested in Scandinavian politics. To those without such an interest, it will still provide stimulating reading. The Danish Social Science Research Council, which financed the 1994 survey (as well as the previous ones), certainly got value for its money. The recent decision to stop financing this line of research defies understanding.

The Stasi: The East German Intelligence and Security Service, 1917–89. By David Childs and Richard Popplewell. New York: New York University Press, 1997. 320p. $45.00.

Henry Krisch, *University of Connecticut, Storrs*

A central characteristic of modern dictatorships is a "political" (often misleadingly called "secret") police. The leaders of regimes "revolutionary" in both domestic and international politics see in their transformational rule the justification for a politically deployed coercive agency.

The scholarly literature about these agencies has generally been of two kinds. One is devoted to establishing and elucidating the record of political police activity, and the other offers conjecture about the relationship of such agencies to the larger political system. The Nazi defeat in 1945 resulted in the availability of records and led more to scholarly narratives than to theorizing about the role of such agencies in the state. This situation seems to be repeating itself with the collapse of communism.

The security forces of the (East) German Democratic Republic present an excellent opportunity for scholars both to explain and explore the GDR's security agencies and to conceptualize their role in the larger political system. Was the GDR's Ministry for State Security—the infamous Stasi—an essential component of the political system? Was it simply a copy of Soviet practice? Was it perhaps an economic drain and social disorder that helped bring down the regime? Which of its activities were peculiar to the system in which it operated and which (as the foreign espionage chief, Markus Wolf, likes to claim) were operations common to modern states?

A special feature of the East German scene is that the voluminous records of the GDR's security agencies, as well as those of the party and state, survived the fall of the regime and are now available to scholars. Most writing on the subject

has focused on establishing the record of Stasi activities. Much of this has come from researchers on the staff of the government agency managing the Stasi files (see various reports on particular episodes by Walter Süss) or from personal accounts by Stasi targets. (See, for example, Vera Wollenberger, *Virus der Heuchler*, 1992, or Timothy Garton Ash, *The File*, 1997.) The work by the leading West German expert, Karl Wilhelm Fricke (*MFS Intern*, 1991), appeared too soon to take advantage of later research.

It is in this context that the first full-length English-language study of the Stasi has appeared. David Childs, a leading British writer on German politics, and Richard Popplewell cover the establishment, development, and organization of the security apparatus, its and relationship to the party and state, its close ties to its Soviet counterparts, its foreign as well as domestic activities, and its performance in the final crisis of the regime from 1987 to 1990.

Given the authors' credentials and the availability of documentary sources, it is unfortunate that *The Stasi* is such a disappointing book. Childs and Popplewell rely almost exclusively on secondary and journalistic sources; a single book, von Flocken and Scholz's *Ernst Wollweber* (1994), bears an extraordinary evidential load. There is slight reference to recent journal publications. More strikingly, there is no sign that the authors worked in the SED, GDR, or Stasi archives.

The authors properly present the evidence for the close relationship of Soviet and GDR security forces, but they do not equal the account in Norman Naimark's *The Russians in Germany* (1995). An opening chapter on the historical background from 1917 to 1945 digresses wildly, including any and every use of force by German communists. Indeed, this chapter—a grab bag of facts out of context—is typical of many to follow. Thus, events in the Spanish Civil War are discussed, but the importance of "Spain" for the later GDR is ignored. Many chapters appear to have been "filled" with material whose relevance to the topic is dubious and in any case not established by the authors (and chapters 5 and 6 overlap considerably). Examples include the case of a British MP who spied for the Czechs (pp. 134–5) or the career of Hans Globke. The work of the West German "Gehlen Organization" is presented twice in almost identical words.

More seriously, Childs and Popplewell are careless with accusations unsupported by evidence. Discussing the scandal regarding Bundeswehr commander General Kiesseling, they remark (p. 165), without citing a single source: "Many believed that behind the entire affair was the Stasi." Accusations that the West German counterintelligence chief Günther Nollau was a Stasi agent are made in the text, but only in the notes does the reader discover that the authors admit to having no evidence for this charge.

Even a flawed book deserves better editing than the publisher has given this one. Thus, Wolfgang Thierse was not formerly "a churchman" (p. 106); Herbert Wehner did not come to the Federal Republic "from the East" (p. 142); Hans Modrow replaced Willi Stoph, not Egon Krenz, as GDR prime minister (p. 191); and the Soviet invasion of Afghanistan was in 1979, not 1980 (p. 107).

The book does present some data about Stasi organization and resources, and the authors are usefully restrained regarding Stasi ties to international terrorism. But the role of the agency in controlling the Berlin Wall and the border is ignored, as is its use (typical for communist regimes) in monitoring the armed forces. Although the story is carried on to early 1990, the role of citizen activists in storming and securing Stasi headquarters is omitted.

Childs and Popplewell review the political struggles involving ministry chiefs and the SED leadership, particularly under Ulbricht in 1953–1958, but they have nothing systematic to say as to the role of the Stasi as an institutional component of the regime. Finally, they present no coherent hypothesis as to the Stasi role in the collapse of the regime. Why was this seemingly ubiquitous security force so helpless and hapless in 1989? If it was all the fault of party leaders, what does that tell us about the Stasi's place in GDR politics? How was it that the now famous army of "informal collaborators," the IMs, could not prepare the regime for the exodus of 1989? To such questions this book has no answers.

Women and Power in Parliamentary Democracies: Cabinet Appointments in Western Europe. By Rebecca Howard Davis. Lincoln: University of Nebraska Press, 1997. 137p. $32.00.

Passages to Power: Legislative Recruitment in Advanced Democracies. Edited by Pippa Norris. Cambridge: Cambridge University Press, 1997. 259p. $59.95.

Political Recruitment: Gender, Race and Class in the British Parliament. By Pippa Norris and Joni Lovenduski. Cambridge: Cambridge University Press, 1995. 280p. $19.95.

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The times are changing. While the twentieth was a century of women's equal suffrage, the twenty-first promises to be one of equal governance, as these books and other research portend. Women's national legislative recruitment in democracies—with some exceptions—is on the move at the end of this century. In the short term, change is accelerating. What was significant for women's recruitment in parliament in the 1970s and 1980s is surpassed by new variables of import in the 1990s. A case in point is personal preference vote laws and party quotas. The former was part of a party-list proportional representation cluster of favorable electoral system variables, including large magnitude districts, associated with higher proportions of women parliamentarians. This personal vote enabled women to choose women among candidates on the parties' lists. But political party quotas for the nomination of substantial proportions of women to parliament—exemplified by the Green parties' 50–50 policy—is the leading variable explaining greater women's recruitment in the 1990s among long-standing democracies. The United States is atypical, however: Significant recruitment of women in Congress and state legislatures has been stopped cold since 1992, while recruitment of women in the president's cabinet is on the increase. What used to be a few token women in American and European cabinets has now become a significant cluster, heralded by almost all parties except die-hard conservatives.

Davis's book is the pathbreaking benchmark study of women in the cabinets of Europe. How did they get there? Pippa Norris ("Legislative Recruitment," in *Comparing Democracies: Elections and Voting in Global Perspective*, ed. Lawrence LeDuc, Richard G. Niemi, and Pippa Norris, 1996, 187) informs us of an indispensable fact: 72% of the variance can be explained by the proportion of women elected to parliament (i.e., a correlation of .85). Given that, how does it happen that more women, and individual women, have become at last the chosen ones with the men of their political parties? Pressure from organized women inside and outside the parties and from favorable national leaders—even in the United States—probably explains why there are more women in cabinets. Favorable leaders might be leftists, centrists, or moderate conservatives. But, Davis asks, which? Historically, the record shows that social democratic prime ministers in the Nordic countries were first to appoint more women to the cabinet. Then other parties determined that

promoting women could increase their vote in their proportional representation countries. It was a bonus. Soon other parties joined in; today, for example, half of Austria's four women cabinet members are socialists, and the other two are conservatives, making up 25% of the cabinet in the 1997 governmental coalition.

Davis does not cover this 25-year historical development, and hence the significance of political party orientation for women's cabinet appointments is discounted. This may be a function of her data set, which uses numbers but not proportions of cabinet women from 1968 to 1992 (pp. 62–3; 115, n. 10). Four women cabinet members of the "left" in a ten-member cabinet would have a different significance than four "rightist" women in a twenty-member cabinet. Nevertheless, the future does appear to promise a continuing mix of political orientations among women cabinet members in most European countries, as Davis contends.

Yet, the long-range prognosis for women cabinet members in the United Kingdom and the United States seems different from that of most of Europe. Under former Prime Minister John Major, two women (9%) were in the cabinet, but with Tony Blair the number more than doubled to five (23%); the situation is similar with presidents Bush and Clinton. Recruitment in the United Kingdom and United States swings up and down, with troughs and spikes in a slow jerky upward trend, and it generally depends on which party occupies the top office. By contrast, the Continent generally shows a steady multiparty linear increase.

Davis's analysis is not on the specific country level speculated about above, but on the patterns of nations she classifies as "generalist," such as the United Kingdom, or "specialist," such as the Scandinavian countries. She does not include the United States in her twelve-nation study, but for comparative purposes it could be classified as a hybrid "mixed semipatronage" model, owing to the deep layer of political appointees under each cabinet member. But are these cabinet typologies predictors of the proportions and type of women in the cabinet? Davis points out very well that in European democratic political systems few women (less than one, on average) were cabinet appointees in the past quarter-century. Yet, she writes that in the "specialist" (mostly Scandinavian) countries women hold about 40% of the top portfolios, and they spent fewer years than men in the parties' waiting line before they were allowed to serve as cabinet members.

If only Davis had added electoral systems to her list of predictors! It is likely that she would have found a strong correlation between, on the one hand, the generalist countries and a majority/plurality single-member district election system (and one that behaves like it, namely, the single transferable vote of Ireland) and, on the other, the low proportion of women in parliament and cabinets. This was the case in the United Kingdom until the Labour Party's spectacular surge in May 1997. Yet, the United States remains an outlier. A country with a mixed semipatronage bent, it has a single-member district system, and women make up only 11% of Congress and have one-third representation in Clinton's second-term appointive apex, the cabinet. Clearly, the United States is among the 18% of deviant cases that need further comparative research.

Are cabinet appointments sex-typed? That is, do women have slim portfolios for children and welfare, while men have thicker ones for munitions and international power? One could say that, but it would be only partially accurate. Dozens of men have served as secretaries of welfare, but few women have been secretaries of defense or state. Again, the Nordic countries have led the way in gender diversification in cabinet appointments, as Davis reports. Madeleine Albright and Janet Reno, the first U.S. women to occupy male gender-typed "inner cabinet" posts, illustrate what women can climb the slippery ladder: Get outstanding educational credentials and experience in a specialized field. Usually, more is better in specialized and mixed-cabinet countries (such as the United States, France, and Germany). At least *completion* of a university degree generally is a requisite for the U.S. cabinet and others. (The reader is referred to case study documentation in Norris's *Passages to Power*.)

The Davis study examined the change in the number of women attending higher education institutions, rather than the proportion completing a degree. This accounts for her counterfactual downplaying of the significance of higher education for cabinet appointees (pp. 32–3). Once in office, however, do women matter? Neither Davis nor Lovenduski and Norris answer that question. It has to be studied. A case in point is what U.S. secretary of state other than Madeleine Albright has denounced a regime, such as the Taliban of Afghanistan, for denying women and girls education, health care, and the opportunity to work?

While Davis is to be commended for opening up a largely unstudied area, Pippa Norris and Joni Lovenduski ask new questions about legislative recruitment and answer in new and provocative ways in three recent books. Although not reviewed here, *Gender and Party Politics* (1993) asked how gender affects party politics and how parties influence women's political representation. This was a first for the study of parties, and it is a top-notch collection of scholarly studies for both students and instructors. *Political Recruitment*, published only two years later, "took the lid off the black box of party nominations," observed R. Darcy at a recent professional meeting. The book is another original achievement, for here we see the inside rules governing each step toward nomination in the two major parties as well as in the minor ones. The Labour Party's recruitment procedures, adopted in 1993, included a "short listing" of only women's names for nomination in certain favorable constituencies (pp. 73–4). This affirmative/positive action resulted four years later in 101 women Labour MPs, which brought female representation to 18% in the House of Commons. The Conservatives, who followed their regular procedures, elected only fourteen women MPs. Norris's edited volume, *Passages to Power*, followed in 1997, and it significantly broadens the scope of political recruitment inquiry.

To place the books by Norris and Lovenduski in perspective, I briefly consider previous theories regarding candidates' induction to political office. Samuel J. Eldersveld (*Political Elites in Modern Society*, 1989) tells us that mid-nineteenth-century theorists discoursed on elite circulation and class-based outputs. A century later, social scientists sought new paradigms to fit political recruitment and policy in the more democratic context. Among these frameworks, as summarized by Moshe Czudnowski ("Political Recruitment," *Handbook of Political Science*, vol. 2, 1975, pp. 170–89), were stages of recruitment (eligibility, nomination, and election), actors (candidates, party selectors, and voters), their characteristics (amateurs, professionals, and motivations), and the context (including political opportunity and culture).

In addition to these dimensions, Norris and Lovenduski have conceptualized recruitment very originally as a political market. Outcome is explained by political party selector demand and candidate supply (*Gender and Politics*, p. 311; cf. *Political Recruitment*, pp. 144, 184, and *Passages to Power*, p. 2). (Davis, in chapter 7, extends Norris's 1993 diagram to include women's cabinet recruitment.) Norris explains in *Passages to Power* (pp. 11–2) that candidate nominations—with certain exceptions—take place within political parties,

which have rules and cultures. These operate within political systems, which set the structure of opportunities. Selector demand is explained by selectors' attitudes regarding qualifications, while candidate supply is dependent upon such "political capital" as financial assets, political connections, and other resources, as well as motivation.

In *Passages*, Norris and Lovenduski draw upon their large, comprehensive, and carefully designed U.K. data set of aspirants, nominees, and selectors analyzed in *Political Recruitment*. They test whether supply-side or demand-side factors are more important for political recruitment (chap. 9, p. 163). They confirm that House of Commons nominees have nonmanual occupations, and about 70% are university graduates; most are male, white, under age forty, devote considerable time to party work, and have substantial financial resources, support networks, political ambition, and drive. Still, when party selectors were asked which factors were important for choosing candidates, the characteristics of young, unmarried, and politically inexperienced emerged as significant in a regression analysis (p. 184). Labour Party selectors prefer males and candidates who spend hours in party work. Since party selectors, the demanders, indicated few preferences and biases, the authors suggest that lack of candidate diversity is due to the fact that only certain applicants come forward. Are we then to blame women, manual workers, the elderly, nonuniversity graduates, and racial minorities for not applying? More significantly, why are they not coming forward?

One highly significant reason in both the United Kingdom and the United States is the lack of "open seats" in elections. These average less than 10% in each country, and it is difficult to dislodge incumbents. Norris and Lovenduski slight these critical factors and their effect on an applicant's calculus of winning nomination (e.g., *Political Recruitment*, pp. 177–8) as well as on the selectors' concern with the applicant's electability. They do find, however, that when women are encouraged to apply for nomination, more are selected. Similarly, Monique Leijenaar and Kees Niemoller ("The Netherlands," in *Passages*, p. 131) discovered that more women are nominated when there are "special policies to strengthen the position of women candidates." Clearly, this should convince most atypical legislative aspirants that in the nomination and election phases of recruitment they would have a reasonable chance of winning.

In conclusion, further theoretical development and careful research is necessary to explain the significant contexts and processes of political recruitment of women and other underrepresented groups to national legislatures and cabinets. Davis and Norris and Lovenduski have started us very well on that quest.

Sovereignty as Responsibility: Conflict Management in Africa. By Francis M. Deng, Sadikiel Kamaro, Terrence Lyons, Donald Rothchild, and I. William Zartman. Washington, DC: Brookings Institution, 1996. 265p. $42.95 cloth, $18.95 paper.

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The argument of this book is succinctly telegraphed by the title: Especially in the context of sub-Saharan Africa, the sovereign state can no longer be treated as an axiomatic, unchallengeable foundation of contemporary world politics. The international community must intervene to secure the realization of basic human rights and the essential dignity of individuals and communities where nation-states cannot or will not assume the responsibility to do so. In essence, the authors contend, epistemic global human rights, humanitarian, and human dignity communities must, and increasingly do, reside at the apex of a hierarchy of "sovereign" governing entities, from local governments on up through civil society, ethnic communities, and national and regional governments to international agencies. Implicitly, the contributors construct a pyramidal theory of sovereignty, with lower levels of sovereignty presumed to be both capable of resolving conflicts to protect basic human rights and responsible for doing so, unless they demonstrate otherwise. Failure at the more local level is to prompt intervention at wider levels, culminating in interventions by and on behalf of the international community.

While the authors take issue with Austinian positivist jurisprudence, which they suggest underlies the theory that law derives solely from the sovereign state, their argument in effect incorporates positivism as well as normative jurisprudence, for which they regard Lon Fuller as a modern champion. They see the realities of contemporary world politics, particularly in the post–Cold War era, as changing in ways to undermine de facto the unchallengeability of nation-state sovereignty and to make room in practice for more comprehensive, far-reaching implementation of basic human rights via democratic governance, the rule of law, economic justice, and the recognition of all ethnic identities. The authors recognize that the scale and intensity of contemporary conflicts, particularly in Africa, have de facto overwhelmed the capacity of states to resolve them and that the application of conflict management practices grounded normatively in the global realization of basic human rights is the order of the day.

Sovereignty as Responsibility opens up an important and underexamined area of scholarship: the nature, limits, obligations, and capabilities of sovereign governments in a post–Cold War era in which weak, corrupt, or malevolent nation-states may no longer rely on rival superpowers to sustain them as needed partners in their global power struggles. It is a welcome contribution to a still small body of theory and research that seeks truly to globalize the study of international relations. It considers the implications of characteristically overlooked sub-Saharan Africa for theories that, while purporting to be global, in fact take insufficient account of all major world regions. Furthermore, several lines of future scholarly inquiry suggested by the book's thesis merit particular attention.

The first is evolutionary connections in the modern history of international relations between sovereign states as "facts" and sovereignty as normative principle. It would be useful to go into this problem in somewhat more historical depth by looking more carefully at the origins of the state system. The Peace of Westphalia only culminated a long, wrenching, complex era in the evolution of the state system, not to mention its relations to the birth of the modern nation-state itself. More attention should be devoted to theorists of this era who made seminal contributions to both sovereignty as a concept, notably Bodin, and the notion of the sovereign state (e.g., Machiavelli and Hobbes).

The second avenue is toward a theory of contingent sovereignty. The classic theorists just mentioned regarded their subjects in near absolutist terms. Bodin saw sovereignty as unlimited, inalienable, and indivisible. Machiavelli and Hobbes admitted few normative restraints on the sovereign state once it came into existence and imported inconsistencies into their arguments to the extent they perceived possible such limitations. The theory of sovereignty fell into disuse in modern times in part because it became difficult to say where it lay in democracies, notwithstanding its normative residence

with "the people." Particularly in the post–Cold War era, when the limitations on state sovereignty have become an empirical reality in parts of the world like sub-Saharan Africa and dictators like Saddam Hussein have been forced to accept it, how viable is the concept of sovereignty itself? Is it possible to define a concept of contingent sovereignty, implied by the authors, that marries normative and empirical utility? How? What would it look like? How would it be incorporated in international law and contemporary analyses of international relations?

The third line of inquiry is normative-cum-empirical engagement with contemporary theories of international relations. The idea of responsible sovereignty as a norm joined with contingent sovereignty in practice is an arresting and potentially prescient contribution to theories of international relations as well as political theory. What I missed in *Sovereignty as Responsibility*, however, was sufficient attention to the idea of normatively responsible, empirically contingent sovereignty as a basis for addressing the weaknesses as well as building on the strengths of rival contemporary schools of international relations. What would be the contours, insights, and outcomes of a debate by the authors of this idea with, for example, Waltz, Art, and Jervis; Nye and Keohane; Krasner; and Gilpin? And what would the debate look like if a representative or two of recently resurrected democratic peace theory entered the lists, too. The strengths and weaknesses of this book as the foundation of new, creative additions to international relations theory will become clearer if the authors of the next volume on the subject accept this challenge.

Representation from Above: Members of Parliament and Representative Democracy in Sweden. By Peter Esaiasson and Sören Holmberg. Aldershot, United Kingdom: Dartmouth. 1996. 360p. $67.95.

Jacques Thomassen, *University of Twente, The Netherlands*

Representation from Above is an impressive and important book, based on an equally impressive series of studies on political representation by scholars at the University of Gothenburg in Sweden.

It is well known that after the great success of *The American Voter*, published in 1960, its authors spread out over the world, in particular to Western Europe, to preach the new gospel of electoral research. As a consequence, the influence of the Michigan school on Western European voting studies can hardly be overestimated. What is perhaps less well known is that at the same time Michigan initiated, or helped to initiate, political representation studies in a number of Western European countries, including Great Britain, France, West Germany, Italy, the Netherlands, and Sweden. The source of inspiration was the famous study on political representation by Donald Stokes and Warren Miller. In most countries this was a one-time event. In Sweden, however, it was not. The first Swedish Representation Study was organized by Warren Miller and Bo Särlvik in 1968 and replicated in 1985 and 1988 under the direction of Sören Holmberg, in this book joined by his junior colleague Peter Esaiasson. Each of these studies combined an election study with a survey among the members of the Swedish parliament, the *Riksdag*. The number of questions asked across levels, in particular on issues, is larger than in any other representation study of which I am aware. Also, the analytical possibilities of this design are enhanced by a content analysis of all government bills, party bills, private member bills, oral questions, and floor debates for the period 1985–88. This made it possible to link the attitudes of members of parliament to their behavior in parliament. Such wealth of data is unparalleled in the world.

But, of course, data do not write good books. It takes good scholarship to do that. It is the combination of a wealth of data and excellent scholarship that makes this such a good book. After a short introductory chapter on "the puzzle of representation," the book is divided in three parts, analyzing how members of parliament act in three different capacities: as representatives, as opinion formers and as decision makers, followed by a concluding chapter on the "not so Responsible Party Model." The analyses throughout the book are guided by two distinctions representation run from above versus representation run from below, and individualist versus collectivist representation. In a nutshell, representation from above refers to a process of opinion formation where the mass public follow the elites, whereas in representation from below the political elites are more responsive and inclined to follow the mood of the public. Which of these two models comes closer to the real world is not easy to assess. The richness of the analytical possibilities of the Swedish series of representation study is best shown when the authors try to answer this fundamental question. It is possible to see not only the differences of opinion between voters and representatives at each separate point in time, but also how these differences have increased or decreased over time and—what is essential here—the dynamics of these changes: top down or bottom up. Cases in which voters and members of parliament have grown closer together can be tested by seeing who have moved most: the voters towards the members of parliament or the members of parliament towards the voters. The conclusion is that in most cases "dynamic representation" means a top down process—voters usually follow the elites.

Equally fascinating is the chapter "Knowing the People." If members of parliament want to follow their voters, or at least are willing to take their voters' opinion into account, they must know where the voters are. Therefore, the authors try to assess to what extent members of parliament are aware of the true opinions of the voters in different policy areas. Not surprisingly this differs from one issue domain to the next. But the most interesting analysis and finding refer to the *explanation* of perceptions and misperceptions of the people's opinions. The authors apply Heider's theory of cognitive balance to politics, i.e., the theory that people want to be in accord with what they like and in discord with what they dislike. They believe their friends think as they do and that their enemies think differently. Politicians appear to be human. They tend to think that their voters have the same opinions as they do. It is not the true opinions of the voters that define MPs' perceptions of where the voters stand, but their own opinion. For the working of perception as a tool in the process of political representation this is a devastating conclusion. What it means is that "information channels between voters and representatives work badly when they ought to work best, and they work best when they need not work at all." When representatives agree with their voters, they will have a correct perception of their voters' position, and a wrong perception when their position deviates from their voters' opinion. But in neither case has it anything to do with real knowledge of where the voters are.

Individualist versus collectivist representation is the second distinction guiding the analysis throughout the book. Individualist models of representation emphasize the dyadic relationship between representatives and their constituents. In this view, which individuals are chosen to represent the voters is decisive. Collectivist representation

models, by contrast, point to parties as the overwhelmingly dominant actors; individual representatives have but an insignificant space in which to maneuver. The distinction between collectivist and individualist models reflects a contrast between European and American perspectives on political representation. European scholars have strongly argued that United States-inspired individualist models are not meaningful in reference to party-dominated parliamentary settings. According to the authors, this distinction is too simple. As much as politics is dominated by party discipline, in Sweden as much as in any other European parliamentary democracy it does make a difference, they argue, which persons on the party lists are actually elected. The analyses make it clear indeed that different members of parliament champion different interests, and different MPs championing different interests exploit different parliamentary channels. As it turns out, myriad interests and loyalties meet and sometimes collide in parliament. Representatives from one-and-the-same party move in partly different worlds. As a consequence, according to the authors, the analysis of political representation should not be limited to examining roll calls and other officially recorded actions—the sort of parliamentary actions where party discipline is of overriding importance. But members of parliament are not just puppets on the string of their party. The extent to which they act as representatives of local interests and policy views in introducing private bills is surprisingly great. This is presented as evidence that one should not overlook the importance of what individual members of parliament bring into the decision-making process, in addition to the importance of their party, which is known well enough.

Representation from Above really captivated me. It is written in plain language, the authors are never on a high horse, and the analyses are parsimonious and as simple as possible. Apart from a few stray regression analyses and a single factor analysis, all analyses are done at a level with which Paul Lazarsfeld, in his day, would have felt quite comfortable. This is not meant as criticism; quite the contrary. Methods should serve one's research questions and not the other way around. This is demonstrated here in an exemplary way: it is a book about politics rather than about advanced methods for their own sake.

I might dispute some of the arguments and interpretations on the two guiding principles of the book, representation from above versus representation from below and the merits of individualist versus collectivist models of political representation. The authors argue that the dispute between American and European scholars on the merits of these two models is too black and white. The main argument of European scholars against the individualist character of the dyadic representation model is that it is hardly feasible in the parliamentary democracies of Western Europe, for the simple reason that party discipline is such that there is hardly anything left to explain at the individual level. This debate refers to the representation of policy issues once it comes to formal decision making, such as roll calls. In spite of their argument, the authors implicitly seem to agree with what they call the European view. In their analysis of policy congruence between MPs and the electorate, they rely on nothing but collective representation. They do not even argue why they do not consider the application of a dyadic model of representation here, but rely immediately on the comparison of voters and MPs party by party. Where they make a case for a more nuanced view of the role of individualist modes of representation in the context of a parliamentary system, they refer mainly not to policy responsiveness but to allocation and service responsiveness. However, a collectivist model of representation in which almost all formal decisions in parliament are made collectively—i.e., party-wise—does not exclude the possibility of MPs' serving the interests of their constituency, however defined.

None of this is meant to deny the importance of the analyses done at the level of the individual member of parliament, which do lead to important and sometimes surprising results. It is surprising indeed how much members of the Swedish parliament care about the problems and interests of both local and functional constituencies. However, it is precisely this finding that might have led the authors to a somewhat more nuanced conclusion with respect to the second major argument of the book: representation from above versus representation from below. The conclusion that representation runs more from above than from below is based completely on the comparison of the issue positions of the members of parliament of a particular party collectively versus the voters of their party. The nuance is that if responsiveness is defined differently than as policy responsiveness, members of the Swedish parliament turn out to be quite responsive, and obviously sensitive to sounds from below. But none of this has much of an effect on my judgment of this book. *Representation from Above* is excellent, based on excellent data, full of well-considered analyses, and a great contribution to the empirical literature on political representation.

Bureaucracy and Race: Native Administration in South Africa. By Ivan Evans. Berkeley: University of California Press, 1997. 416p. $55.00.

Anthony W. Marx, *Columbia University*

Ivan Evans has provided an impressive historical analysis of the administrative emergence of apartheid. He ably argues that racial domination was based not just on coercion but also on institutionalization and socialization. Not satisfied with a simple state determinism, he looks inside the "black box" of the state to explain relatively autonomous policies influenced by economic interests, bureaucratic careerism, ideology, and opposition. The result is a portrait of routinized oppression that explains how the "banality of evil" actually develops. Scholars intrigued by South Africa's transition to inclusive democracy will be well served by this study, which provides crucial background on the extensive racial domination from which the postapartheid regime emerged.

Evans acknowledges that apartheid did not develop out of a uniform grand design, but at the same time he demonstrates a clear pattern of rising state intervention. Racial administration was bureaucratized, providing a new career path for social engineers. Centralization of power produced increasingly regimented outcomes, most notably the grids of cheap matchbox houses provided to black urban dwellers. As James Scott's recent work suggests in other contexts, state intervention produced damaging simplifications. The market was skewed by such intervention, guided by racist images of science and anthropology. The result was a highly effective form of social control by race, augmented by but not fully reliant on the use of force. And all of this was accomplished with careful attention to the bottom line (albeit very costly in other ways). Indeed, the construction of urban black townships was achieved relatively cheaply, with blacks encouraged to build for themselves within the regimented design and forced to pay for materials.

The urban experience is the focus of the first part of this book. Competing labor needs and interests of farmers,

mining companies, and manufacturing firms were managed by state authorities seeking the best possible compromise. The result controlled urban influx while providing sufficient cheap labor to the various economic sectors. Blacks themselves unwittingly cooperated, drawn into the urban schemes by their need for work and housing. And as black urbanization grew with the economy, so did the level of administrative and legal control. The extent of the regulation is revealed by the eventual criminalization of one in four Africans. Kept off balance by massive prosecution for any behavior violating apartheid, blacks were channeled into making the awful system work.

Admirably not satisfied with only looking at the more infamous urban controls, Evans focuses on the rural experience in the second part of his study. State authorities were long uncertain about how to maintain a rural home-base for the majority while ensuring sufficient urban labor. The solution was the reserves, later made into officially autonomous homelands. The system built upon the earlier British colonial practice of buying the loyalty of supposedly traditional chiefs and headmen. Self-serving black elites were recruited into positions of domination over their own people. "Tribalism" was justified by skewed anthropology, borrowing even from Nazi writings. And such categorization of the majority was used as a form of divide and rule, which was uneasily balanced with the overriding racial domination that risked unifying apartheid's victims.

The tension between rural ethnic policies dividing blacks and urban racial domination potentially unifying blacks refers to an important debate in South African historiography, which is not fully explored by Evans. Mahmood Mamdani's *Citizen and Subject* (1996) has provocatively argued that South Africa's use of ethnicity was comparable to the divide-and-rule tactics of colonialism elsewhere in Africa. Yet, as Evans notes, the racial basis of domination eventually superceded ethnic division, at the cost of indeed provoking a more unified black opposition. Was this outcome simply a reflection of rising urbanization, in which ethnicity was diluted? Was it demographically determined? In my view, the increasing focus on race over ethnicity was not determined by bureaucracy but by national-level electoral competition and issues. This dynamic gets short shrift in Evans's analysis, pushed aside by considerations of administration and economic interests. His project of looking inside the black box of the state would be furthered by greater focus on politics per se.

The author's larger point, that apartheid worked not just because of force but also because of effective administration, stands. Max Weber's combined focus on force and bureaucracy is vindicated, with only legitimacy crucially missing from the apartheid state. Yet, Evans suggests, despite his own argument, that even legitimacy of an odd sort was evident, at least among whites. The very effectiveness of racial administration won respect from the unexpected quarters of former liberals within the bureaucracy. Here, Hannah Arendt's analysis and Eichmann's shadow of banality falls on South Africa. But was apartheid so banal? Certainly, its bureaucracy was, almost by definition, but it took orders from white elected politicians driven by less banal imperatives and ideology. Evans would not dispute this, though he largely overlooks what determined the direction of policy. Still, his well-researched study breaks ground in showing how one of the most developed, evil instruments of social engineering in this century, the apartheid state, gained effectiveness in implementation and enforcement.

Threats to Democracy: The Radical Right in Italy after the War. By Franco Ferraresi. Princeton, NJ: Princeton University Press, 1996. 311p. $35.00.

The French National Front: The Extremist Challenge to Democracy. By Harvey G. Simmons. Boulder, CO: Westview Press, 1996. 285p. $19.95.

William Safran, *University of Colorado at Boulder*

Of the making of books on right-wing extremism there seems to be no end. The persistence of radical-Right political movements, especially in Western Europe, in face of a growing consensus about institutional democracy continues to baffle social scientists, who are engaged, as before, in examining the root causes, ideological foundations, and popular support of these movements. These two books make a significant contribution to this effort; both are thorough, judicious, and impressively documented, and both cover the essentials: the circumstances surrounding the rise of the movements with which they deal, their internal structures, their dogmas, and their relationship to other political organizations. Franco Ferraresi's book is particularly welcome, because it is the first detailed examination of extreme-Right political activity in postwar Italy to go far beyond the numerous studies that have tended to focus on the Italian Social Movement (MSI) and its electoral fortunes. Ferraresi analyzes the background of the MSI, the changes in its post-Mussolini leadership, its connections with other extreme-Right formations, and its attempts to adapt to the changing context, notably the political evolution of republican Italy.

Ferraresi tackles the difficult problem of Italian right-wing ideology, which appeared to draw much of its inspiration from German romantic, organic, and mystical nationalism. That inspiration was particularly exaggerated during the short-lived Nazi puppet regime. As a part of his effort, the author provides an extensive treatment of the ideas of Julius Evola, one of the major fascist ideologues, whose contributions have been largely ignored by Anglo-American scholars of Italian right-wing extremism. In attempting to deal with the "generic" features of fascism, Ferraresi usefully reminds us of the difficulty of placing it on the Right-Left continuum and points to a well-known "paradox" (p. 33). On the one hand, fascism in Italy, as elsewhere, has been clearly right-wing in its elitism, hero worship, statism, collusion with business, and contempt for the masses. On the other hand, in the context of the reality of competitive elections—and while maintaining its anti-Marxism—Italian fascism had to appeal to a significant section of the electorate in order to make its influence felt, and in so doing it proclaimed its opposition to capitalism and to traditional bourgeois attitudes. As Ferraresi insists, however, the fascist interest in mass support was purely instrumental and, ultimately, not followed up with policies evincing a credible concern for the problems of the masses. (That the fascists were often successful in obtaining such support may tell us something about the rationality of the political behavior of the masses.)

The democratic "adaptation" of the MSI to Italy's national political developments was signaled by its growing moderation, which gained momentum in the 1960s, when the Christian Democratic Party moved toward the center, and the MSI began to feel marginalized. At the same time, it could not break completely with other right-wing formations to which it was related; among these were the Ordine Nuovo (ON) and the Avanguardia Nazionale (AN), which Ferraresi discusses in some detail. He also devotes considerable space to terrorist movements, which may disconcert those who tend to identify Italian terrorism exclusively with the radical Left.

In reading this book, we learn a great deal about the Italian polity as a whole—its institutional confusions, its strata of

corruption, and the "spongelike features" of the whole system (p. 142), which (at least before the shakeups of the early 1990s) absorbed, controlled, and often colluded with the MSI and other organizations usually referred to as "antisystem." Ferraresi calls particular attention to the secret service, the police, the military, segments of the judiciary, and—until its apertura a sinistra—the Christian Democratic Party. These relationship are illustrated by several case studies involving corruption, disappearances, assassinations, and attempted coups d'état. The author's knowledge about such matters derives in large part from his role as a consultant with a parliamentary committee of inquiry on political massacres. These events are interesting in themselves, but they are recounted in so much detail that we tend to be overwhelmed by them.

In his excursus on the evolution of radical-Right ideology, Ferraresi provides many useful comparisons with Germany but hardly any with France. It would have been interesting to contrast the thinking of Evola with that of, say, Maurras; to compare the ON and similar organicist and elitist Italian "study groups" with the French GRECE; to explain more fully why right-wing extremism in Italy was accompanied by a much greater degree of violence than were similar orientations in France; to explain why the MSI had more difficulty than the French National Front (FN) in accepting homo oeconomicus; and, finally, to elaborate on why the MSI never fully subscribed to the anti-Semitism of the German Nazis, neo-Nazis, and the FN. We are told that for the ON, the MSI, and even Evola himself the Jews were only a symbolic enemy, part of a whole array of enemies, whereas for FN leader Le Pen the Jew is the enemy par excellence. While the MSI's successor, the Alleanza Nazionale (AN), has been distancing itself from Nazism in general and from much of Mussolini's heritage, and its current leader, Gianfranco Fini, has abjured anti-Semitism and even called for the restitution of looted property to Jews, Le Pen has perpetuated a certain nostalgia for the Hitler regime and continues to be obsessed with Jews. To be sure, certain similarities between the AN and the FN persist; thus, the AN wants to maintain the "heroic dimension" of politics, a dimension often articulated in Italy from Mussolini's balcony, and the FN backdrop is often one or another statue of Joan of Arc.

An interesting finding by Ferraresi is the evolution of the Italian radical Right from an antisystem party toward the mainstream. This is an effort that the FN has not yet been able to match, as the study by Harvey Simmons indicates. Despite steady growth in membership and widening electoral appeal, the FN is still a marginal party, at least in terms of its representation on the national level, and it is considered by many to be a danger to democracy. The image of the FN owes a great deal to its antirepublican origins, of which Simmons provides a useful summary description. The FN drew its inspiration from the Action Française and a miscellany of other antidemocratic forces, and it emerged in the early 1970s under the leadership of Jean-Marie Le Pen as a rallying point of Poujadists, anti-Semites, xenophobes, unreconstructed Vichyites, adherents of the neofascist Ordre Nouveau, and erstwhile proponents of Algérie Française. The FN capitalized on the growing disaffection with Gaullism and on the backlash against the apparent disorderliness of the rebellions of 1968. Subsequently, it exploited for its own propaganda purposes the massive influx of immigrants and growing unemployment, on whose causal connection the FN has continued to insist. Immigration has contributed to a change in the texture of French society, which is becoming ever more "multicultural"; this development has led to a debate about the nature of the French nation and the threat to traditional images of French national identity, a debate that has benefited the FN.

In an interesting chapter on women and the FN, Simmons provides some insight into one of the party's many ambiguities. The party clearly appeals to "macho" instincts, and for that reason as well as its identification with violence, women have tended to give the FN less electoral support than have men (here, some of the available exit polls would have been helpful). Le Pen has had some success, however, in gaining support from strongly Catholic women, not so much by evoking the warrior myth of Joan of Arc but by stressing traditional family values.

To round out the context within which the FN operates, Simmons refers to its relationship with GRECE, the Club de l'Horloge, and other right-wing clubs and think tanks. He devotes considerable attention to the heterophobia, racism, and anti-Semitism of the FN. Not only do these attitudes occupy an important space in the ideological baggage of the movement as a whole and of much of its electorate, but also Le Pen's Judeophobia has been reflected in the articulation of revisionist themes concerning the Holocaust and in charges about "double loyalty" and worldwide conspiracy. To be sure, the FN does not have a monopoly on anti-Semitism, nor—as public opinion surveys show—are all FN voters consumed by it, especially since the traditional preoccupation with Jews seems to be eclipsed by a growing animus toward the much more numerous Muslim Arabs. Moreover, it is a matter of controversy to what extent Le Pen openly voices stereotypical attitudes about Jews that are held by "mainstream" conservatives, among them presidents of the Republic. In any case, in order to make his point, Simmons provides examples of racist and anti-Semitic incidents.

Simmons deals extensively with the electoral tactics of the FN, including its relationship with Mouvement pour la France (MPF) and the "republican" Right, especially the Gaullist party. He points to the "copycat" behavior of Gaullist politicians, notably with respect to policies on immigrants and immigration, and he recalls the divisions within the Gaullist party and its ally, the UDF, about what attitude to adopt toward the FN, ranging from alliance building, adamant opposition, attempts to stay out of the debate, and efforts to ignore the FN while trying to recapture FN voters for mainstream conservative parties. This conflict is, in a sense, the obverse of the conflict *within* the FN between the ideological hardliners, who wish to demarcate themselves from "the gang of four," and the "moderates," who want to improve their republican image and increase their national respectability. One manifestation of this conflict has been what an FN leader calls "the vocabulary battle" (p. 217): the clever use of euphemisms (such as "antinational," "cosmopolitan," "mondialiste"), especially by those trying to compensate for the crass obscenities often uttered by Le Pen himself.

The analysis might have benefitted from further elaboration of a certain number of items: the FN relationship with the MPF and the Centre National des Indépendants, since both of these small formations have been regarded as "passageways" to the FN; the FN's economic programs (including its prescriptions for solving the unemployment problem, to the extent that these have been credible or have existed at all) and institutional ideas; and, finally, the evolving conflicts within the FN leadership regarding tactics, notably between Le Pen and Bruno Megret. But these minor points do not detract from the excellence of this finely tuned combination of chronological and topical perspectives.

The Cost of Being Female. By Sue Headlee and Margery Elfin. Westport, CT, and London: Praeger, 1996. 229p. $65.00 cloth, $22.95 paper.

V. Spike Peterson, *University of Arizona*

As their eye-catching title suggests, Sue Headlee and Margery Elfin set out to demonstrate that discrimination against women can be assigned a monetary value. Prompted by reflections on the government's Cost of Living Adjustment index (COLA) and the gender gap in earnings, the authors produce a Cost of Being Female (COBF) index intended to facilitate both comparative studies and policy recommendations. Individual chapters present five aspects of the COBF (economic, political, social, educational, and health measures); the COBF in Sweden, Norway, France, and China; the COBF in England, France, and the United States in the nineteenth century (reviewing the impact of industrialization); a comparison of the COBF across generations in the United States from World War II to the present; and a short conclusion gathering these comparisons into a chart and making ten policy recommendations. Throughout, the authors insert brief narrative excerpts from women's lives and experiences, derived from interviews with seventy women over the past five years.

The economic aspect of the COBF—women's earnings as a percentage of men's—is the most familiar and easiest to document. While recognizing that discrimination does not account for the entirety of the difference between men's and women's wages, the authors defend using the full earnings gap as an appropriate proxy of the economic cost of being female. The authors analyze why the majority of women now work for wages and document differing costs for single, married, parenting, and childless women. Conventional reasons for and analyses of the pay gap follow.

The authors recognize that discrimination in the workplace is too narrow a measure of what it costs to be female. In their words, "Equal pay itself does not answer our desire for justice" (p. xxiv). The chapter on a political dimension presents conventional points about women and politics, with specific treatment of the apparent (1992) but then attenuated (1994) gains for women in recent elections. Arguing that high campaign costs impede women's entry, the authors recommend public financing of elections. They identify women's lower status and "supporting roles" due to childrearing expectations as the COBF in social life, and select the number of children per woman as its measure. The COBF appears to be the least in regard to education. The authors note, however, that females receive less an inferior than a *different* education (less math and/or career oriented), which effectively reproduces gender inequalities. Regarding health, the authors point out inferior funding for women's health concerns and gender bias in medical studies; they recommend preventive care, public support for family planning, and universal health care.

By reference to the five aspects of COBF, the authors' national comparisons illustrate the singular importance of child care and support parenting in determining the COBF. The historical chapters illustrate how industrialization realigned gender relations and how costs have varied generationally from 1945 to 1995. The authors conclude that the *objective* COBF is "lower now than a generation ago because women have moved out of the separate sphere of the home into economic life" (p. 201), but that the *subjective* COBF is actually higher because social changes have not kept up with economic ones. Working for wages simply *added* an extra shift for women, who also confront less role security because what they do no longer matches traditional—and not much altered—expectations.

I agreed to review this book because I expected (from the publisher's blurb) a wealth of current data along a variety of dimensions and an interesting comparison of gender discrimination in five countries. I appreciate cross-generational attention, which enriches our understanding of how women are differently positioned in relation to experiences of, and attitudes toward, discrimination. I applaud attempts to humanize statistical data, in this case by inserting real women's narratives. And I value comparative studies. Moreover, in spite of my reservations about reducing complex social phenomena to numbers, I thought a cost-benefit analysis of gender discrimination might be particularly effective: it would enjoy immediate appeal in a culture committed to quantification and consumption, and its presumably less equivocal analysis might be of particular value in clarifying and promoting a "progressive" political agenda.

The book does present a great deal of information, but in spite of its increased availability (due to feminist research and activism), the currentness of the data varied widely, and at some expense to the quality of the work. I was also exasperated by apparent inconsistencies. The rate of participation of U.S. women in the workforce was 57% on page 2, but on page 130, "a higher percentage of French women work for pay than American women—57 percent compared to 51 percent of American women." Particularly confusing was the identification of both 71 and 59 cents as women's proportion of male earnings in the United States; only on page 210 do the authors finally clarify that the differing statistics are due to different sources (reliance on U.S. Department of Labor [71 cents] versus United Nations [59 cents]). And in their capstone summary chart of the COBF Index (p. 209) they show the educational gap in the United States today as 110 in one row, 30 in another—a dramatic inconsistency. Close reading of explanatory notes revealed 110 as reference to "ratio of female to male graduates from college," whereas 30 referred to "percent of females [in the population] graduating from college"; two pages later the text clarifies that "today" in the latter instance is 1960! In short, the inconsistencies were not so much errors (the authors report their research accurately) but irritations (the reader has to work too hard to make consistent sense of the text).

Unfortunately, the narrative excerpts failed to make the statistical data come alive. Perhaps, because of space constraints, it is simply not possible to include the detail required to paint a picture of "real" people. But the text as a whole suffers from what I experienced as a wooden writing style, particularly apparent in the narrative excerpts.

Finally, I remain ambivalent about reducing complex social phenomena to numbers. On the one hand, I want people to be aware of gender discrimination, and quantitative measures offer stark evidence that women literally pay a price for being female. Given today's backlash against feminism and the continued obfuscation of women's structural subordination, I commend Headlee and Elfin's desire to make the cost of being female visible. On the other hand, relying on quantitative analysis imposes its own costs. From my vantage point (and briefly), this book pays its highest costs in terms of narrowness. By narrowing their focus to five single measures (wage gap, percentage legislative seats, children per woman, maternal mortality, percentage college graduates), the authors sacrifice attention to significant features of real women's lives (for example, systemic objectification of and violence against females). In a related sense, by focusing on women, men drop out of the picture even as the dichotomy of men versus women is maintained. In a different sense, the

book is specifically narrow in its uncritical presumption and promotion of "traditional" families and heterosexuality. I do not expect all gender-sensitive work to engage these issues in depth, but complete silence is problematic. As a final concern, the five incommensurate single-score indices that constitute their ultimate COBF index are so narrow as to be of limited utility, especially for comparative studies.

In spite of these reservations, I acknowledge that Headlee and Elfin have a good, catchy idea. Focusing on costs has appeal, and publicizing the costs of being female has feminist appeal. By combining indisputable facts with accepted argumentation and a nonconfrontational tone, the book's strength may be its utility in introducing students to the real world of gender discrimination.

The Social Psychology of Protest. By Bert Klandermans. Cambridge, MA: Blackwell. 1997. 257p. $21.95.

John D. McCarthy, *Pennsylvania State University*

During the last two decades we have witnessed the emergence of an immense and vigorous scholarship on collective action. Political scientists and sociologists working under a variety of theoretical banners (such as resource mobilization, political process, political opportunity, new social movements) have crafted diverse macrostructural accounts of the timing and social location of public protest as well as the emergence and development of social movement organizations around issues as diverse as civil rights, the environment, gender, peace, and sexual orientation. These many complementary accounts were motivated, in large part, by a rejection of the earlier, excessively psychological, and for the most part poorly documented accounts that had dominated collective action scholarship during the early Cold War. Those who claimed there was no need for a special social psychology for activists achieved theoretical dominance, despite the fact that, as John Lofland has noted, the bulk of published research remained focused upon the participation of activists.

Recently, however, a number of articulate critics of the pervasiveness of the new theoretical accounts of collective action have claimed that one risks throwing out the baby with the bathwater by marginalizing social psychological accounts of participation in public protest and social movement organizations. The accounts take mostly for granted the three key cognitive processes that underlie individual activism: (1) coming to understand that one's personal circumstances are the result of a public problem, in C. Wright Mills's classic formulation, (2) choosing to act upon such an understanding, and (3) sustaining or abandoning the new-found commitment to act.

Bert Klandermans vigorously pursued his own distinctive research agenda during the same period that the macrostructural accounts came to dominate, one motivated, instead, by an unwavering commitment to making central the problematics of the three key cognitive processes underlying activism. *The Social Psychology of Protest* is an impressive synthesis of his own extensive research on these processes with a widely diverse research literature on individual collective action participation. Unlike many of the critics of macrostructural hegemony in this scholarly arena, Klandermans seeks a detente by, first, specifying a narrow scope for a social psychology of protest by limiting the questions it should be expected to answer (how to explain the timing, form, intensity, and persistence of individual participation in public protests and social movement organizations) and, second, attempting to specify the broader social structural contexts that shape individual participation decisions (variable social movement organizations, multiorganizational fields, and political opportunity structures).

The social psychological principles Klandermans deploys to explain variable individual movement participation are an eclectic mix. His scheme melds elements of value expectancy theory (behavior is seen as a function of its *expected outcomes*, which are in turn partially a function of the expectation of the behavior of others), collective action theory (especially the implications of the key distinction between selective and collective incentives), and theories of organizational psychology (featuring a synthesis of accounts of variable affective and normative commitment to social movement organizations) in order to account for individual decisions to participate, the preferred forms of participation, and trajectories of participation.

Devotees of any particular theoretical perspective are not likely to come away pleased with the resulting theoretical melange, yet it allows Klandermans to make convincing sense of a variety of empirical patterns of participation in the Dutch antimissile mobilization of the 1980s, several Dutch labor union campaigns, various phases of the Dutch women's movement, as well as in collective action in postapartheid South Africa (based upon national surveys carried out there in collaboration with Johan Olivier). His success in applying his thoroughly unpolemical synthesis to the interpretation of such diverse empirical evidence is a strong advertisement for its general utility.

A substantial portion of the empirical and theoretical leverage achieved by the analyses depends upon the use of time; Klandermans marshals evidence on individual careers of participation, social movement organizational life cycles, and protest event cycles (an appendix touts the methodological requirements and theoretical utility of this class of research designs). As is widely understood, when research designs integrate time, they allow for far more robust causal interpretations than do cross-sectional ones. But they also demand more diachronic theoretical accounts, moving the center of interpretive gravity from static attitudinal and characterological ones to dynamic contextual ones that include responsive actors, as Klandermans demonstrates.

The author elaborates a rich set of conceptual tools and employs them with telling effect throughout the book. The scheme he offers for categorizing forms of protest participation (a joint function of the effort required and its duration) is more theoretically grounded than similar widely used schemes. With it Klandermans is able to generate strong predictions about the differing motivational dynamics governing, for instance, taking part in a protest event versus regular volunteer work in a movement organization—ideological commitment is more important, he argues, in accounting for the latter. Similarly, his elaboration of the distinction between consensus and action mobilization (altering predispositions versus participate or generating participation itself) allows him to specify more appropriately the goals and consequences of the many efforts of movement organizations. A failed attempt to bring thousands of supporters into the streets can, under the right conditions, generate widespread sympathy for their goals, for instance. His consensus/action mobilization terminology has already been so widely appropriated by social movement analysts that it is now rarely attributed to Klandermans.

Bert Klandermans has succeeded, then, in writing a quite valuable book which fills an important intellectual niche, the boundaries of which he has been instrumental in establishing. He is ideally situated to attempt a grand synthesis like this one, since he has been a roving ambassador of collective action research among disciplines and continents for the last

twenty years. Those activities provided him access to the best current collective action scholarship, which provides the bedrock of his synthesis.

Nevertheless, the book is not without minor flaws. Its focus is almost exclusively upon the motivations and behavior of rank-and-file activists rather than leaders, mirroring the collective action research landscape, a problem acknowledged by Klandermans. Finally, the book would have been improved by better copy editing. It is replete with inelegant phrases and awkward sentences that detract from the generally cogent line of argument. Regardless, *The Social Psychology of Protest* is likely to stand for some time as the most comprehensive effort to craft a social psychology of collective action.

Out from Underdevelopment Revisited: Changing Global Structures and the Remaking of the Third World. By James H. Mittelman and Mustapha Kamal Pasha. New York: St. Martin's, 1997. 289p. $55.00 cloth, $16.95 paper.

Understanding Development: Theory and Practice in the Third World. By John Rapley. Boulder, CO: Lynne Reinner, 1996. 203p. $42.00 cloth, $17.95 paper.

Bruce E. Moon, *Lehigh University*

Few policy areas have witnessed the rapid swings of sentiment, the fierce ideological divisions, and the technical complexity that make Third World development so daunting a subject for outsiders. These two books provide an effective introduction to this fractious literature for the intended audience of generalists and students, though there is little new here for development scholars.

Rapley organizes his exposition around a timeline of development theory's shifting fashions concerning the relative role of the market and the state. He begins with a review of classical theory's homage to the free market, a primer on the Keynesian response, and a brief summary of the state of the Third World through the middle of the twentieth century. With these pieces assembled, he outlines the basic elements of development thinking in the postwar era: the initial structuralism associated with ECLA and various strands of modernization and dependency theories. Chapter 2 then examines statism as the policy upshot of this theoretical melange, though he correctly notes (p. 27) that "events, rather than theory, drove the early experiments with import substitution industrialization (ISI)." He argues that the statism exemplified by the initial successes of Latin American ISI eventually fell victim to the crises of the 1970s. In the following chapters, Rapley plays out his thesis that development theory and practice have moved more or less together in a continuing pendulum swing. Chapter 3 documents the reemergence of neoclassical thought in the 1980s in response to the discrediting of ISI, and chapter 4 examines structural adjustment policies as the embodiment of that neoclassical theory. Chapter 5 sketches the disparate responses to the evident limitations of structural adjustment, emphasizing the reemergence of the Left and the appearance of the "developmental state" model to encapsulate the economic successes of East Asia. Rapley seems to embrace this "new statism," but chapter 6 offers a generally pessimistic assessment of prospects for successful implementation of the model elsewhere, especially in Africa, due both to internal limitations (especially the weakness of the state) and international obstacles. Indeed, he correctly notes that African states, under the pressure of austerity, are generally moving away from this expansive role. As he adroitly does throughout the book, Rapley supports this claim with thumbnail sketches of the experience of several countries.

By emphasizing a close link between development theory and the actual practice of economic policymaking, Rapley grounds the discussion in a way that will be much appreciated by both students and their instructors. Others may find this account somewhat antiseptic because it downplays the role of domestic politics, ideology, and international financial institutions, but any 200-page work that covers the waterfront of development must compromise somewhere.

Whereas Rapley's account is somewhat more neat than the underlying phenomena it portrays, the book by Mittelman and Pasha is more chaotic. In part this is because their work is centered more around problems and policies than theory, but it also is organized much more loosely. In fact, though this second edition is coauthored, the overall structure and feel of the book remains that of a personal journey by Mittelman, the single author of the first edition. It begins with his own earliest exposure to the Third World in an entertaining six-page introduction to the first edition, and observations from Mittelman's African fieldwork pepper the remainder of the work. The book champions a structural perspective rooted in theory, but an unconventionally eclectic approach is used to elaborate it. The result is hit-and-miss. For example, chapter 1 decries the simplistic understanding of the Third World proffered by public media, but it is not enhanced by a two-page excerpt from the 1977 interview with Fidel Castro by Barbara Walters, which centered on personal questions about marriage and family. Yet, the critique of modernization theory and explanation for the rise of dependency thinking in chapter 2 is enlivened by a three-page excerpt from the work of a Ugandan poet. The critique of international organizations, foreign aid, and transnational corporations in chapter 3 will seem tired to experienced readers, but these familiar points are made in a spirited way that will capture the imagination of those new to this way of thinking. Chapter 4 introduces the rudiments of Marxist ideas on development in an accessible way.

Extended case studies rather than idealized models are used to portray the three strategies of accumulation open to Third World nations: "Join, leave, or weave." Chapter 5 illustrates the option of joining global capitalism with a fine description of the Brazilian "miracle" (1967–74) that emphasizes its inequitable distribution and unsustainability. The historical narrative sketches the events and conditions that preceded this justly (in)famous episode and chronicles the developmental tragedies that have ensued. Unfortunately, the account of Brazilian policy swings over the last 50 years may tend to confuse readers seeking a single model of what it means "to join." Furthermore, the only new chapter in this edition (the sixth, drafted by Pasha) offers the "miracle" of the Asian NICs as a variant on this option, downplaying the diversity of policy approaches undertaken across these cases and over time. Several theories purporting to explain their success are skillfully delineated, but this exposition gives no hint of the trouble to come for these East Asian success stories. As does nearly every other commentator, Pasha has reservations about this developmental model that are much more related to its social and political dimensions than to its economic sustainability. (One wonders how much more pessimistic their assessments would be today, when the East Asian miracles appear not merely untransferable but, even in themselves, temporary and illusory.) His forecast that this model will not be widely applied elsewhere—because, as Pasha adeptly explains, the conditions favoring success were unique in time and space—will surely fare better. Chapter 7 explores the "exit" option, using as an illustration China under Mao, with a brief discussion of the "reentry" under Deng. Mozambique is an example of the alternative path of

"weaving through global capitalism" in chapter 8. Unfortunately, much of this treatment seems dated now, and the theoretical and/or policy lessons to be drawn from these historical narratives are not clearly articulated.

The divergence between the two books is captured by Mittelman's observation that "neophytes seek an account that de-mystifies the issues, while many experts write about what their fellow experts say about the topic." Rapley does indeed write about what the experts say, but he does so with a rare clarity that makes accessible a literature teeming with the contributions of theorists, area scholars and policy analysts, and the perspectives offered by economists, political scientists, and sociologists. He injects greater analytic depth than is usually found in a survey of such diversity, and yet he does it in a compact work that should prove very attractive to both the student and the nonspecialist. The latter especially will appreciate the frequent references to historical events and cases as well as the theoretical snippets dotting the book. While the organization of *Understanding Development* is surely theory-centric, it by no means short changes policy practice. Many of these references are too brief to inform an inexperienced reader fully, but they will provide a helpful grounding for the informed nonspecialist and fodder for expansion by classroom lecturers. Furthermore, ample footnoting points the way to most of the classics of development theory. By contrast, the complete absence of footnotes limits the value of *Out from Underdevelopment Revisited* as a springboard to further reading, despite a lightly annotated bibliography at the end.

Important similarities in the messages of these books are revealed by the way they conclude, speaking to the current state of development theory and practice while looking ahead to the future. Both close with more hope than despair but also with more questions than answers. Rapley ends with the forecast that development theory has now reached a revolutionary phase marked by the search for a new paradigm, but he also suggests that profound cultural differences limit the universality of any development theory. The final section heading of *Understanding Development* asks: "What is the future of leftist thought?" The ambiguous and optimistic answer is in the first sentence of the Introduction: "The left is dead; long live the left."

Mittelman and Pasha are even more emphatic on each of these points. The central mission of their book is to demonstrate that hopeful alternatives exist to neoliberal orthodoxy, most of which emanate from ideas associated with the Left. The choice among them, however, cannot be prescribed universally by theorists, most especially those from the North, but must be made "by those engaged in praxis under markedly distinct conditions and most directly in touch with the specific contradictions that beset their lives" (p. 242). Finally, *Out from Underdevelopment Revisited* ends with a chapter entitled "What Works in the Third World?" Despite their best efforts, neither volume has convinced this reviewer that the answer is not "not much."

A Morbid Fascination: White Prose and Politics in Apartheid South Africa. By Richard Peck. Westport, CT: Greenwood, 1997. 216p. $57.95.

Audie Klotz, *University of Illinois at Chicago*

For those only aware of Nobel Prize–winning novelist Nadine Gordimer, or Alan Paton's consciousness-raising classic, *Cry the Beloved Country* (1948), Peck's *Morbid Fascination* will serve as a useful introduction to a diverse array of English-language writers, not least the Tom Clancy of southern Africa, Wilbur Smith. Indeed, to find any patterns across such various styles and subject matters is an achievement. Yet, Peck's thesis, that these and the other writers he covers exhibit both a fascination with and a distaste for politics, falls short of the deeper analysis warranted by his subject.

Peck somewhat apologetically limits his study to white writers in English. The book devotes separate chapters to Smith, Gordimer, and mystery writer James McClure; other chapters cluster a few novelists together under common themes: mysteries, liberalism, antiapartheid, existentialism, and theater. Included are Herman Charles Bosman, Wessel Ebersohn, June Drummond, Christopher Sherlock, Bryce Courtenay, Shawn and Gillian Slovo, Laurens van der Post, Dan Jacobson, Phyllis Altman, Mary Benson, and Athol Fugard. It is hoped that readers will be inspired by Peck's overviews to explore many of these authors in the original.

One can always question the exclusion of particular names from such a broad list, but especially given the inclusion of Fugard and performance artist Pieter-Dirk Uys, the decision rules remain murky. Certainly, André Brink, best known for *A Dry White Season* (1979), deserves more attention. Additional authors who also bridge the English-Afrikaans divide, such as J. M. Coetzee, warrant a separate chapter. Inclusion of black playwrights also calls into question the proclaimed focus only on white prose, especially in theater which frequently sought the same audience and market. Nonetheless, Peck's work should be evaluated on its own terms.

In his discussions of various writers, Peck focuses on their implicit and explicit views in order to identify a common "morbid fascination," in essence an ambivalence about politics. Unfortunately, this analysis is not sufficiently literary to add significantly to our understanding of the texts. Organizing the discussions more around themes might have enhanced the work. For example, Peck indicates a number of political myths, and he could have traced these more systematically across authors.

More disappointing for political scientists, Peck could have illuminated more strongly the context in which these authors were working. Only in passing does he acknowledge the constraints of apartheid (pp. 9–10), a system of institutionalized racial discrimination which relied in part on some of the harshest censorship laws in the world. Displeasing the government could result not only in one's work being banned from store and library shelves, but also in the author being restricted from speaking publicly, being quoted in the press, and meeting with more than one person at any time. Theater productions were frequently shut down. And, of course, many opponents of apartheid served prison sentences. Under these circumstances, it is difficult to imagine writers having opportunities to express their political views openly.

Peck fails to explore the possibility that the ambivalence about politics that he identifies across these works is an artifact of such repression, rather than the result of common political views of the authors or, more generally, liberal English-speaking whites in South Africa. Readers should turn to Brink's *Writing in a State of Siege* (1983), for example, for insights into these dynamics. Gordimer (*The Essential Gesture,* 1988) and others have also penned essays on the difficulties of working under apartheid restrictions. Literature produced in the postapartheid era will help us sort out the role of previous censorship and repression in the arts.

In addition to underemphasizing domestic political constraints, Peck only hints at the importance of international criticism of apartheid. Publishers are global businesses and thus sensitive to the reception of South African authors overseas. Granted, cultural boycotts affected theater and some films more than literature. But it remains unclear just

how much Gordimer's particular knack for explaining the ironies of apartheid to foreigners is responsible for her international stature. Perhaps international criticism of apartheid drove writers to be more explicitly political than their own proclivities; in any case, authors were unavoidably evaluated on their political views. Such a politicized context for the arts surely had an effect on literary content. Although Peck raises some of these issues indirectly, questions at the intersection of political economy and culture deserve more sustained attention.

A Morbid Fascination stakes out important territory for gaining a better grasp of South African politics. Literature, arts, and sports played critical roles in both the nationalist ideology that underpinned apartheid and the struggles against it. It is hoped that others will follow in Peck's footsteps but travel farther in exploring more fully the relationship between culture and politics.

The Grand Experiment: Debating Shock Therapy, Transition Theory, and the East German Experience. By Andreas Pickel and Helmut Wiesenthal. Boulder, CO: Westview, 1997. 262p. $53.00.

Anders Åslund, *Carnegie Endowment for International Peace*

The subtitle of this book describes its contents accurately. It deals with the postcommunist transition in former East Germany and related themes, such as shock therapy, democracy, interest representation, myths, and ideology. The political scientists Andreas Pickel and Helmut Wiesenthal have written five articles each; they collaborated on the introduction and the conclusion.

The East German experience is astounding. Between 1990 and 1995, the average monthly wage rose from one-third of the West German level to three-quarters. Is this not an economic wonder? "No," the authors firmly respond, because the rise was accomplished through injection of an incredible $800 billion from West Germany until 1996. But is this not an extraordinarily idealistic deed, and are not the East Germans grateful? Again the answers are negative. The East German economy collapsed in the process, and unemployment rose higher than in any other postcommunist country. The questions are: Why did East Germany do so badly despite massive resources, and what is the broader relevance of the East German transformation?

The main contribution of this book is a substantial critique of the political economy of the East German transition, primarily delivered by Helmut Wiesenthal. His judgment is that the German debate about the transformation of a former communist country was badly underdeveloped, as these problems were not taken seriously. He blames mainly two policies for the economic damage: the restitution of property and wage policy. The restitution of millions of properties spawned legal disputes that stalled the usage of much property for years, and sharp wage hikes swiftly priced much East German labor out of the market.

Wiesenthal's main original contribution is the point that this occurred because of "the way problems were defined, the range of policies considered, and the criteria employed for decision-making," which "bear a strong bias in favor of West German policy traditions and interests" (p. 60). His best example is the wage settlement. The decisive agreement was made in the metal industry between all-German trade unions and employer associations in early 1991, and Wiesenthal concludes that neither party "acted on behalf of predominantly East German interests" (p. 62). Neither cared that their agreement effectively put most of East German industry out of business, and the German government was not directly responsible. A similar example is the medical associations, although Wiesenthal finds that housing associations and farmers' associations accommodated East German interests. Wiesenthal evokes an image of strong but inconsiderate West German interest groups and weak and confused East Germans. Formal democracy persisted, but that did not solve the problem. The end result is alienation and unnecessarily high social costs. No one really wanted a bad outcome, but far too few gave much thought to what would result.

Andreas Pickel tries to put East Germany into the broader context of postcommunist transformation. His premise is that "the former GDR represents an ideal empirical test case for the strengths and weaknesses of the radical strategy" (p. 73). He goes on to argue: "Rapid and comprehensive reform has not created the conditions for self-sustained economic development, as had been expected" (p. 73). A long criticism of shock therapy ensues. This line of argument fails to convince. The first problem is that Pickel interprets shock therapy as holistic social engineering. He takes that point from opponents of shock therapy, but the proponents of radical market reforms are against any detailed future plan and instead see their strategy as a constructive first step in the reform process.

The second problem with Pickel's criticism is that he argues shock therapy took place in East Germany, which Wiesenthal disputes. The outstanding peculiarities of the East German transformation were that wages were regulated far above the market clearing level and that East Germany was flooded with Western social transfers. These are not market economy peculiarities. Although the government was center-rightist, the essence of this policy was social democratic. The East German situation is best described as a social welfare trap: People are so well paid through social security that it would be irrational for them to work, but they regret not having meaningful activity. Therefore, Pickel's contention that East Germany is a special case of shock therapy and holistic social engineering is rather pointless, because its social democratic kind of policy is not the ordinary meaning of shock therapy. East Germany remains a peculiar case of postcommunist transformation, because only the West German government has so much money to spend, and in the future we are likely to see this as foolhardy waste.

Pickel also discusses whether radical market reform is compatible with democracy. He observes that several political scientists, notably Jon Elster, Claus Offe, and Adam Przeworski, doubted early on that democratization and marketization could be combined, but they have been proven wrong. Populism has not turned out to be a major problem. Nor would a reversion to authoritarianism improve the chances for successful economic transformation, since the real threat has largely proven to be the old elite. Yet, despite very limited evidence, Pickel implies that neoliberals would be inclined toward authoritarian policies. His strongest quotes come from Silviu Brucan, whom I would place in the communist camp. A substantial literature in the last decade suggests that democratization and radical market reform are positively correlated.

Since this is essentially a political science book, economics is treated in the margin, but radical reforms have proven more economically successful than gradual reforms. The excessive social costs of radical reforms anticipated by many leftist political scientists never arose, while gradual reforms have turned out to be extremely costly. Hence, radical reforms and democracy have gone hand in hand, as have minimal reform and dictatorship.

The book is well written, and the authors are well read, thoughtful, and open-minded; a useful bibliography is included, as well as an index. A drawback is that both authors tend to discuss the same issues, making the book rather repetitive. Yet, to my knowledge it is the best work in English to date on political aspects of the East German transformation.

Overlooking Nazareth: The Ethnography of Exclusion in Galilee. By Dan Rabinowitz. Cambridge: Cambridge University Press, 1997. 222p. $59.95 cloth, $19.95 paper.

Sammy Smooha, *University of Haifa*

Rabinowitz's study has two levels. On the micro level, it is a well-documented, insightful, and readable ethnography of the troubled encounters between Arabs and Jews in an Israeli town. It is less satisfying on the macro level of analyzing the problematics of the status of the Arab minority in the Jewish state.

The town of Natzerat Illit was founded in 1957 on expropriated lands from residents of Arab Nazareth. It was officially built as an exclusively Jewish town and as part of the national project of Judaization of the Galilee. Due mostly to shortage of housing, Arabs from Nazareth, mostly middle-class Christians, moved to Natzerat Illit during the 1970s and 1980s (constituting 13.5% of the total population of 21,800 in 1989).

Rabinowitz, an Israeli critical anthropologist, did fieldwork for his doctoral dissertation in Natzerat Illit in the late 1980s. His book is not a comprehensive community study but a focused ethnography of the relations between the Arabs and Jews in the town. The area is conceived of as a frontier, where the Jews enjoy special opportunities and advantages, and the two sides compete and clash. The detailed accounts include the split real estate market, the separate educational system and community center, and the hardships faced by three professional Arabs—a medical doctor, a basketball coach, and a local politician.

The leitmotiv in the analysis is the failure of Jewish liberalism. The Jewish residents of the town object to the Arab purchase of housing and feel uneasy about receiving services from Arab professionals. The Arabs react with resistance to the Jewish strategy of exclusion. Yet, the end results of the high potential for conflict are quite surprising. Despite public agitation and ideological opposition, hundreds of apartments are sold to the Arabs for the benefit of all, relations are amicable, and there is no violence. Jews avail themselves of the professional services of the Arabs, notwithstanding the ill-feelings of status reversal. These benign consequences are not appreciated by the author, who chooses to emphasize the various manifestations of exclusion and discrimination.

It is the author's point of departure that the Jews, like himself, subscribe to liberal democracy and values but fail to practice their liberalism in daily life. The thrust of the study is to expose the Jews as "bigoted liberals" (the title of Part 1 of the book) and to point to their intolerant attitudes and behavior and their rationalizations. The issue is whether this framework of liberalism applies to Israeli Jews, society, and state. This takes us to the macro level of the study.

Israel declares itself as a Jewish and democratic state. It is constituted by law as "the homeland of the Jewish people." By law, no political party can register or run an election list to the Knesset if it denies this principle. The state's majority, symbols, calendar, institutions, culture, heroes, policies, and some of its laws are explicitly or implicitly biased in favor of the Jews. At the same time, Israel is a democracy, extending individual and collective rights to the Arabs. These rights cannot be full and equal because of the Jewish-Zionist nature of the state and its preferential treatment of the Jews. There is a genuine contradiction between the Jewish and democratic nature of the state, which incessantly generates numerous insoluble dilemmas.

Israel is neither a liberal nor a consociational democracy. It is not liberal because the state is not neutral and does not treat Arab and Jewish citizens equally. It also does not try to form a new Israeli nation composed of both peoples. Neither is Israel a consociational democracy, because it does not treat Arabs and Jews as equal collectivities, and it does not institute any of the standard mechanisms of consociationalism (autonomy, proportionality, elite cartel, veto power, and the like). Rather, Israel is what I call an "ethnic democracy," which combines the contradictory principles of democracy for all with institutionalized dominance.

Liberalism is foreign to both Jews and Arabs in Israel. The Jews wish to keep Israel Jewish and democratic and oppose the assimilation of the Arabs, while the Arabs want to preserve their separate culture and identity but strive for equality without assimilation. The struggle is not about liberalism, which is rejected by both sides. Their clashes test the limits of the ethnic-democratic system and result in patchy compromises. Do Jews have the right to maintain a Jewish majority in the country as a whole? Do they have the right to build a Jewish majority in the central and western Galilee, where Arabs still constitute 70% of the population, and Jews fear Arab irredentism? Do Jews have the right to keep towns like Natzerat Illit Jewish? Do Jews and Arabs have the right to completely separate localities, neighborhoods, and schools? Do they have the right to object to intermarriage without being called "racist" or "bigot"? The answers to these questions are obvious only in a liberal democracy, but they are not clear in an ethnic democracy like Israel. To term the Jews "bigoted liberals" is to miss their inherent dilemmas. The Israeli Supreme Court is at present deliberating a case brought by an Arab who was barred from buying a unit in a building project in a small Jewish community. The ruling can go either way because of the dual nature of the state and the absence of unidimensional liberal discourse in dealing with such questions. The author claims to take the Jewish side "more seriously and less judgmentally" (p. 189). He does not. His ethnographic account airs many of these issues, however.

Aside from its misplaced liberal bias, this study deserves our attention for its fine ethnography and its touching on questions of ethnicity, nationalism, and democracy.

The Dynamics of Conflict in Northern Ireland: Power, Conflict and Emancipation. By Joseph Ruane and Jennifer Todd. Cambridge: Cambridge University Press, 1996. 365p. $59.95 cloth, $19.95 paper.

Arthur Aughey, *University of Ulster*

This book achieves a rare distinction in studies of the political conflict in Northern Ireland. It scrupulously avoids a polemical tone and almost never moralizes. For the most part it assesses the claims of the parties to the conflict in the manner in which they should be assessed—unsentimentally and with a coolly rational eye. Unsentimentality and cool rationality, however, do not prevent the authors from showing a sympathetic understanding of the conditions (or "structures," as they put it) in which such claims are made. Ruane and Todd achieve what they set out to do, which is to take seriously the points of view of Unionists and Nationalists while subjecting

their assumptions to critical scrutiny. It is a measure of their success in this endeavor that the book can take its place alongside the late John Whyte's magisterial *Interpreting Northern Ireland* (1991) as essential reading on the subject.

The enormous and patient labor of John Whyte distilled the academic literature on Northern Ireland into a set of propositions which have been not only accessible to students but also persuasive to academics. Ruane and Todd's study is an equally richly textured work of synthesis drawing on a wide body of literature, and like Whyte, they offer well-judged views without being self-righteously judgmental. They are broad minded and tolerant, providing the reader with a clear-sighted guide to the conflict in concise, yet subtle and intelligent, surveys of what the authors call the "dynamics of conflict"—economy, culture, politics—and what might be called the external context of the conflict—Great Britain, the Republic of Ireland, international opinion, and especially the views of opinion leaders in the United States and the European Union.

The great strength of the book lies in its realism (in the sense that it gives priority to a calculation, past and present, of the relative power positions of Unionists and Nationalists). In the debate about the historical character of the Irish Question, the authors appear closer to the position of someone like A. T. Q. Stewart (*The Narrow Ground: Patterns of Ulster History*, 1977), who argues that the communities in Northern Ireland act according to patterns, even laws, inherited from the past, than to the position of someone like Brian Walker (*Ulster Politics: The Formative Years, 1868–86*, 1989), who proposes that these patterns are contingent, transient, and capable of significant change. Ruane and Todd are not absolute determinists, however. Theirs is a study of the enduring nature of communal conflict, though conflict which may take different historical forms and which need not be violent in its expressions.

Ten of the eleven chapters are written in this realist style. Ruane and Todd explore the first two elements of the book's subtitle, power and conflict, in which the continuity of political struggle is manifest. Only in the final chapter, and then only very briefly, is the concept of emancipation examined, a concept which intimates the possibility of qualitative change in the relationships between the two communities. The authors' "emancipatory approach to the conflict" represents their distinctive contribution to a "resolution" of the Northern Ireland conflict. Unfortunately, this is the weakest part of the book.

The language of most of the book, the language of "power and conflict," is generally taut, the arguments are well focused, and the explanations, though contestable, sharp. The language of "emancipation," by contrast, is vague, and the arguments are abstract and imprecise. This is not to say that the vision of a realm of freedom in which "new social and cultural potentials could be realized" (p. 307) is not an attractive one. But it is difficult to see, precisely, how the people of Northern Ireland, aware of themselves existing in a political condition not unlike Carl Schmitt's notion of a struggle between friends and enemies (*The Concept of the Political*, 1976)—Ruane and Todd's own words for this prevailing understanding are "a structure of dominance, dependence and inequality" (p. 107)—might willingly and together enter this realm of freedom. The logical jump from power and conflict to emancipation seems too wide.

For instance, Ruane and Todd are most persuasive and insightful when they explore the ways in which the cultural, symbolic, and mythical aspects of Unionism and Nationalism can be, and have been, formulated and reformulated in reasoned defense of substantial communal interests. And they are sound in their criticism of the rationalist illusion of expecting—once having shown how mythological, irrational, and anachronistic ideological beliefs are—that these beliefs subsequently will fade away. Yet, in their desire to outline the lineaments of a "resolution," Ruane and Todd themselves adopt ultimately a rationalist perspective. They appear to assume that politicians and those they represent are capable of recognizing a common good which transcends the traditional and partial objectives of the respective constituencies. "Appear"—for it is not at all clear what they are proposing. Another reading of their position would be this. Since, as they claim, "the immediate source of conflict is the nexus of Northern Protestant dependence on the British state and dominance over Northern Catholics" (p. 309), then this dependence and dominance ought to be broken. Northern Protestants are the source of power and conflict. They ought to be forced to emancipate themselves. This might lead to a resolution of sorts but only of the sort which supposes that resolution can be achieved as the crow flies by Protestants being compelled to "see reason." The lack of clarity here may be the result of the authors' unwillingness to admit that conclusion. A settlement (rather than a resolution) is possible in Northern Ireland, but it is unlikely to be achieved in "emancipatory" terms (whichever reading one takes).

Despite its flawed conclusion, Ruane and Todd's book is full of insight and realist wisdom. It remains one of the best contributions to the study of Northern Ireland politics.

Parties and Their Members: Organizing for Victory in Britain and Germany. By Susan E. Scarrow. Oxford and New York: Oxford University Press, 1996. 277p. $72.00.

Alan Ware, *Oxford University*

During the 1990s, the findings of a number of major research projects on party members and activists in Great Britain have been published. The most important have been Seyd and Whiteley's *Labour's Grass Roots* (1992), Whiteley, Seyd, and Richardson's *True Blues* (1994), and Norris and Lovenduski's analysis of the candidate selection process, *Political Recruitment* (1995). To this list can now be added Susan E. Scarrow's impressive first book, *Parties and Their Members*.

This monograph has two features that are especially worthy of attention. First, it provides an excellent model of how to conduct research in comparative politics. Scarrow examines four parties—the British Conservative and Labour parties and the German Christian Democratic and Social Democratic parties. For her purposes, these are probably the best parties to examine, for all are large and operate in party systems in which they must pursue vote-maximizing strategies. Second, Scarrow's focus is on how parties make use of members, rather than on how members conceive their own role, and on how that use has changed over time. (Unlike the other studies just cited, she does not make use of original survey data.) This enables her to address the much discussed argument that various changes, especially in campaign technology, have reduced the need of parties for a membership base.

Scarrow is able to demonstrate that the argument is false. The tasks that parties want members to perform have altered, but, if anything, the range of activities is broader. Furthermore, all the parties have tended to adjust the basis of their relationship with members; mediating party institutions have been replaced by more direct contact between party headquarters and individual members. This has several advantages for the former, especially in the mobilization of hitherto inactive members.

Nevertheless, although Scarrow notes that members are "valuable assets," events since the completion of her book indicate that the management of these assets may be getting more difficult. Even in parties with a strong tradition of deference by members to the decisions of the leadership, there are demands for membership input into certain aspects of party decision making. Thus, one of the serious disputes among Tories surrounding the election of the new party leader, William Hague, in 1997 was the lack of any formal role accorded to the members. In 1965, when the Conservatives abandoned their traditionally informal methods of selecting a leader, the membership neither demanded nor was given any part in the process; the matter was left to the parliamentarians, and this procedure generated no controversy within the membership at the time. The more recent experience conforms with Scarrow's conclusion that the trend has been toward a kind of intraparty democratization.

More serious still for party leaderships is the question of how to provide membership input into the setting of public policy objectives, so that members believe they really have a stake in the party, without leaving the leadership at the mercy of decisions that may make them unelectable. The problem lies not so much in possible issue-extremism among members; rather, even moderate members may take a somewhat different view of party priorities from their leaders. Thus, by the end of 1997 the Labour Party was pushing ahead with long-term plans for the greater involvement of members in discussing policy, at a time when it was becoming clear that there was considerable disquiet among members about the Labour government's policies, particularly the reduction in state benefits to single parents. Anxious to avoid the internal wrangles of earlier decades, the party had already made various moves to prevent members from embarrassing it, notably in debates at the Annual Conference; yet, it now faced a problem of how to hold members who lacked the emotional commitment to the "Labour movement" of their predecessors and who would only persist with the party if they believed it were pushing public policy in the general direction they wanted. By late 1997 there were already a number of signs of declining membership in the Labour Party.

The contrasting experiences of the German Christian Democrats and the British Conservatives suggests that the Labour Party would be right to worry about any hemorrhaging in its membership base. A really large membership is not necessary—one of the many points Scarrow makes is that successful membership parties have often not had a large base, in not the sense suggested by "mass membership party"—but a minimum level of support is required to be effective. By the early 1970s the Christian Democrats had become sufficiently worried about getting their voice heard in communities—the "spirals of silence" problem—that they embarked on membership recruitment. They returned to power within a decade. Throughout their years in government (1979–97), the Tories did nothing to reverse a membership decline that began in the early 1960s. In their heyday, the Conservatives had nearly 2 million more members than (nonunion-affiliated) members of the Labour Party, but by the late 1990s that advantage had been reduced to, at most, a few hundred thousand. Furthermore, the average age of Conservative members now exceeds 60, so that the medium term prospects for a strong membership base appear bleak.

Scarrow's conclusion, however, that "parties are transforming themselves into something quite different from the hierarchical and disciplined mass/branch parties which Duverger described" (p. 210), indicates that successful regeneration of the Conservatives may well require a significant transformation in how that party operates. In turn, as her conclusions suggest, that will affect the entire British political process. Consequently, she is surely correct in believing that the study of membership parties remains a topic of central concern for political science; the spate of research on the subject in recent years is not a fad but a recognition of the importance for the polity of the member-leader relationship in parties.

Rituals of Conflict: Religion, Politics and Public Policy in Israel. By Ira Sharkansky. Boulder, CO: Lynne Rienner, 1996. 172p. $49.95.

Eliezer Don-Yehiya, *Bar-Ilan University*

The stated aim of Sharkansky's book is to analyze patterns of conflict on the role of religion in Israeli politics. The author argues that the controversy over religion and state in Israel has a ritualistic nature. It is an ongoing conflict in which the parties involved seek to avoid clear-cut decisions, and neither of them achieves the upper hand. This accounts for the low intensity of confrontations in this area, while many of the problems remain unsolved. As a result, conflicts arise time and again around the same basic issues, although they appear in various forms and versions.

Sharkansky also claims that in those respects, and in others as well, Israel rather than being a unique case follows the pattern of religious politics in many other democracies. He further argues that there is a continuity in the patterns of religious conflict throughout Jewish history, from Biblical times to modern Israel.

I agree with Sharkansky's argument that the basic problems inherent in the nature of relations between religion and state in Israel remain unresolved and undecided, which does not preclude cooperation and agreements on a pragmatic basis between the contending parties. What seems more problematic is the author's claim that the religious conflict in contemporary Israel resembles conflicts on the role of religion in other democracies or in previous periods of Jewish history.

As for the first point, there is indeed a certain resemblance between the management of religious conflicts in Israel and in some of the West European countries, especially those called "consociational democracies" by Arend Lijphart. Thus, in Israel, the issue of religious education has been solved on the basis of the same "consociational" devices that served for the resolution of the "school's issue" in Belgium and the Netherlands: the allocation of governmental resources on a proportional basis to an autonomous system of religious schools. In the West European countries, however, by resolving the education problem the religious issue is no longer a major source of conflict. By contrast, in Israel there are major religion-state issues in addition to education. Several of these such as the role of religion in defining the content of Jewish national identity, cannot be resolved by resorting to the same methods employed to solve the problem of religious education. There is much to be said in this regard. Unfortunately, there is no real effort at a systematic comparison between Israel and other democratic states in Sharkansky's book.

The author devotes much more space to demonstrating his other point: the continuity in patterns of religious conflict in Jewish history. In order to prove his point, Sharkansky points to cases in the Biblical era as well as later periods, but he ignores or deemphasizes the basic differences between religious controversies within Jewish society before and after Jewish emancipation began in Western Europe in the early nineteenth century. The most notable difference is that until the beginning of the emancipation period, underlying the

various controversies was a basic consensus within Jewish society on the commitment to the traditional Halakha (Jewish religious law) as the supreme authority in all spheres of life. That consensus has been undermined in the postemancipation period, which lends the later conflicts a very different nature.

The establishment of the State of Israel has introduced another important change in the pattern of religious conflicts in Jewish society. Many issues which were once theoretical became sources of an operative political conflict with the establishment of statehood. Thus, the problem of religion and national identity has been controversial ever since the beginning of the emancipation era, but only with statehood did intense political conflict arise over the appropriate criteria for designating a person as a "Jew" in the Israeli passport.

All this does not mean to say there is no continuity between religious politics in modern Israel and earlier times, but in his efforts to stress similarities, Sharkansky does not give adequate consideration to the unique nature of religious conflict in Israeli society today. This is related to a major problem with the book. The author's stated aim is to deal with the effect of religion on politics and public policy in Israel, and this aim is manifested in the main theoretical arguments presented in the Preface. But the author does not elaborate on his theoretical points or apply them systematically by analyzing the main controversies over religion and state in contemporary Israeli society and patterns of conflict resolution in this area. Instead, he devotes considerable space to historical accounts of Jewish religion and its relationship to secular political authorities. There are many interesting points in those discussions, based on the author's interpretation of Biblical narratives and other sources of Jewish historiography and theology. Yet, those discussions are only marginally related to the author's stated aims.

There are, to be sure, chapters dealing with issues of religion and politics in contemporary Israel, and they contain interesting and illuminating accounts of confrontations between religious and secular views in the Jewish state. Instead of focusing on the "great issues," however, the author chooses to concentrate mainly on rather anecdotal incidents. Despite these reservations, Sharkansky's book is of considerable value to anyone interested in the important issue of religion and politics in Israel.

Reforming Justice in Russia, 1864–1996: Power, Culture, and the Limits of Legal Order. Edited by Peter H. Solomon, Jr. Armonk, NY, and London: M. E. Sharpe, 1997. 406p. $82.95.

Stanislaw Pomorski, *Rutgers University*

The book under review belongs to a genre all too common in academic life: A grant was awarded (in this case two), a conference was held, participants delivered papers, the papers were edited into a publishable form, and the end result is before us in hard cover. The genre has its strengths and weaknesses. Among the strengths, I count the relative ease with which such books can be quickly created as well as their "polyphonic" perspectives on a chosen subject. Among the weaknesses typically if not invariably exhibited by such projects is the uneven quality of individual contributions, their loose correspondence to the organizing theme, and their even looser mutual interconnections. In other words, there is an ever-present danger that polyphony can degenerate into cacophony.

Reforming Justice in Russia is in many ways typical. In addition to the introductory essay by the editor, who is an experienced researcher in Soviet legal history, there are fifteen papers written by as many authors from five different countries (Canada, France, Great Britain, Russia, and the United States, which accounts for the great majority). These are divided into three parts, each dealing with a different period of Russian history. Part one, "Varieties of Justice in Imperial Russia," has studies by William G. Wagner ("Civil Law, Individual Rights, and Judicial Activism in Late Imperial Russia"), Sergei M. Kazantsev ("The Judicial Reform of 1864 and the Procuracy in Russia"), Girish N. Bhat ("The Consensual Dimension of Late Imperial Russian Criminal Procedure: The Example of Trial by Jury"), Jane Burbank ("Legal Culture, Citizenship and Peasant Jurisprudence: Perspectives from the Early 20th Century"), and Cathy A. Frierson ("Of Red Rooster, Revenge, and the Search for Justice: Rural Arson in European Russia in the Late Imperial Era").

Part two, "Courts, Law and Soviet Power," is the largest segment of the volume. It consists of contributions by Lisa Granik ("The Trial of the *Proletarka*: Sexual Harassment Claims in the 1920's"), Golfo Alexopoulos ("Exposing Illegality and Oneself: Complaint Risk in Stalin's Russia"), Oleg V. Khlevniuk, ("The Politburo, Penal Policy, and 'Legal Reforms' in the 1930's"), Gábor T. Rittersporn ("Extra-Judicial Repression and the Courts: Their Relationship in the 1930's"), Peter H. Solomon ("The Bureaucratization of Criminal Justice under Stalin"), and Yoram Gorlizki ("Political Reform and Local Party Interventions under Krushchev").

Part three, "Justice and the Russian Transition," deals in principle with the post-Soviet period. There are papers by Eugene Huskey ("Russian Judicial Reform after Communism"), Gordon R. Smith ("The Struggle over the Procuracy"), and Sarah J. Reynolds ("Drawing upon the Past: Jury Trials in Modern Russia"). For reasons unclear to me, the editor also included in this section a paper by Todd Foglesong dealing with the late Soviet period.

The team of contributors is dominated by historians and political scientists; academic lawyers are in a small minority. Judging by academic rank, one notices a strong presence of younger scholars. Furthermore, with so many contributors, a review cannot give a fair and full evaluation of the collection. The observations and comments offered below are not meant to be exhaustive.

The book addresses an interesting subject of immense breadth and great practical importance in view of current developments in Russia. The scope and multiplicity of contributors call for a disciplined focus on the organizing theme and, accordingly, selectivity in choosing topics of individual papers. In this regard, the volume leaves something to be desired. The crucial concept of judicial reform is never defined, let alone developed. Obviously, not every change in law, legal policy, or legal procedure deserves the name "reform." Such usage would trivialize the very concept and ultimately would render it meaningless since the universe of law constantly undergoes *some* alteration. The term should be reserved only for a fundamental change, a change having a holistic quality to it and as a rule following a blueprint developed in advance.

This lack of conceptual clarity about the organizing theme took its heaviest toll in the second part. One is at a loss to see what kind of judicial reform is to be the focus under the not particularly telling title, "Courts, Law, and Soviet Power." Are we to believe that the whole Soviet period was filled with one, continuous judicial reform? The contributions grouped here (some of them quite informative in their own right) have little to do with a judicial reform. Yet, a student of legal

reforms in Russia could find an important subject for investigation during the Soviet era: a major effort to reform Soviet administration of justice under the auspices of Krushchev, who saw it as a part of his "destalinization" campaign. Krushchevian law reform at the time generated substantial interest in the West, particularly in the United States. It certainly deserves an analysis from the perspective of the 1990s. Unfortunately, the book almost entirely ignores the subject save for one quite thoughtful paper, however limited in scope, by Yoram Gorlizki.

The quality of the contributions is very uneven. Several represent first-class academic work, to mention only the studies by Burbank, Frierson, and Reynolds; they are well researched, well reasoned, and full of original insights. My favorite in the volume is the piece by Frierson, who ably demonstrates how rural customary standards regarding arson (particularly a distinction that peasants made between "moral" arsons, as justified retaliation, and wrongful arsons) were injected into the formal process of guilt determination by Russian juries. The thoughtful article by Sarah Reynolds deals primarily with trials by jury in contemporary Russia, but she transcends her narrow subject and makes interesting observations about interconnections with reform in the 1860s. Regarding her specific subject, Reynolds convincingly argues that for now, and contrary to high expectations, the institution of jury is of marginal importance at best. The paper is partly based on empirical investigation in the Saratov region, and much more of this type of research on the administration of justice in contemporary Russia is badly needed. We need to find out what is happening "on the ground," in the provinces, at the district and regional level. Empirical work geared toward such findings appears to me much more valuable than somewhat exaggerated preoccupation with the Byzantine high politics of Moscow bureaucracies. From the bits of information contained in the book, particularly in a paper by Huskey, it appears that the Russian courts are close to catastrophe (see p. 337).

Regrettably, several contributions to the volume are quite pedestrian, flatly descriptive. Furthermore, I had difficulty grasping the essence of the "consensual dimension" of late imperial criminal procedure suggested by Bhat. He claims that "consensualism" was a uniquely Russian feature of criminal procedure. In support of his theory, he points to such characteristics as presentation of evidence by the presiding judge, the limited role played by the parties, the restrained style of the prosecutor, the right of the defendant to comment on individual pieces of evidence, and the somewhat informal atmosphere of the trial, allowing for a relatively free dialogue among the participants (pp. 67, 74, 75, 77). I can discern in those characteristics nothing "consensual," since defendants often contested their guilt and the cases were decided not by agreements between the opposing parties but by verdicts of an independent adjudicator. Nor were these arrangements uniquely Russian; they can be found at the core of continental European criminal procedure, as may be readily discerned by examining basic comparative law literature on the subject. (See, for example, John H. Langbein, *Comparative Criminal Procedure: Germany*, 1977.)

On balance, despite its weaknesses, the book makes a useful contribution to our knowledge about the administration of justice in Russia, old and new. The contribution would have been even greater with a better focus on the key concept of judicial reform and a stronger empirical orientation on the current postcommunist period.

Democracy without Equity: Failures of Reform in Brazil. By Kurt Weyland. Pittsburgh: University of Pittsburgh Press, 1996. 293p. $44.95 cloth, $22.95 paper.

Peter R. Kingstone, *University of Vermont*

During the 1990s, scholars have become increasingly skeptical of the prospects for consolidation and deepening of democracy in Latin America. In particular, they have observed that the new democracies there appear to be hybrids that have grafted authoritarian and/or clientelist institutions onto democratic ones. This literature has provided ample and persuasive empirical evidence to support skepticism. Yet, as Guillermo O'Donnell has noted ("Illusions about Consolidation," in *Consolidating the Third Wave Democracies*, ed. Larry Diamond et al., 1997, p. 45), the causal processes at work in these new democracies have been undertheorized.

Kurt Weyland makes a valuable contribution to understanding this problem. He asks what accounts for the virtual absence of redistributive reform in Brazil. He notes that he frequent response—elites have managed to retain their power—is an accurate description, but "it remains unclear why and how elites could hang on to their power, despite a profound revamping of the rules of the political game" (p. 3).

By treating democracy as an independent variable, Weyland is able to assess how and to what extent the regime change affected redistribution. To make his case, Weyland traces reform efforts in three areas: tax policy, social security, and health care. The empirical analysis covers the authoritarian administration of General Ernesto Geisel (1974–79) as well as democratic presidents José Sarney (1985–90), Fernando Collor (1990–92), and Itamar Franco (1992–94).

Weyland argues that organizational fragmentation within both the state and society severely hindered reform efforts. This fragmentation promoted clientelism and segmentalism over universalism and enhanced the power of privileged groups at the expense of the poor. Weak, clientelist political parties and a factionalized civil society left the poor dependent on state-led redistribution, but reformers within the state repeatedly saw their efforts severely watered down or stymied by bureaucratic in-fighting and intense lobbying from privileged groups. As a consequence of these enduring institutional factors, Brazilian democracy has produced virtually no equity-enhancing reforms.

Weyland contrasts this "institutionalist" explanation with rational choice and socioeconomic arguments. Rational choice, Weyland argues, points to a logic of collective action that is biased against the poor in general and the rural poor in particular. Socioeconomic arguments, notably dependency theory, point to the structural limits of external dependency. In response, Weyland provides empirical evidence that contradicts the expectations of both sets of explanations. Moreover, he argues that both need to understand how enduring institutional factors frame rational incentives and filter socioeconomic limits.

Weyland seeks to bolster his argument by placing Brazilian reform failures in a broader comparative perspective. His concluding chapter offers explanations of the relative successes of equity-enhancing reforms in modern Costa Rica and Chile as compared to the rest of the region; during the populist period in Latin America as compared to the current period; and in the Western European cases as compared to the United States. Although cursory by necessity, this concluding analysis is well done and convincing. Perhaps the strongest part of this chapter is Weyland's interpretation of the populist era in Latin America. Noting that organizational fragmentation can vary over time, Weyland views this period as a window of opportunity in which organizational fragmen-

tation was still low (prior to the onset of corporatist interest organization), while clientelism was diminishing (sparked by industrialization and urbanization).

Weyland makes a good case for his institutional explanation, but the argument could have been stronger had he given more consideration to his rival explanations. He repeatedly notes that socioeconomic and rational choice explanations must be set in an institutional context, but there is no systematic consideration of the ways in which they might interact. Socioeconomic factors and strategic choices frequently appear in the empirical discussion as critical determinants of central actors' behavior, but in an *ad hoc* fashion.

For example, Weyland charts in some detail the struggles among various bureaucratic agencies over tax and social security reforms. While business lobbies played a role in this conflict, so did bureaucrats' strategic decisions about how to promote development. Thus, reformers in the 1990s under presidents Collor and Franco confronted a conflict between redistribution and enhanced competitiveness of the private sector. Similarly, the empirical discussions are laced with references to politicians' strategic choices about how to mobilize support. In dismissing his rival explanations, Weyland frames rational choice as a problem of collective action and socioeconomic factors as external dependency. Viewing them in terms of strategic choices—about mobilizing political support or promoting development—may have helped him think theoretically about how strategic considerations interact with institutions to affect equity.

Despite these concerns, this remains a strong and important book. First, Weyland provides an exemplary model for conducting qualitative research. Several things stand out in particular. The argument rises out of a dense, rich lode of empirical research, including almost 300 interviews. Weyland also takes great care to define the key concepts, from "democracy," to "redistribution," to "organizations." Finally, he has traced very carefully the possible causal links between his independent and dependent variables.

Second, Weyland has effectively problematized the task of analyzing new democracies. The abundant and growing literature on democratic consolidation and deepening has convincingly shown the importance of norms, behavior, efficacy, and equity. Now this literature needs to focus on *why* these new democracies have failed to live up to earlier, guardedly optimistic expectations. *Democracy without Equity* is a provocative and persuasive contribution.

Party vs. State in Post–1949 China: The Institutional Dilemma. By Shiping Zheng. Cambridge: Cambridge University Press, 1997. 294p. $59.95 cloth, $19.95 paper.

Yu-Shan Wu, *National Taiwan University*

Although area studies in general are not prone to produce grand theories, there has been no lack of analytical models in China studies. Some of the most prominent examples deal with factionalism, bureaucracy, and tendency analysis. Most of the models specify major political actors, ascertain goals pursued by the actors, and delineate rules of the game. Historical cases are then brought in for analysis through the perspectives of the models.

Shiping Zheng's book follows this tradition. Though Zheng claims to use the "neo-institutional" approach, his analytical framework boils down to a bureaucratic model, with the Chinese Communist Party and the PRC state as the two major actors. The party is defined in two ways. It is a complicated organizational structure that includes the central and provincial committees and their work departments, party core groups in state institutions, the Politburo and its Standing Committee, and all the key members in these party units. The party is also defined in terms of its political ideology, organizational principle, and the revolutionary tradition of guerrilla warfare and mass mobilization (p. 19). The state refers to the legislative, judicial, administrative, and military institutions and those who manage them (p. 18). Unlike ordinary bureaucratic analysis, however, this book does not assume institutional interest as the goal pursued by the actors. It is claimed that, under the heavy influence of more than two decades of civil war, the Party has developed a distinct "organizational rationale and logic" (p. 255), which lent the Party easily to Mao Zedong's call for continued class struggle under the dictatorship of the proletariat. The state leans toward legislative supremacy, judicial independence, and separation of the party from the government (and the military). Thus, there are structural conflicts between the party and the state it built. Zheng disintegrates the concept of "party-state" and claims that the two are analytically separate entities. This is part one of the book.

Zheng's conceptual innovation prepares the readers for a revisit of China's post–1949 political development in parts two and three. It is emphatically demonstrated that the party's organization logic made it a superb instrument for defeating the Kuomintang government and conquering China, but the same ethos was unsuitable for the new regime to acquire lasting legitimacy and revive the economy, hence the need for state building. Once the elaborate apparatus of the state was in place, the inherent tension between the party and the state emerged, leading to various efforts by the party to attack and replace the state, and the country was thrown into chaos. After the Cultural Revolution and Mao's death, Deng Xiaoping had a golden opportunity to restore the balance between the two, but he was still under the spell of the old revolutionary mentality. His failure to separate the two entities was shown vividly in the Tiananmen incident, in which the army was brought in to fight an intraparty power struggle. China's post–1949 history is thus characterized as the playing out of the inherent tension between the party and the state, which have different organizational logic and tasks.

Zheng has made a brilliant attempt to recapture the thrust of the PRC's history, but there are conceptual and empirical problems. Conceptually, it is difficult to differentiate between the party and the state. The Chinese followed the Soviet model in setting up an elaborate system of rule that places ultimate power in the party and establishes state apparatus as an instrument for party rule. The interlocking directorates (personnel overlap), party core groups, party work departments, and jointly issued directives are the major mechanisms through which the party exercises control over the state. They are intertwined organizationally, and government officials almost by definition hold corresponding posts in parallel party committees or core groups, so it is virtually impossible to separate the one from the other. The same can be said of party-military relations. Furthermore, the frequent transfers of personnel between party and state (including the military) make separation even more difficult. This being the case, the "party versus state" perspective is bound to meet difficulties.

There are empirical problems, too. Mao is treated as the main culprit who plunged China into chaos by insisting the party take over the state's functions, starting with the Anti-Rightist Campaign of 1957. The Great Leap Forward, the purge of Peng Dehuai, the Cultural Revolution, and the Tiananmen incident are all interpreted in terms of the party usurping the state's authority. In those cases, however, one finds that the line separating persecutors and victims was not drawn between the party and the state but between the

regime and the intellectuals (the Anti-Rightist Campaign); between Mao and his critics in the military (purge of Peng); between Mao and almost all the senior leaders in the party, the government, and the military (the Cultural Revolution); and between Deng, with his conservative supporters, and the nominal party boss Zhao Ziyang (Tiananmen incident). In most of the cases, the party itself was split, and so was the state. Victims and persecutors can be found on both sides (as long as a specific leader can be discerned as either a party or a state official at all). On many occasions, it was not a case of the party usurping state power but of Mao terrorizing his comrades in both arenas. With bureaucratic boundaries blurred in those cases, a Mao-in-command model, a tendency approach, or factionalism appears more proper for analysis than the bureaucratic model provided by the book.

Even though Zheng's theoretical thrust is questionable, there are nevertheless very interesting analyses of the PRC's political development, such as a discussion of the constitutional crisis of 1989. The book also touches on a wide range of institutional issues facing the post-Deng leadership, including administrative reform in the state council, establishment of the civil service, and the change in central-provincial relations. Toward the end, the author has these words for the opposition: "By separating the state from the Party, one can discover and pursue various actual and potential institutional alternatives to the Party rule" (p. 265). This shows that disintegrating the concept of the "party-state" may be a fruitful political strategy, but it may not be a proper analytical tool for investigating the history of the PRC.

International Relations

Wars, Internal Conflicts, and Political Order: A Jewish Democracy in the Middle East. By Gad Barzilai. Albany: State University of New York Press, 1996. 301p. $23.95.

Trial and Error: Israel's Route from War to De-escalation. By Yagil Levy. Albany: State University of New York Press, 1997. 282p. $18.95.

Yaacov Bar-Siman-Tov, *The Hebrew University of Jerusalem*

These two new books, which probably are based on doctoral dissertations, provide unique and interesting perspectives on the linkage between Israel's policy in the Arab-Israeli conflict and its domestic politics. Both studies emphasize that behavior in external conflicts should be seen as a complex interaction between external and domestic factors. Whereas Barzilai's purpose is to explain national consensus and dissent in Israel during war and lesser conflict, Levy analyzes Israel's security policy not only as a direct and inevitable reaction to Arab hostility but also as determined by social and economic interests of different elites and groups in Israel.

Barzilai's book is the first comprehensive study of the influence of the protracted Arab-Israeli conflict on Israel's domestic politics and society. Part 1 introduces the conceptual framework. Parts 2 and 3 examine how consensus and dissent gradually developed during the Suez Campaign, the Six Day War, the War of Attrition, the Yom Kippur War, the Lebanese War, the Gulf War, and the Intifada. Part 4 presents the study's findings in comparative and theoretical perspective.

The conceptual framework, which is very briefly set forth (pp. 3–15), is the weakest part of this important book. Although it presents the concepts of consensus and dissent, some of the political dilemmas involved in the use of force, and some review of the literature, it fails to provide an innovative and comprehensive theoretical framework. Such a framework would identify the main variables and present new propositions about the interrelationships between war and consensus or dissent. This might involve, for example, the problem of acquiring consensus for war, the conditions that enable such consensus, and the strategies employed by decision makers to acquire consensus or to minimize dissent. How does consensus or dissent influence the initiation, management, and termination of war? How may consensus or dissent influence the accomplishment of war aims? How do consensus and dissent differentially matter when the aims of war are offensive or defensive? Indeed, the empirical and the concluding chapters offer some ideas about these matters, as well as criteria for assessing consensus and dissent; but this material should have been presented in the theoretical chapter and could later have guided the examination of the case studies. In this regard, the final chapter is an excellent one that summarizes the cumulative ramifications of the Arab-Israeli conflict for Israel's politics and society, while arguing some theoretical and comparative propositions.

The choice of Israel as a crucial case study is clearly justified. As a democracy involved in a protracted conflict that has escalated into six different wars (defensive and offensive, choice and nonchoice) and into a good deal of smaller scale violence, Israel is a unique case that yields many theoretical and empirical insights. Because the characteristics of the Arab-Israeli conflict make Israel such a unique case, Barzilai provides for comparison some examples of other democracies in times of conflict and war.

Barzilai focuses both on the parliamentary plane (political parties) and the extraparliamentary plane (interest groups). This distinction enables him to discuss how wars and lesser violence have affected the development of the political regime. The wide consensus during the 1948–49, 1956, and 1967 wars, which produced the impression that wars necessarily bring about national unity, changed during the War of Attrition, the Yom Kippur War, and especially the Lebanese War. Dissent became not only more legitimate but also a serious constraint on conducting "political" or offensive war.

The book is based on extensive historical documentation, including original archival and documentary material, as well as interviews.

Levy's book is an ambitious, innovative study, a contribution to the attempt to "revise" the traditional, mainstream historical and social studies of Israel. Levy explores the interrelationships between social structure and foreign policy in Israel. His main argument is that Israel's security policy was devised not only as a direct and inevitable response to Arab states' hostility but also largely in accord with the social interests of a rising middle class composed of Western Israelis (mostly Ashkenazis). "Due to the protracted state of war, this class achieved its dominant status over other groups: Oriental Jews . . . and the minority of Palestinian citizens" (p. 23). This is, indeed, a very provocative argument that requires not only a solid conceptual framework but also clear-cut evidence.

After reviewing the flaws of international relations and statist theories in explaining states' interests and behavior, Levy presents a complicated conceptual framework that is

composed of three domains of action involving relations between the state and domestic groups. Interests and state behavior may be explained by a combination of international relations elements and state features. State policies that are motivated by realist and neorealist considerations may produce outcomes such as war or peace. These outcomes of external activities, however, affect domestic actors and shape their attitudes toward issues of war and peace; they also affect—through these actors' willingness to legitimize and be mobilized—the implementation of state policies. Whereas external developments affect, via the state, the interests of domestic groups, internal agents reciprocally affect the realities of international systems via the state. Whereas a state may use external action to reconstruct interests internally, it also may mold its international environment by using its domestic factors.

This attempt to combine different international relations theories such as realism, neorealism, and constructivism with statist theories is indeed innovative, but Levy himself admits some problems: "By integrating several schools of thought, I hope to gain in scope more than I might lose in accuracy" (p. 17). It seems that the main problem, however, is not only how to measure the role of each different theory in this "theory integration" but also the method chosen for that purpose. The analytical test for arguments, as Levy maintains, is more interpretive than empirical. Attempts to corroborate or falsify certain theoretical propositions about Israel's behavior in the Arab-Israeli conflict are based mainly on interpretation of different external and domestic developments rather than on empirical analysis of data.

Instead of case studies, Levy considers four different periods: the bellicose policy that drove internal state expansion and vice versa (1951–56); the Six Day War, which expanded the war-prone circle; the watershed years (1968–81); and from escalation to deescalation (1982–96). The rationale of this division is not sufficiently clear. Levy is right in maintaining that realist arguments may explain only part of Israel's behavior, including its shift toward peace. Political and social domestic interests also affected Israel's behavior in the Arab-Israeli conflict. He fails to demonstrate, however, how Israel's security interests were largely determined by the social interests of Ashkenazis, how the protracted state of war gave this group a dominant status over other groups and especially over oriental Jews, and how this interrelationship determined Israel's route from war to peace.

In sum, these two books offer new ideas about the need to examine both international and domestic politics in order to understand better the sources of foreign and domestic policies. The interrelationship between the two domains is crucial for understanding the foreign and domestic policies of Israel.

The Politics of Humanitarian Aid Operations. Edited by Eric A. Belgrad and Nitza Nachmias. Westport, CT: Praeger, 1997. 220p. $59.95.

The Ethics and Politics of Humanitarian Intervention. By Stanley Hoffman, with Robert C. Johansen, James P. Sterba, and Raimo Vayrynen, contributors. Notre Dame, IN: University of Notre Dame Press, 1996. 116p. $25.00 cloth, $14.95 paper.

Timothy D. Sisk, *United States Institute of Peace*

The perplexing moral and practical issues raised by international intervention in deadly civil conflicts—or "complex humanitarian emergencies"—are described in these two multiauthor volumes. Despite inherent ethical dilemmas and operational constraints, the contributors generally agree that multilateral interventions should be mounted to prevent and alleviate human suffering. Neither the legal norm of state sovereignty nor the practical political challenges ought to inhibit the international community from using force to provide relief to the needy in times of civil war. Intervention is justified with both utilitarian self-interest and moral imperative arguments by various authors.

The contributors to *The Politics of Humanitarian Aid Operations* argue that efforts to mitigate suffering inevitably have political motives and consequences. Neutrality and impartiality are compromised when third parties intervene to distribute new resources in domestic conflicts. The book contains theoretical chapters on issues and concepts in international humanitarian aid, case studies of the United Nations Relief Works Agency, Rwanda and Burundi, Cambodia, the former Yugoslavia, and three concluding chapters on the prospects for future humanitarian interventions.

General Sir Michael Rose, the former commander of UN forces in Bosnia, writes a foreword that implicitly places a good deal of the blame of the UN peacekeeping failure in Bosnia on the United States: "Those countries which cried loudest over Bosnia were, in fact, the most reluctant to offer the UN actual support on the ground" (pp. x–xi). This practitioner perspective resonates in the introductory chapter by Belgrad, which explains the humanitarian operations in the twentieth century (e.g., the Marshall Plan) as a consequence of political objectives. Belgrad, in a utilitarian defense of humanitarian intervention, suggests that the international community is increasingly informed by "the understanding that human misery cannot be quarantined; that it cannot be kept at arm's length forever; that if it is not opposed there, sooner or later it will descend upon us here" (p. 10).

A chapter by Jim Whitman offers reflections on the linkages between military risk and political commitment to explain the limits of assertive multilateralism, while David Forsythe convincingly describes a nascent but growing international norm that justifies the receipt of humanitarian aid as a universal human right. Alan James, in disappointingly inaccessible prose, argues that humanitarian operations and peacekeeping for conflict management should be kept as conceptually and operationally distinct as possible.

The case study chapters describe specific aid operations. Nachmias provides a stinging critique of the UN Relief Works Agency for Palestinians, arguing that the organization should fold. Although the excesses and malfeasance of UNRWA are well known, Nachmias unfairly sets up a paper tiger by describing the 50-year-old organization as a unitary rational actor that continually seeks only its own self-preservation. Two brief essays on Rwanda and Burundi (mostly Rwanda) are written by practitioners and presented as a single case study. The first describes the efforts of Catholic Relief Services to provide aid to these deeply troubled states; the second describes Israel's unilateral operation to establish a field hospital in eastern former Zaire for Rwandan refugees. The former essay offers an interesting personal insight into the ethical challenges facing front-line nongovernmental organizations, but the latter essay is mostly a self-congratulatory account of an Israeli good deed that offers few broader insights. Janet Heiniger provides an excellent overview of the complex Cambodian intervention, and James Rudolph offers a postmortem on why the UN failed in the former Yugoslavia. Rudolph's sweeping conclusions on the inherent limitations of UN peacekeeping, however, generalize from a single (arguably anomalous) case.

The highlights of *The Politics of Humanitarian Aid Operations* are the concluding chapters. Gil Loescher provides an

informed analysis of the UN High Commission for Refugees; Thomas Weiss describes the trends toward and away from assertive humanitarian intervention during the 1990s; and Leon Gordenker integrates the essays in a concluding chapter that emphasizes the importance of coordinating the humanitarian relief and political policies of donor states and international organizations. The book includes a short but useful bibliography.

Overall, there is no unifying framework chapter; the essays do not relate at all to one another; and no rationale is provided for the selection of case studies. The volume would have been stronger if the authors had related to a common analytical framework and set of assumptions. With the absence of thematic integration, and its steep price, *The Politics of Humanitarian Aid Operations* is not a good candidate for course adoption, given the many recent offerings on multilateral intervention that cover similar terrain.

The Ethics and Politics of Humanitarian Intervention, in contrast, may make an excellent companion reader for courses on war, international relations, or ethics. The readable book offers insights into the principal issues confronting the international community in the post–Cold War era. The core of the volume is two lectures by the eminent international relations theorist Stanley A. Hoffman, delivered at the Joan B. Kroc Institute of International Peace Studies at the University of Notre Dame in 1995. The first is a general rumination on the sovereignty and the ethics of intervention, and the second offers reflections on intervention in the former Yugoslavia. The two lectures are bounded by an introductory chapter by Kroc director Raimo Vayrynen and critiques by Notre Dame scholars Robert Johansen and James Sterba, with a brief concluding response by Hoffman to Johansen's and Sterba's comments.

Hoffman deftly argues that "the contemporary challenge to sovereignty is much more profound" because "the distinction between domestic and international politics is crumbling" (p. 13). Given the erosion of the norm, and the weakening or lack of resolve by states to protect their citizens, practical ethics suggests a jus ad interventionem that "allows for collective intervention when a state's condition or behavior results in grave threats to other states' and peoples' peace and security, and in grave and massive violations of human rights" (p. 23). This doctrine means stronger military coercion and more assertive diplomacy should have been used earlier to prevent or end the human rights tragedies in the former Yugoslavia. A moral sense of duty should have precipitated earlier and more forceful action to end the bloodletting.

Johansen counters Hoffman's appeal for greater use of force with a more restrictive and precise justification for humanitarian intervention. He suggests a graduated set of noncoercive responses—preventive deployments, police monitoring, international tribunals, and so on—that seek to minimize the need for tough military measures. Sterba puts Hoffman's arguments in more abstract philosophical form, suggesting that Hoffman offers an essentially Kantian justification for intervention. Hoffman accepts some criticisms of his approach but repeats his rationale for aggressive military responses.

From the moral perspective, Hoffman fails to address adequately the ethical dilemmas faced by decision makers in intervening states. How certain is it that greater force can lastingly resolve a civil war, and what of the cost in military casualties? The Somalia experience suggests that overwhelming force may save lives but not end wars, and Sterba makes a good argument that it is wholly unclear whether earlier use of force would have brought enduring Serb acquiescence to an independent Bosnia. Hoffman also raises the practical obstacles to using overwhelming force, but he does not offer much on how to overcome them. On the former Yugoslavia, Hoffman engages in the equivalent of intellectual backseat driving that oversimplifies the intricacies of decision making, for example, the difficult tension between prosecution of war criminals and clinching a negotiated settlement in Dayton. Hoffman belittles Western policymakers, in a somewhat cynical tone, without sufficiently acknowledging the countervailing pressures that worked against an earlier military response. Political will was weak on Bosnia, but the countervailing pressures (not least of which was congressional opposition to U.S. deployments) were very intense.

These volumes contribute to a burgeoning scholarly literature on multilateral international intervention, and *The Ethics and Politics of Humanitarian Intervention* relates especially well to international relations theory. Yet, the volumes suffer from three common shortcomings. First, the analysis would have been much stronger had insights from coalition theory been used to explain the conditions under which a collective international response can be mounted and sustained. The practical problem of building and sustaining an international coalition for intervention is largely overlooked. What of garnering Russian and Chinese concurrence for Chapter VII mandates in the Security Council, not to mention the support of member states from the developing world?

Second, with the exception of Weiss's essay in *The Politics of Humanitarian Aid Operations*, the contributors fail to discuss the most important U.S. foreign policy guideline on multilateral peace operations—Presidential Decision Directive 25, signed in early 1994. PDD-25 describes highly restrictive criteria for U.S. participation and support for intervention. The criteria are important not only for understanding U.S. policy but also because strict application of the guidelines stymied Security Council action in responding to the horrendous April 1994 genocide in Rwanda. The choice not to intervene in Rwanda—justified mostly in PDD-25's neorealist terms—was a bellwether decision that Madeliene Albright, who was then U.S. Permanent Representative to the United Nations, subsequently admits was mistaken. With such a glaring omission of the most important U.S. foreign policy criteria on humanitarian intervention, those seeking direct relevance to current policy debates will find the lofty analysis in these edited volumes poorly informed.

The third common shortcoming is that the authors often fail to take into sufficient account the political calculus of the parties. If humanitarian intervention is akin to medical treatment for the critical illness of a state, then these volumes describe the physicians' ethical challenges and practical limitations quite well. But they do not adequately address whether and how the patient's fundamental maladies can be diagnosed and healed. Insights from comparative politics can help. Recent work on civil war termination (e.g., Roy Licklider, *Stopping the Killing*, 1993) and domestic-international interactions (e.g., Michael E. Brown, ed., *The International Dimensions of Internal Conflict*, 1996) provide these all-important, inside-out perspectives.

The New Realism: Perspectives on Multilateralism and World Order. Edited by Robert W. Cox. New York: St. Martin's/United Nations University Press, 1997. 275p. $55.00.

David Skidmore, *Drake University*

This book is one in a series of publications produced under the auspices of a wide-ranging program on the future of multilateralism sponsored by the United Nations University

from 1991 to 1995. According to program coordinator and volume editor Robert Cox, the project's principal aim was to explore the prospects for creating "a new multilateralism built from the bottom up on the foundations of a broadly participative global society" (p. vii).

Cox contrasts the "structural-critical" approach adopted by contributors to the present volume with the "problem-solving" theories that dominate mainstream studies of multilateralism. Scholars working in the regime theory tradition implicitly adopt a normative preference for the status quo while seeking to resolve the short-term coordination problems faced by national authorities in the management of multilateral cooperation. Cox and his fellow contributors to *The New Realism* instead prefer a more critical stance toward existing multilateral institutions and focus on the conditions that might allow for long-term structural change in the direction of a more just and democratic world order.

The book is organized into three parts. Part I, "Realities," explores the underlying structural trends that are driving the evolution of multilateralism in the present world order. Three of the four essays in this section complement one another nicely. Collectively, these contributions examine the declining competence of the modern state as the central manager of social, political, and economic life. As territoriality becomes an increasingly tenuous foundation for political authority, states face growing challenges from an increasingly diverse array of social forces.

Susan Strange notes that economic globalization has enhanced the bargaining power of internationally mobile capital vis-à-vis state authorities while also complicating the task of managing increasingly integrated national economies. These realities have compelled states to cede growing authority to nonstate actors, including transnational business firms, intergovernmental organizations, and professional associations.

This reshuffling of authority relations takes place, however, within what Cox later refers to as a "dominant transnational class" (p. 247). The diffusion of power away from traditional states in favor of actors and institutions that can better perform certain system-maintaining functions poses no fundamental threat to the hierarchies of privilege that characterize the existing international order. Multilateral institutions may gain autonomy at the expense of states, but the international system remains firmly controlled by elite interests.

Yet, as Rodolfo Stavenhagen points out, globalization also gives rise to an array of antisystemic forces that could eventually provide the basis for a more genuinely bottom-up vision of multilateralism. Stavenhagen posits the emergence of a nascent grassroots civil society at the global level. As capital has become more mobile, popular forces have likewise been compelled to seek alliances beyond existing national borders. Transnational social movements have begun to cohere around issues such as environmental decay, human rights, labor conditions, the status of indigenous peoples and alternative development. Although it would be wishful thinking, as Stavenhagen recognizes, to suppose that these popular movements yet have the power or organization to overturn the institutions of the present international order, their very existence suggests that top-down models of globalization will not go unchallenged.

Rounding out this group of essays, James Rosenau explores the dialectic between globalizing and localizing forces in the contemporary international system. The latter include ethnic, religious, familial, and other subnational groups who seek to obtain a greater degree of autonomy or control over their lives from outside forces. Rosenau speculates about whether both globalizing and localizing tendencies may derive from common microfoundations. More concretely, he argues that both sets of forces challenge the authority of the nation-state in favor of some sort of alternative "imagined community" (p. 63), whether subnational, supranational, or transnational in scope.

Together, these three articles highlight the fragility of the existing state system and suggest the possibility of a stronger and less state-centric multilateralism in the future. As some scholars have suggested, perhaps the world is moving toward a multilevel polity in which authority is parceled out more on the basis of functional specialization than territoriality. What these contributions underline, however, is that the character of any restructured multilateral order will be hotly contested among three sets of forces: top-down globalizers, bottom-up globalizers, and localizers.

Cutting across all three of these categories are gender cleavages. V. Spike Peterson's contribution to Part I provides a valuable overview of how gender discrimination remains built into the very fabric of political and economic life in modern societies. Yet, neither Peterson nor the other authors adequately explore how gender issues can be successfully integrated with the models of globalization and multilateralism discussed elsewhere in the book.

Part II, "Perspectives," examines how various non-Western cultural traditions approach the topic of multilateralism. Separate chapters discuss Islamic (Hassan Hanafi), Indian (Satish Chandra), and Chinese (Hongying Wang) perspectives. All three offer rich accounts of how the intellectual traditions of each civilization have regarded cooperation across political and cultural boundaries. In refreshing contrast with some recent works, these authors resist the temptation to treat civilization clusters as homogeneous and unchanging wholes. Instead, these essays emphasize the intellectual diversity within each culture and the important role that interactions among distinct civilizations have played in the evolution of each.

In comparing civilizations, two important common patterns stand out. In each civilization, including the West, centralizing and monist tendencies compete with decentralizing and pluralistic tendencies. Centralizing forces often gain the upper hand during periods when members of a civilization face severe competition or threat posed by outside forces. A second cleavage common to each of these civilizations pits realists, who tend to adopt a pragmatic approach to dealing with outsiders, against idealists, who typically defend visions of cultural purity against perceived threats of contamination.

Given these variations within each cultural complex, it is clear that attitudes toward multilateral cooperation are nowhere constant or monolithic. Each civilization has passed through periods of greater openness and others of greater closure. Rather than blanket generalizations, it is necessary to undertake detailed study of the conditions under which different cultures either embrace or resist multilateralism.

Part II also contains perhaps the most original essay in the volume. Kinhade Mushakoji explores the relationship between multiculturalism and multilateralism. The global hegemony of the Western world over the past century and more has shaped the boundaries of légitimate political discourse worldwide. The institutional forms of the modern European state have been universally adopted or imposed throughout the postcolonial world. The language of modern political and economic life revolves around rational-formal concepts such as sovereignty, the market, development, progress, nationalism, and human rights. The design, procedures, and language of existing multilateral organizations reflect these same char-

acteristics. Western discourse is treated as universal and timeless.

Mushakoji argues that the triumph of rational-formalism, itself a by-product of the Western-sponsored modernization process, has "occulted" or hidden the survival of traditional ways of thinking and acting in much of the non-Western world. At the overt level, traditional thought and language are disparaged and treated as illegitimate. Yet, beneath the veneer of modernization, many people continue to find that traditional discourses make better sense of the substantive realities that shape their lives. The result is a yawning gap between the formal principles of political life and the tradition-guided practices of ostensibly modern institutions.

This gap between universalistic forms and culturally pluralistic realities hampers the effectiveness of existing multilateral institutions. A consensual facade glosses over substantive differences and promotes miscommunication. Peacekeeping operations or development projects that neglect "occulted" realities are doomed to failure. Mushakoji calls for a multilateralism that acknowledges not only a "pluralism of interests" but also a "multicultural pluralism of values" (p. 246). This would require that previously marginalized discourses be accorded legitimacy and a voice.

Part III, "Dynamics," is mostly disappointing. Chapters on the U.S. relationship to the United Nations (Harold Jacobson), multilateralism and the former Soviet bloc (Mihály Simai), and Africa (Fantu Cheru) are unremarkable. Bjorn Hettne's chapter on regional responses to globalization does, however, merit comment. Hettne discusses the various motivations and forms that comprise recent movements toward regional integration and cooperation. He contends that some forms of regionalism offer a reasonable middle ground between the limitations of the nation-state as an economic unit and the homogenizing and undemocratic characteristics of an unfettered global market economy. His preferred brand of regionalism would center around a "benign" mercantilism designed to preserve a degree of diversity and autonomy among groups of like-minded nations.

Robert Cox's conclusion focuses on the social polarizing consequences of globalization. A new multilateralism is needed that can steer this process toward more just and democratic outcomes. Cox is pessimistic about efforts to restructure existing multilateral institutions through piecemeal reform. Instead, he argues that a reconfigured multilateral order will follow the ripening of popular mobilization and the creation of a genuinely grassroots civil society at the global level.

It is perhaps this insistence on radical restructuring rather than incremental reform that explains the volume's strange neglect of existing multilateral institutions and the struggles that surround them. Popular movements have shown a surprising ability to shame or pressure governments, international organizations, and large corporations into embracing progressive multilateral initiatives in recent years. The global treaty banning landmines, the World Bank's shift toward "greener" lending policies, the spread of microbanks that provide credit to the poor, and the public pledges by several high-profile corporations to renounce reliance upon child labor and Third World sweatshops are all products of an increasingly effective grassroots style of transnational politics. Pitched at a higher level of abstraction, much of the volume (excepting Stavenhagen's brief chapter) fails to analyze the lessons and implications of these hopeful instances of bottom-up transformation.

The chief value of this volume is that it reflects a welcome emphasis on social forces, rather than the distribution of state power or institutional structure, as the principal determinants of future multilateralism. Mainstream inquiries typically seek to explain variation in the *degree* of multilateral cooperation or the *efficiency* of international institutions. Cox and his colleagues instead focus on the *content* of multilateralism, or, in other words, the particular purposes, interests and values served by multilateral institutions and practices. While problem-solving theories emphasize *quantitative* measures of cooperation, a structural-critical approach steers attention toward the *qualitative* features of a given international order.

Still, *The New Realism* suffers from a number of unresolved tensions. While some authors, such as Cox, stress materialist factors, others, such as Mushakoji, accent ideational factors, such as cultural values and beliefs. It is left unclear how these fundamentally differing perspectives can be stitched together. A number of contributors extol the need for a more multicultural form of multilateralism. Yet, this raises the question of whether the enshrinement of cultural diversity means abandoning the search for common values and universal rights across civilizations.

The book's most puzzling feature is its title. Few of the contributors could rightfully be termed realists, old or new. Cox himself is more an intellectual descendent of Gramsci than of Morgenthau. If labels matter, then *The New Realism* is an inexplicably misleading choice.

These complaints aside, the book as a whole makes a welcome contribution to our understanding of the long-term forces shaping the future of multilateralism. *The New Realism* raises questions too often ignored in mainstream research and several of its contributors offer fresh insights that deserve further thought and research.

Shadows in the Forest: Japan and the Politics of Timber in Southeast Asia. By Peter Dauvergne. Cambridge, MA: MIT Press, 1997. 308p. $45.00 cloth, $22.00 paper.

David M. Potter, *Northern Kentucky University*

Peter Dauvergne examines the problem of unsustainable environmental policies in Southeast Asia by exploring the link between domestic and economic forces in developing states and the international demand for tropical timber that drives environmentally unsound forestry practices. Specifically, he examines Japanese consumption and trading practices and how they affect timber policies in Indonesia, Malaysia, and the Philippines. Dauvergne develops the twin frameworks of shadow ecology and patron-client networks to explore that link. By shadow ecology, a term coined by Jim MacNeill, Peter Winsemius, and Taiyo Yakushiji (*Beyond Interdependence: the Meeting of the World Economy and the Earth's Ecology*, 1991), Dauvergne means "the aggregate environmental impact on resources outside [a country's] territory of government practices, especially official development assistance (ODA); corporate conduct, investment and technology transfers; and trade, including consumption, export and consumer prices, and import tariffs" (pp. 2–3). He focuses on timber extraction in this case. Dauvergne contends that Japan's shadow ecology of timber in Southeast Asia has had indirect effects on unsustainable forestry by providing the demand that drives local patronage networks to exploit forestry resources. He places blame in particular on Japanese sogo shosha (general trading companies), which depend on large volumes of inexpensive log imports to stay in business. Sogo shosha were instrumental in the overlogging of the Philippines, legal and illegal, in the 1950s and 1960s. When cheap timber stocks ran out, they moved on to Indonesia in the 1970s, then to Malaysia in the 1980s. Japan's foreign aid fostered timber extraction efforts through the

1980s. In the 1990s that aid has largely been given the task of inadequate support for reforestation of degraded sites.

Dauvergne makes it clear that this would not have happened without favorable policies in the host countries. He suggests two refinements to Joel Migdal's idea (*Strong Societies and Weak States*, 1988) by arguing that states may be strong in some policy areas (limiting foreign control over natural resources) but weak in others (management of environmental resources). Timber management suffers in both cases: Strong state policies reinforce unsustainable extraction, while weak enforcement of environmental protection policies hinders timber management. The key to understanding this divergence in state capabilities lies in the structure of patron-client networks, ubiquitous in the Southeast Asian countries under study and especially harmful to sustainable timber management. Powerful patrons or their clients reap substantial rents (in all three cases the government lays claim to forest areas) by destructive timber extraction. Timber management bureaucrats at middle and low levels cannot control the devastation. In many cases underpaid officials are bribed to look the other way; in a few instances they are clients of the patronage networks that depend on resource extraction to maintain themselves.

This work undertakes the laudable task of linking developed countries' consumption practices with production in developing nations. By connecting Japan with its timber-producing neighbors, Dauvergne puts environmental resource management into a broader context. Yet, this same focus is the major problem with the book. The links between the two are never effectively made. The subtitle is something of a misnomer, since the work is mainly about Southeast Asian patronage networks and their effects on forestry practices. The opening chapter on Japanese official and corporate environmental policies is much less nuanced than the later chapters on Indonesia, Malaysia, and the Philippines. Japanese policy and practice in the latter chapters are tacked on to the preceding discussions of clientelism in each country, with often superficial discussion of the links between the two. Remedying this deficiency would have made the work longer but more significant.

The book raises two issues, one methodological and the other theoretical, which the author does not confront. The first has to do with measurement. At a number of points, Dauvergne cites nongovernmental organization (NGO) estimates of volume and rate of timber extraction in Indonesia, Malaysia, and the Philippines, typically as counters to official or corporate estimates. Here, it seems to me, we have a problem of reliability in measurement. While the author clearly finds NGO estimates more reliable than official estimates, especially in Indonesia, how do we know they are accurate? NGOs tend to lack resources that states have to carry out statistical studies, and even official estimates in developing countries are notoriously unreliable. Dauvergne tends to assume that NGOs have less reason than governments or cronies to cook their figures, but this may not be the case. The author points out in chapter 5 (pp. 156, 163) that a number of Philippine "NGOs" which have availed themselves of government reforestation funds have been front organizations for rent-seeking timber operators. In the end, we are left with two sets of data, neither of which may be accurate. This is not simply a problem of specific cases; Dauvergne asserts that an essential element of Japan's ecological shadow in Southeast Asia is the consistent undervaluation of the environmental effects of deforestation. With unreliable statistics, there is no way to make a convincing argument in that direction.

The second issue involves the effects of patron-client networks on logging incentives. The middle chapters of the book treat patron-client relationships in each Southeast Asian case in great detail. Dauvergne implies, but does not clearly state, a theoretically interesting link between the independent variable of structure of patronage network and the dependent variable of timber extraction. The author describes two kinds of patronage networks: volatile, unstable networks centered on electoral systems and fluid parties (Malaysia, the Philippines before and after martial law) and relatively stable centralized networks in authoritarian political systems (Indonesia under Suharto, the Philippines under Marcos). The author asserts that in the first kind of patron-client system timber concessionaires have incentives to extract resources as fast as possible regardless of environmental damage, before their patrons are voted out of office and they lose their license. He makes essentially the same argument, however, about the centralized networks created under Suharto and Marcos. If there is no difference, then why all the detail about network structure? If there is a difference, the author has missed the opportunity for a key insight into politics and policy in clientelist states in general.

Its weaknesses aside, *Shadows in the Forest* contributes much to our understanding of an increasingly contentious and problematic issue both in domestic economic development and international politics. Scholars of environmental policy, Japanese and Asian political economy, and comparative politics of developing societies will find much useful material.

Bridging the Mexican Border: Transforming Mexico–U.S. Relations. Edited by Rodolfo O. de la Garza and Jesus Velasco. New York: Rowman & Littlefield, 1997. 208p. $65.00 cloth, $24.95 paper.

Robert A. Pastor, *Emory University*

From the second to the last decade of the 20th century—from the Mexican Revolution to NAFTA—the relationship between Mexico and its northern neighbor was distant and often difficult. Fearful that closer relations would be suffocating, the Mexican government chose to defend its sovereignty by insisting on "limits to friendship." Some Mexicans, like Jorge G. Castañeda, urged a more activist approach in order to influence U.S. policy, but such recommendations were ignored. Until President Salinas and NAFTA.

In spring 1990, Carlos Salinas proposed a free-trade agreement and reversed traditional Mexican policy toward the United States. He judged that Mexico's future depended on guaranteed access to the U.S. market and that an assertive policy by Mexico was needed to advance its interests. The volume edited by Rodolfo de la Garza, professor of government at the University of Texas at Austin, and Jesus Velasco, professor at the Center for Economics Research and Training (CIDE) in Mexico City, explains the new Mexican foreign policy and its implications for the relationship.

One of the great scholars of the Mexican Revolution, Alan Knight, introduces the subject by describing how the event gave Mexico a sense of cultural and national confidence that is rare among developing nations. The United States also learned something valuable as a result of the revolution: Military intervention against Mexico is costly and ineffective. Since then, the two countries have been careful to avoid military confrontation and, indeed, have established an interesting "congruence" in their political systems. Lazaro Cardenas and Franklin Roosevelt both believed in the need for state intervention to cope with the economic crisis; their successors in the 1950s were staunchly anti-Communist and probusiness; Lopez Mateos and John Kennedy were moder-

ately liberal; and, of course, George Bush, Carlos Salinas, Bill Clinton, and Ernesto Zedillo are all Ivy League graduates.

Knight is too wise to stretch the point too tightly, but his thesis is both novel and convincing. The congruence is not due to mimicry but to similar challenges that the two countries have faced at roughly the same time. Each can afford to be somewhat similar because their differences are both more transparent and profound, and each has developed a grudging respect for those differences and an appreciation of the advantages of integration.

In the post–Cold War world, Mexico has employed a new set of foreign policy tools to address the new challenge of integration. These range from lobbying in Washington to inviting "international visitors" to observe the Mexican elections in 1994. The use of the new tools, Jorge Chabat writes, "marked the beginning of the end of the discourse of 'sovereign democracy'" (p. 43).

In February 1990, President Salinas decided to strengthen the 40 consulates in the United States and establish the Program for Mexican Communities Living in Foreign Countries (PMCLFC) to enhance the Mexican government's capacity to influence the United States. The initiatives did help Mexico learn more about the Mexican-American communities in the United States, but as Rodolfo de la Garza demonstrates in his astute analysis of the PMCLFC, the more compelling lesson was that the interests of Mexico and of Mexican Americans did not coincide on a great many issues. On three out of the four most important immigration issues, the Mexican-American community defined its interests differently from that of Mexico. Carlos Gonzalez Gutierrez confirms that "had it not been for the discriminatory fervor of the initiative's sympathizers, Mexican American voters would have approved Proposition 187 [in California to prevent services for undocumented workers] by an ample margin" (p. 60).

In a well-researched essay, Todd Eisenstadt surveys the spectacular growth of Mexican government expenditures for lobbying in Washington—from less than $70,000 in 1985 to $16 million in 1993 (p. 94). Jesus Velasco shows how Mexico also tried to buy influence through its support for key "think tanks" in Washington. Still, both authors believe that congressional votes on NAFTA were influenced more by constituent pressures and the president than by Mexican lobbying, and Eisenstadt documents 19 separate deals that the Clinton administration made with 51 members of Congress to secure their votes.

In a concluding essay aimed at trying to weave the various contributions together, Jorge Dominguez uses three models to explain different levels of the relationship. A state-based model explains the coexistence between the two governments. The most profound changes in the relationship, however, are due to accelerating social integration by immigration and education, and he applies a society-based model for that. Finally, new institutions, like NAFTA, and functional government agencies, like the trade ministries and drug agencies, have assumed greater importance than even the foreign ministries. Dominguez makes a persuasive case that the contemporary relationship can only be fully understood by reference to the three models.

What had been a silent, barely noticeable integration between Mexico and the United States a few decades ago has become loud and momentous. The two countries together with Canada have embarked on an uncharacteristically intimate relationship that involves new costs and vulnerabilities. De la Garza and Velasco have done a great service in assembling a group of such able scholars to explore one new dimension of that relationship—Mexico's increasing efforts to influence the United States.

Some in the United States may be uneasy about the new Mexican activism; that, of course, was Mexico's historical reaction to U.S. activism. But the more both countries understand that the door between them can swing both ways, the better they will be able to benefit from integration and cope with its costs. *Bridging the Border* is a well-written and valuable contribution to understanding the changing phenomenon of U.S.–Mexican relations.

Power Steering: Global Automakers and the Transformation of Rural Communities. By Michele M. Hoyman. Lawrence: University Press of Kansas, 1997. 262p. $35.00 cloth, $17.75 paper.

Capital beyond Borders: States and Firms in the Auto Industry, 1960–1994. By Kenneth P. Thomas. New York: St. Martin's, 1997. 191p. $55.00.

Richard Florida, *Carnegie Mellon University*

Multinational firms are increasingly powerful actors in the world economy, affecting state strategy and economic policy in numerous ways. Their ability to move capital and factories across borders has placed enormous pressure on governments—at all levels—to create ever more attractive "business climates," to provide ever more generous incentive packages, and to engage in costly bidding wars to lure multinational factories inside their borders. Nowhere is this more apparent than the automotive industry, as governments around the world vie to attract new assembly factories in the hope of creating jobs and spurring economic development. Two new books by political scientists tackle important dimensions of this subject.

Capital beyond Borders, by Kenneth P. Thomas of the University of Missouri in St. Louis, examines the effects of increased capital mobility in the automotive industry on nation-state behavior over the post–World War II period. Building on themes originally outlined by Charles Kindleberger, Raymond Vernon, and Robert Gilpin, among others, Thomas makes two related arguments: Capital mobility in the automotive industry has increased substantially over time, and this, in turn, has substantially altered the balance of power between multinational firms and nation-states in favor of the former. Thomas contends that the ability to relocate plants around the globe has increased the bargaining power of multinational enterprises vis-à-vis states along several key dimensions: by substantially expanding the locational options available to firms, by enabling them to consider a wider range of possible investment locations (thus eroding the bargaining power of states), and by introducing powerful bidding dynamics into state strategy. Thomas focuses, specifically, on the use of investment incentives by governments and escalating bidding wars among states as evidence that the balance of power in international political-economic affairs has shifted away from states and toward multinational firms.

In developing his argument, Thomas provides a comprehensive synthesis of the broad and diverse literature on this subject—for this alone the book is worthwhile. He then analyzes a variety of statistical evidence on the global activities of leading automotive firms (particularly the American Big Three). The statistical data suggest that at least two of the Big Three have substantially expanded their global production activities since World War II. At both Ford and GM, the percentage of foreign employees (i.e., from outside the United States) has virtually doubled, growing from 30.6% in 1951 to 63.7% in 1990 at Ford, and from 30.3% in 1970 to 52.8% in 1994 at GM. Chrysler, much less global in orienta-

tion, has actually experienced some reduction in the percentage of foreign workers in its work force since the late 1970s.

The heart of the book revolves around a series of case studies of factory location decisions in the United States, Canada, and the United Kingdom, including the Diamond-Star joint venture between Chrysler and Mitsubishi in Illinois and various Big Three plants in Mexico and Canada in the context of NAFTA. These case studies probe the dynamics of corporate location, focusing on how increased corporate locational flexibility places pressure on governments to offer ever larger incentive packages both to lure firms to new places and to retain them in existing locations. The findings suggest a trend toward higher incentive packages. Thomas further argues that capital mobility simultaneously erodes the power of labor unions and workers, resulting in slower wage increases (and in some case in actual wage reductions) and the establishment of more flexible work environments.

Power Steering, by Michelle Hoyman, also of the University of Missouri at St. Louis, explores a related dimension of the effects of capital mobility on governments and public policy. She focuses on the long-run influence of major corporate location decisions on the social, economic, fiscal, cultural, and political organization of communities. Hoyman examines four new automotive assembly investments in the United States: GM's Saturn plant in Spring Hill, Tennessee; Nissan in Smryna, Tennessee; Toyota in Georgetown, Kentucky; and Honda in Marysville, Ohio. These are among the newest, largest, and most advanced automotive assembly complexes in the United States and the world. And each is a greenfield site, located in a small rural community.

While Hoyman describes the incentive packages and bidding strategies that enabled each of these communities to land a major automotive assembly complex, she zeroes in on the effects of these factories on the surrounding community. She contends that instead of looking at plant location as a static, "one-shot" event, it can be more appropriately conceived as a broad and dynamic process of development, starting with the decision to establish a plant and moving through both short- and long-term economic development effects. To do so, she draws upon extensive primary source research, including detailed analyses of community fiscal issues, interviews with government and corporate officials, and analyses of various public opinion polls which gauge changing community sentiments.

Her findings suggest that the multinational production complexes create enormous pressures (as well as benefits) for communities. Considerable burdens are placed on local infrastructure, from roads to water treatment facilities. Revenues do not always rise in synch with soaring service and expenditure needs. World-class factories require world-class education and training infrastructure, creating more pressure for advanced school curricula and state-of-the-art vocational training programs. The neighboring communities and counties are frequently hardest hit, with escalating demands for services but little in the way of new revenue. Furthermore, Hoyman finds a disjuncture between the state government officials who actively organize these investment "deals" and their local government counterparts, who must cope with the costs. She also finds that the new complexes have a powerful effect on a community's politics and political structure, ushering in a leadership change toward progrowth coalitions and further economic development. Still, she concludes that, despite these costs, large multinational automotive assembly complexes tend to garner the support of both local leadership and the citizenry at large.

Both books are welcome and basically solid contributions to the growing debate over investment incentives and the role of multinational firms in conditioning state behavior. Both are very well argued, and I agree with the central arguments of each. Both, however, suffer from some similar flaws.

The first set of problems is mainly conceptual. Both books pose multinational firms as relatively unconstrained actors, operating with full information and able to move capital and factories around the globe virtually at will. But other research shows that multinational enterprises are constrained by various factors, including their own organizational history and path. For example, it is very hard—and probably impossible—for Volkswagen to abandon its major assembly complex in Germany, even though it can and does produce cars far more efficiently and with far more modern work systems at other locations. Location in the automotive industry is strongly conditioned by markets, as major car makers pursue "build-where-you-sell" strategies. Both books overlook the interesting collective action problems that face both enterprises and states in the era of globalization. Stiff competition and highly imperfect information mean that multinational automotive firms frequently adopt follower strategies in emerging markets, leading to well-documented overcapacity and shaping potentially devastating shakeouts for the not-too-distant future. Due to the focus on the investment strategies of major automotive assembly companies, both books miss the even more interesting globalization of the supply sector, as a group of large international automotive component producers (such as Bosch, TRW, Johnson Controls, and Denso) consolidate their hold on the market, drive out or acquire smaller domestic suppliers, and develop an integrated multinational supply infrastructure from which all major automotive assemblers draw.

Furthermore, both books overlook the potential countervailing strategies of states. The centrality of markets in attracting automotive investments affords substantial power to large states, such as China, to extract concessions from multinational firms in the form of joint ventures with and technology transfer to national enterprises. Simply put, the relationship between multinational enterprise and states is not unidimensional: Indeed, states differ substantially in the degree of leverage they possess in bargaining situations with multinationals. And, surprisingly, both books disregard the potential for states to engage in collective action strategies (as some are already doing) to limit the bargaining power of multinational enterprises, through agreements to confine the use of investment incentives.

The second set of problems involves methods and research design. Both books rely on limited and to some extent biased samples. While the authors establish the plausibility of their arguments (which is very useful and valuable in and of itself), they fail to produce the kinds of evidence required to prove those arguments and hypotheses in ways that are externally valid. *Capital beyond Borders* relies on a very limited sample of case studies drawn from Big Three plants located in just three advanced nations. To really nail down its case, this book would require a broader sample of firms and locations, including Japanese and European automotive companies and locations in continental Europe, Asia, and South America. *Power Steering* depends on a limited sample of four large assembly complexes located in four small, rural communities. A broader sample of complexes of various sizes located in large and small urban and rural communities would be required to substantiate many of its claims.

Still, these are minor quibbles with books that are useful and valuable additions to the growing literature on the effects of globalization on politics, political organization, and public policy in the automotive industry.

When all is said and done, there remains little doubt that

the outward investment activities of multinational enterprises have spurred widespread economic growth and development both in the emerging economies and in formerly "written-off" industrial regions of the United Kingdom and the United States as well. Thus, it should come as little surprise that governments have unflinchingly embraced corporate investment as a core strategy for economic growth and development. For the time being, multinational firms are clearly in the driver's seat, but a backlash may well be waiting in the wings. In any event, it will take time for states to develop the right mix of strategies to cope with increased multinational investment in this newest era of globalization.

A Paradigm for the New World Order: A Schools of Thought Analysis of American Foreign Policy in the Post–Cold-War Era. John C. Hulsman. New York: St. Martin's, 1997. 212p. $65.00.

Mary Durfee, *Michigan Technological University*

John C. Hulsman has written about a very difficult subject—how ideas about the international system help decision makers make sense of their policy choices and how collections of these ideas may play out in bureaucratic politics. In an effort to make these complex, nested links he combines ideas, people, and bureaucracies together into three "schools of thought," each of which begins with first principles as to what helps produce international peace. From the principles, he deduces the policies American decision makers would advance for different issues and regions. He then uses the schools to explain the course of Clinton's policy toward Bosnia through the Dayton Peace Accords.

Unfortunately, the links do not hold, or at least they are unconvincing in the face of the evidence. First, the schools-of-thought concept itself proves definitionally slippery. It is smaller than an ideology because it does not transform individual belief systems into collective belief systems (p. 10). It is somewhat larger than Ole Holsti's notion of a belief system, because the priority setting of an individual belief system goes on at "a higher analytical plane" (p. 10). Second, the underlying principles of the schools seem to need clear domestic "first principles" to match the international ones. Third, while Hulsman says that only elites occupying bureaucratic roles (he includes the Senate here) share and contest over schools of thought, public opinion can nevertheless influence those elites. Last, the schools are so flexible that leaders and policies can readily change in the face of domestic or international events, and thus one wonders what stability they provide.

The three schools of thought Hulsman identifies are democratists, neorealists, and institutionalists. As first principles, Democratists dream of a peaceful world of democracies; neorealists imagine the world in zero-sum or relative gains terms, and institutionalists emphasize absolute gains from trade and interdependence. He summarizes all this as follows. "Shared interests is the catchphrase for institutionalists, just as shared values is a crucial notion for democratists, and national interests is an essential thought underpinning neo-realism" (p. 66). Even though Hulsman says it is crucial to understand first principles, his actual sorting of individuals and policies relies on orientations to European allies and to Russia (and China, though he provides no case evidence). Logically, this should make sense, providing one has the right first principles and can show that they, rather than the immediate logic of events, really do structure the regional policy preferences.

A convincing connection between first principles and regional policy stands, however, is not made. Hulsman places nine individuals into the three schools according to their views on regional issues. He writes of Secretary of Defense William Perry: "His neorealist givens lead to, paradoxically, institutionalist policy outputs" (p. 97). Perry thought Bosnia was not important enough for intervention (as did the neorealists), but he also opposed lifting the embargo, an institutionalist stand. Nor does use of a more abstract bridging concept, like multilateral or unilateral action, help clarify the schools. All three schools prefer multilateral action over unilateral, but they differ in degree. Perhaps if the case material had gone beyond the Bosnia problem—for example, trade issues—it would have been easier to imagine how the nuances in regionalism and multilateralism help explain policy outcomes.

In the end, the schools are so susceptible to changing events that pragmatism seems a more robust concept than schools. It would help the schools concept enormously if Hulsman thought more about pragmatism and compromise. For example, he notes that a change from "passive," "static," and "reactive" institutionalism (he uses these words interchangeably) to a "muscular" institutionalism (p. 169) was caused by three alarms: the massacres at "Srebrenica, European pressure to leave Bosnia and Congressional hostility to the arms embargo" (p. 169). The situation changed, and with it the "institutionalist" interest in sanctions and the U.N. as the way to solve the problem. These events opened opportunities to change policy, to reassert other policy preferences both within the foreign policy establishment and between the United States and the allies and Russia. The schools do not help explain this change, because they say nothing about the openness of the schools to compromise or new information.

Even more problematic is the importance Hulsman gives to domestic factors in shaping the schools; yet, his schools have no domestic logic to them, which causes endless problems in making the tie to bureaucracy and politics. For example, the reason the president can sometimes get his way is because the neorealists (like Dole) believe that the executive should be strong (p. 173). Yet, this key element in explaining outcomes is not discussed as an important part of the neorealist school. The problem of compromise appears again, this time with another branch of government. "With his institutionalist Bosnia policy so unpopular both with the public and the neo-realist Congress [after the 1994 elections], the President was forced to compromise, even during negotiations regarding the [Dayton] treaty, adopting some neorealist planks in the accord so that the package stood a chance of receiving congressional approval" (p. 179). By this logic, the president might have become a neorealist had the Republicans done even better in the 1994 election. As it was, the margins of electoral victory were too slim in Congress to override presidential vetoes, so this much change was not required of the administration. One needs only the U.S. Constitution to understand this domestic outcome.

Certainly, there are policy differences, and these ebb and flow in daily politics. But are they extensive enough to be called schools? How would we tell? For example, one might use a set of cases to capture more carefully the first principles of the purported schools. These cases would help readers decide whether the schools idea is more useful than simply saying "the president's policy." They would, presumably, increase the number of individuals who matter, thus giving a stronger sense of when personality as opposed to role matters. The cases could be used to tease out differences in orientation to regionalism, multilateralism, compromise, and pragmatism. The cases might also help identify the missing domestic concept the schools require. In the end, one comes

away from the book feeling that the logical framework misses important variables and bears only a tenuous relationship to the evidence.

Strategic Geography and the Changing Middle East. By Geoffrey Kemp and Robert E. Harkavy. Washington, DC: Brookings Institution Press, 1997. 493p. $52.95 cloth, $22.95 paper.

Aurel Braun, *University of Toronto*

We are living in a period of transition. Monumental changes have occurred in the international system with the collapse of communism and the demise of the Soviet Union, the second superpower. Instead of what many perceived as a bipolar world, we are in a transition, possibly, to multipolarity. The tense and risky confrontations of the Cold War have been transformed in some important respects into processes of cooperation and inclusion. A Russian general sits at NATO headquarters in Belgium as part of a new cooperative arrangement between the Atlantic alliance and Russia. Three former members of the Warsaw Pact have been invited to become full members of NATO. There is, therefore, a need for new thinking about the international system and about all aspects of regional conflict and cooperation.

If we are to reconceptualize, then we must focus not only on the new but also on that which is both probative and relevant. George Bush's rather inarticulate musings about a new world order hardly moved our understanding much farther. More sophisticated analyses have been provided by scholars such as Samuel Huntington ("The Clash of Civilizations," *Foreign Affairs*, Summer 1993, pp. 22–49), who has argued that the great divisions among humankind and the dominating source of conflict will be cultural. That is, a clash of civilizations will dominate global politics. Others, such as Francis Fukuyama ("The End of History?" *National Interest*, Summer 1989, pp. 3–18), have written, perhaps partly tongue in cheek, about "the end of history." But there is no definitive answer about the future. There is not even a consensus about current developments. Still, we continue to strive to understand the motor forces for change and to identify more clearly the warning signals of international danger.

In no region of the world has conflict been more endemic or interstate problems more intractable than in the Middle East. The end of the Cold War could present new opportunities to reshape that region, and yet, just as in the rest of the world, there are also enormous risks. To understand these, there is an especially urgent need for conceptual clarity and appreciation of the linkages among political, economic, and geographic factors, together with a thorough examination of the historical context. Geopolitical analysis, which emphasizes the relation of international political power to geographical setting, offers some promise, but it is usually such a broadly construed approach that, for the Middle East in particular, we likely need a more sharply defined focus. This is where Geoffrey Kemp and Robert Harkavy, two veteran analysts of the Middle East, are idealy suited to help. Both have written extensively and with distinction on the region and have worked as government advisors and policymakers. They can bridge the gap that so often exists between scholarship and policymaking. They have produced an intelligently organized and extensively researched work that makes a great contribution to our understanding not only of the Middle East but also of the workings of the international system itself. Two aspects of the book are especially noteworthy: the conceptualization and the geographic definition of the region.

Strategic geography, according to the authors, unlike geopolitics, has a more specific meaning in that "it is directed at the tactical elements of geography that contribute to grand strategy" (p. 8). Moreover, the former refers to the control of or access to special areas (land, water, and air) that have an effect, either positive or negative, on the security and economic prosperity of nations and embrace both human and physical geography. Finally, the several subcategories allow the authors great scope and opportunity. But this also makes for a remarkably ambitious undertaking that needs and seeks to explain the constant interplay of crucial factors, including physical geography, which changes slowly, and political geography (a subcategory of human geography), which can shift suddenly and dramatically.

Yet, this is an eminently sensible approach. In any particular period, but especially during one of fundamental transition, good analysis demands attention to a multiplicity of factors and the fluidity of change. Many would be daunted by the task, but Kemp and Harkavy plunge ahead. In examining the sources of conflict, they analyze the role not only of raw materials and energy but also of water. They assess not only the linkages between geography and conventional warfare but also the revolution in military affairs (RMA) that was so starkly demonstrated during the Gulf War. And they provide extremely valuable information on the status and potential use of weapons of mass destruction in the region.

In defining the region, the authors make their task even more complex. The area known as the Middle East (at least since World War II) or, according to the traditional U.S. State Department definition, the area that stretches from Morocco to Iran, encompasses a zone of almost unparalleled strife and controversy. Would the study of this area alone not be sufficiently challenging, especially given Kemp and Harkavy's complex conceptual approach? They opt instead for what they call the greater Middle East. This vast quadrilateral is bounded by the Sahara and the Mediterranean on the west; the Black Sea, Caucasus, Elburz Mountains, and Hindu Kush to the north; the plains of India to the east; and the Indian Ocean to the south. Thus, a huge part of the former Soviet Union and of India and Pakistan are encompassed. It is not hard to imagine that this would be enough to worry geographers and panic many political scientists.

Still, if one accepts the conceptual framework of the authors, there is considerable logic to this geographic expansion, despite the potential pitfalls. Religious strife, political transformation, historic enmities, the contest over resources, and the revolution in military technology are sufficiently intertwined here to allow for a reasonable geographic cohesion, at least for analytic purposes. But given the risks of lapsing into mere description and data compilation, the onus is on the authors to justify this geographic logic throughout the work. They succeed admirably. In the process they provide new and important insights into the Arab-Israeli peace process, the India-Pakistan dispute, the violence in the Caucasus, and the tribulations in the Persian Gulf.

Furthermore, Kemp and Harkavy not only cover a breathtaking range in their analysis of political, economic, and military trends but also provide both context and perspective. They suggest two disturbingly different regional scenarios: cooperation, modernization, and prosperity or instability, tension, and various conflicts. They also address in the process a variety of myths, including that of Western decline.

There may be some quibbles. Though the authors are not unsympathetic to Israel, they clearly favor the Labor Party view, and they give Mikhail Gorbachev too much credit for ending the Cold War. Overall, however, the book is admirably balanced, remarkably insightful, and clearly written. It is

a most welcome and important study of a region that is so crucial to international prosperity and security yet remains so unstable.

Interests, Institutions, and Information: Domestic Politics and International Relations. By Helen Milner. Princeton, NJ: Princeton University Press, 1997. 309p. $47.50 cloth, $18.95 paper.

Daniel Drezner, *University of Colorado at Boulder*

It is easy to tell an ancedote showing that domestic politics matters in international relations. It is much harder to explain in a systematic fashion how, why, and when domestic forces matter. International relations theorists have been squeamish about taking this step, in part because of the complexity of the task. What aspects of domestic politics are important? Interest groups? Institutions? Public opinion? Bureaucracy? It is not surprising that most scholars duck behind Kenneth Waltz's warning of reductionism and tread no farther.

In this ambitious book, Helen Milner enters the breach and constructs a fully developed model of the interaction between domestic and international politics to explain the pattern of international cooperation. What is truly extraordinary about Milner's approach is that it encompasses practically every facet of domestic politics without sacrificing parsimony or clarity. Milner argues that nation-states are polyarchies of institutional and societal actors, each with a distinct set of preferences. For her model, the relevant actors are the executive, the legislature, and interest groups that act as "endorsers," supplying the legislature with information about the proposed agreement if the executive withholds it. Her game consists of two parts. First, the executive and the foreign country negotiate along a unidimensional space. If an agreement is reached, the executive presents it as a take-it-or-leave-it offer to the legislature for ratification. From this beginning, Milner varies the divergence of interests between the actors, the degree of information held by each actor, and the ability of each actor to set the agenda, amend, veto, and make side payments. Milner's formal model allows her to pinpoint exactly when and how domestic politics matters. For example, a legislature that is more dovish (preferences closer to the foreign country) than the executive will have no effect on international cooperation. A hawkish legislature, by contrast, acts as a strong constraint on the terms of the final agreement.

Milner comes to three central conclusions. First, systemic approaches to international relations, including realism, have exaggerated the extent of international cooperation. Relative to the state-as-unitary-actor approach, the introduction of domestic politics never improves the chances of cooperation. As the legislature becomes more hawkish, the increasingly divided government becomes less capable of cooperating with other states. Furthermore, any agreement reached will reflect legislative rather than executive preferences. To some extent, this undercuts claims made by Thomas Schelling and Robert Putnam that a recalcitrant legislature increases the executive's bargaining power in the international arena. Second, interest groups are important not because of their ability to apply political pressure, but, consistent with McCubbins and Schwartz, because they can transmit information to uninformed actors. Contrary to most game-theoretic approaches, Milner argues that imperfect information, rather than impede cooperation, makes it more likely. Legislatures will rely on endorsers at different ends of the political spectrum to signal the content of the agreement. Thus, no endorsements lead to rejection, whereas multiple endorsements lead to ratification. Third, changes in the ratification procedure (i.e., a popular referendum rather than a legislative vote; a supermajority rather than a majority vote) made after an agreement has been signed will scuttle the chances for ratification. Executives will negotiate an agreement to target the support of a median legislator; a change in the institutional process shifts the location of the median voter, making ratification more difficult.

Milner's approach represents a large step forward for two reasons. First, it moves beyond a plethora of rather stale debates in international relations. With one model, Milner has addressed the literatures on strong and weak states, presidential versus parliamentary systems, and the importance of societal actors versus political institutions. Her framework represents a theoretical advance because it incorporates these disputes and much more. Second, the model specifies the precise causal mechanisms through which legislative and societal actors influence foreign policy. Executives anticipate the preferences of legislators and endorsers prior to negotiating with other countries. Thus, legislators do not influence international relations through active measures but by letting leaders and interest groups do their work for them.

The flaws in this book emerge when one turns from the theoretical to the empirical chapters. Milner tests her model on four paired sets of cases. She looks at Anglo-American negotiations during the 1940s on exchange rates and trade, oil, and aviation; Franco-German negotiations on the European Coal and Steel Community and the European Defense Community; and, more recently, NAFTA and the Maastricht Treaty. I found the testing strategy unsatisfactory. On the one hand, the hypotheses are specific enough to have permitted some statistical tests. On the other hand, the case studies are incomplete and not sufficiently detailed to show the causal mechanisms at work. For example, Milner asserts that President Truman could not get the International Trade Organization charter ratified in the late 1940s because of the Republican-dominated Congress. All well and good, but she fails to explain why it did not pass when the Democrats controlled both houses of Congress during the 1948–1950 session. This is an example of the tendency to assert rather than delineate the causal evidence. The limited scope of the case studies also prevents the author from seriously assessing alternative explanations to international cooperation. Milner refers to realism and liberal institutionalism at various points, but she admits that "they are not systematically tested in this book" (p. 23). For one case, Milner dismisses the epistemic community argument with one sentence.

Although the evidence is consistent with Milner's approach, there are some puzzles that do not quite jibe with the model's assumptions or predictions. In many cases, the negotiated agreements are supported by centrists but rejected by groups on both extremes. This is inconsistent with her characterization of actors' utility functions. The problem may be that several of the issues under negotiation have more than one dimension. What is also striking is the number of times executives are able to buy off endorsers with side payments and thereby win ratification. What is surprising is why the executive fails to make these payments in cases in which agreements are not ratified. One possible explanation is that side payments are easier to make in cases of economic agreements as opposed to security agreements. An industrial lobby can be satiated with a trade exemption; a military that sees its autonomy undercut is less likely to be mollified by an increased defense budget.

The empirical problems do not undermine the strengths of this book. Milner has created an elegant and useful model of

how domestic politics affects the ability of nations to cooperate with one another. It has more sophistication and precision than any other theory of how domestic politics influences international relations. It will be the benchmark on the subject well into the next century.

The Chemical Weapons Taboo. By Richard M. Price. Ithaca and London: Cornell University Press, 1997. 233p. $32.50.

Marie Isabelle Chevrier, *Harvard University*

I keep in my mind one picture of Saddam Hussein's gassing of his own Kurdish people—a mother curled around her child, both of them frozen in death by chemical weapons. Other images of warfare lurk there as well: victims of napalm, land mines, mortar shells, and nuclear weapons. Each weapon exacts its own horror. Yet, thus far, only chemical and biological weapons have sustained nearly universal condemnation for the past hundred years. Why these and not others? Many weapons, condemned when introduced into warfare, have gained acceptance over time. The abhorrence of chemical weapons, however, has endured.

Many authors have sought to explain the persistence of this condemnation. One line of thought posits that the human reaction to the use of chemical weapons is universal, perhaps genetic, certainly centuries old, mysterious, and maybe unexplainable. Among its roots is the ban against the poisoning of wells. In *The Chemical Weapons Taboo*, Richard M. Price has articulated an opposing position: The prohibition against the use of chemical weapons is a political construct that has evolved throughout this century, growing stronger over time, even as it has sustained some serious violations in World War I and the Iran-Iraq war. Price supports his position by tracing the political genealogy of the chemical weapons taboo from its international political birth at the Hague Conference of 1899 to the entry into force of the 1993 Chemical Weapons Convention in April 1997. In doing so, he follows the construction of the norm, the tempests that it has weathered—albeit not entirely intact—and its reinforcement following such storms.

Price does more than piece together the lineage of the taboo. In original and valuable chapters, he confronts the common understanding that the taboo was shattered by the extensive use of gas warfare during World War I and ignored by Iraq in the Gulf War. He carefully compiles evidence to build the case that in these instances the use of chemical agents in warfare was carried out in recognition of the taboo. In both instances, the users of chemical weapons did so only after a period of restraint, and then only with an explanation that attempted to justify the step. Germany claimed that it used chemical weapons in response to French use, while Iraq maintained that chemical weapons were needed to counter the Iranian "human waves" invading Iraq. Although the taboo was weakened in these two cases, the international community responded by strengthening the political and legal constraints upholding the norm.

Equally important, Price revisits effectively and thoroughly the nonuse of chemical weapons in World War II. There were a few cases of chemical weapons use in the interwar period: Italy in Ethiopia in 1935–36 and Japan in its war with China. In striking contrast to World War I, however, none of the major belligerents in World War II used chemical weapons. Price concludes that the burden of providing a justification for breaking the taboo was substantial enough to forestall the preparations necessary to undertake a chemical weapons assault and ultimately a decisive factor preventing their use. He builds a convincing case of the important role of individual heads of state and adds flavor to his narrative by contrasting the positions of Roosevelt, an ardent opponent of chemical weapons, and Churchill, who had many fewer misgivings.

Price's inclusion of the philosophical as well as the political basis of the taboo, and its changes over time, enriches his argument and strengthens his conclusions. His depiction of the image of chemical weapons as "uncivilized" and their historic association with weakness is thought provoking and challenging. The discussion of the development of the ban on poisons is enlightening, particularly the conclusion that poison weapons somehow upset the norms of warfare as a contest of power. Poisons were deemed unacceptable because they gave a great capacity for destruction to relatively weak states. Thus, powerful countries condemned the use of poisons as an unacceptable form of warfare to avoid the risk of conflicts that would not necessarily be won with superior economic and military strength.

Price makes a major contribution to the understanding the vicissitudes of the chemical weapons taboo, but his book is marred by some weaknesses. Most important, Price sets up his explanation of the taboo, as a twentieth-century political construct, in opposition to the view that chemical weapons are a modern form of an ancient abomination, recast in different guises over time. He appropriately rejects both the essentialist argument, that the prohibition stems from the cruelty of the weapons, and the realist argument, that chemical weapons are useless militarily, as an inadequate explanation of the durability of the taboo. Price argues that diplomats at the Hague Conference viewed chemicals as different from poison. The latter weapons had a long history, but chemical weapons, he claims, were not treated as a variant or subset of poisons but as a new category. His evidence for this novel contention is intriguing, but some of his conclusions are not convincing. He does not present the evidence from both sides of this argument, weigh it in a balanced fashion, and support his conclusion in the face of evidence from both sides.

Moreover, Price's insights do not depend on separating the chemical from the poison taboo. The two theories explaining the taboo are not exclusive. It is perfectly possible to accept the validity of what Price says concerning the political construction of the chemical weapons taboo while viewing it as a cloak surrounding and protecting the time-honored poison taboo. His observation that the moral, perhaps primeval, objection to the use of poisons was not a sufficient explanation for the strength and duration of the chemical weapons taboo does not necessarily lead to the conclusion that its origins are elsewhere. Nor does the existence of the centuries-old objection to poisons, attached to the political construction now defined as the ban against the possession of chemical weapons, mean that the germ of abhorrence to poisons was absolutely necessary to the establishment of the political structure. By abandoning the poison taboo, Price is unable to evaluate the question of whether the chemical weapons taboo could have been established without its historical roots, with the implications for establishing barriers to the use or possession of other types of weapons.

Price's use of obscure words and needlessly complex sentences is likely to frustrate even erudite academics. It is almost certain to discourage the casual reader of military history from what is an otherwise fascinating look at an important subject. The purpose of academic research, particularly in the social sciences, is to make new ideas and important findings accessible to a wide audience, not simply the experts in a narrow subfield.

Despite the above reservations, Price's study of the polit-

ical history of the chemical weapons taboo in the twentieth century is an important and intelligent contribution to the field, and it is a model of interdisciplinary research covering areas of philosophy, history, political science, ethics, and security. It is likely to be an enduring record of the importance of tackling complex subjects in a forthright manner.

Global Challenges: An Approach to Environmental, Political, and Economic Problems. By Todd Sandler. New York: Cambridge University Press, 1997. 234p. $54.95 cloth, $16.95 paper.

Michael McGinnis, *Indiana University*

Todd Sandler uses a common set of analytical tools to analyze an unusually wide range of substantive concerns. He presents the necessary tools of game theory and public policy in a very accessible form. Throughout the book the focus remains on transnational dilemmas involving the environment (acid rain, ozone depletion, global warming, deforestation, loss of biodiversity) as well as terrorism, nuclear proliferation, and the transmission of disease. All are treated as instances of collective action in response to different forms of market failure.

The argument builds on basic justifications for government intervention in the presence of externalities, public goods, or poorly defined property rights. Since national governments jealously protect their sovereignty, some practices are not easily applicable to transnational problems. But Sandler argues that many options short of global governance are still available. His overriding theme is that many regional and global environmental problems can be solved without signing international treaties or establishing global organizations, provided the incentives are right. Once sufficiently credible information is available, he argues, most transnational problems can be resolved in one way or another.

This notion that different collective action problems call for different responses is hardly novel, so the contribution of this book must lie in the ways the author distinguishes among types of problems. Sandler provides a series of useful distinctions, each clearly illustrated with succinct descriptions of appropriate examples. Analysis quickly moves beyond such standard distinctions as the contrast between local environmental problems with immediate and easily observable consequences (such as acid rain) and problems of global scope involving longer term and uncertain consequences (such as global warming). Of particular interest is his use of maps to illustrate why European nations found it easier to cooperate on reducing sulfur emissions than nitrogen oxide, even though both are components of acid rain.

Less well-known distinctions are put to innovative uses. Sandler argues that unequal distributions of effort may facilitate production of public goods characterized by a "best-shot" production technology (such as discovering a cure for a new disease) while at the same time making it difficult to produce public goods with a "weakest link" technology (such as prophylactic measures taken to limit the spread of that disease). In the former case, levels of effort are related to the private advantages large contributors gain from development of a novel solution. In such circumstances, Sandler argues that the international community should act to reinforce or encourage unequal allocations of benefits. This (limited) celebration of inequity as a solution to some public goods provision problems is perhaps the clearest instance of his avowed intention that this book be seen as "provocative."

What I found most intriguing was Sandler's analysis of intergenerational justice. Sandler argues that the current population of the world is, in effect, engaged in a strategic interaction with subsequent generations. The current generation has a "first-mover advantage," since it can structure the range of choices available to later generations. This second mover has less effect on the outcome than in models of contract disputes or election campaigns, but emotional ties between generations may make the first player less likely to exploit this strategic advantage (under some conditions). This example nicely illustrates the benefits of forcing theory and substance to interact on a sustained basis; after reading this book, one's vision of both theory and substance may be changed. In particular, distinctions between national and international politics break down as the reader comes to realize that the same collective action problems and a similar (but not identical) range of solutions occur in both arenas.

Overall, the diverse components of this analysis are tightly integrated into a logically coherent whole. Yet, portions of the argument could have been more fully justified, and possible extensions or elaborations are left undeveloped. For example, controversies concerning income redistribution as a justification for governmental intervention are not discussed. Also, contrasts between democratic and autocratic regimes are not presented systematically. In different chapters democratic regimes are reputed to be both *less* concerned with maintaining a long-term reputation for toughness (by negotiating with terrorists) and *more* concerned with the longer term welfare of future generations (by protecting the environment). There may not be a direct contradiction here, since different actors (officials seeking reelection versus public preferences) were used in these examples, but the overall implications of democratization remain unclear.

The chapter on the "architecture of institutions" posits a developmental sequence in which a small group of like-minded states cooperate on an ad hoc basis and generate reliable information about the magnitude of the problem, thus providing the basis for negotiation of generally applicable treaty restrictions. The author admits this process may need to be supplemented with a minimal global infrastructure that can define property rights and sanction treaty violators. One important question was left unexamined, however. Since, as the author demonstrates, separate environmental problems are interrelated in the physical world, is it not possible that the international community needs to respond in a more comprehensive fashion? I share the author's skepticism regarding calls for comprehensive schemes of global governance, but I would like to have seen a stronger analytical basis for his dismissal of this alternative.

One unfortunate omission is application of the theoretical argument to multinational corporations and nongovernmental organizations. Sandler focuses almost exclusively on the incentives facing national governments, which remain the single most important type of actor in transnational dilemmas. He does discuss INTELSAT as an example of nongovernmental provision of a club good, but this possibility could easily have been included in the initial presentation of the theoretical framework. (Some public policy analysts see nonprofits as filling gaps left open by market and government failures.)

This book develops a series of important connections between theory and substance in an eminently clear fashion. Basic background data are provided for major environmental concerns (plus transnational terrorism), but an expert in any one substantive area is likely to feel slighted by the frequent changes in topic. A reader seeking technical details will be disappointed by Sandler's informal style. Such readers can turn to other sources, including books by this same author.

But when it comes to demonstrating the benefits of comparison across different substantive issues, the importance of cross-issue linkages, or the value of communication between policy analysts and game theorists, this book would be hard to beat.

Reversing Course: Carter's Foreign Policy, Domestic Politics, and the Failure of Reform. By David Skidmore. Nashville, TN: Vanderbilt University Press, 1996. 256p. $29.95.

Gordon Silverstein, *University of Minnesota*

Jimmy Carter may be the most successful and celebrated former president in American history (though John Quincy Adam's recent appearance in Amistad may give Carter a brief chase). Yet, Carter was all but hounded out of office in 1980, denounced for a disastrous domestic economic policy and a vacillating and ineffective foreign policy. Has Carter simply improved with age (and declining official responsibilities)? Or have we rushed to judgment and condemned Carter's term in the White House unfairly?

It has been argued that the Carter administration's "sheer level and range of activity, if not the results, suggested a foreign policy equivalent of FDR's New Deal" (p. xii, quoting Gaddis Smith). This bold foreign policy agenda had been abandoned, however, by the end of Carter's single term in office. David Skidmore argues that the traditional realist view cannot fully explain this reversal of course. Carter did not naively underestimate the threats facing the United States in the late 1970s. Quite the contrary, Carter understood that the real threat would be a refusal to adjust and reconfigure America's commitments in the post-Vietnam era. Nor was it the case, Skidmore maintains, that Carter's foreign policy was incoherent and ineffective. There were important successes (Camp David, the forging of normal relations with the People's Republic of China, the transition to majority rule in Zimbabwe, the negotiation of the Panama Canal treaty, and the negotiation of SALT II were among them), and it was a policy that was intentionally flexible, nonideological, and responsive.

Yet, it was unsustainable. By the end of his term, Carter had swung back to a more traditional Cold War orientation. The reason, Skidmore suggests, is that Carter's policy of adjustment could not be sold at home. Skidmore uses survey data to support his claim that domestic interest groups were able to defeat the liberal internationalism of Carter's early years.

It is not easy to get a nation inclined toward isolationism to commit significant resources to issues and people across two oceans. To do it, Skidmore suggests, realist foreign policy has to be recast into a doctrinal form built on simple, declarative rationales. These are subject to clear and inflammatory symbolic language that links those policies to traditional American ideological values. Finally, there has to be a clear threat, and often that means an overstated threat so that sweeping solutions can be oversold to the American people. In the case of the Cold War, the result was an activist foreign policy which eventually could not be sustained by a declining hegemonic power. (Skidmore acknowledges the vibrant debate over whether the United States was a declining hegemon, but this discussion is relegated to a footnote.)

For Skidmore, the determining variables in a declining nation's reaction to the need for a foreign policy of adjustment are the degree of external compulsion and the degree of domestic constraint. When external compulsion is high, the realist would expect policymakers to try to adapt and adjust. When it is low, one would expect a state to remain fairly rigid and resist change. But the second variable complicates things: Even when external compulsion is high, a state with a weak domestic power structure will be bogged down by domestic interests, and it will be unable to adjust foreign policy as realists would have it do. "Contrary to the predictions of realist theory, such states might well pursue strategies of resistance, despite the excessive external costs such a strategy entails. They can better afford to ignore international change than to ignore the vested interests or ideological constraints that favor existing policies" (p. 9). Carter's mistake was not a naive underestimation of the world abroad but an underestimation of the powerful interests that were vested in resisting change in American foreign policy.

The central purpose of this study, the author maintains, is "diagnostic rather than prescriptive" (p. 176), and in its diagnosis it is quite convincing. But there is just enough prescription and analysis in the book to leave the reader looking for more. Skidmore does provide one chapter on the post-Carter administrations of Reagan, Bush, and Clinton, but the book ends without specific prescriptions for how future presidents (or leaders of any other declining hegemon) might avoid Carter's pitfalls.

The book opens the door to a broad theory of hegemonic decline that would integrate realist and institutionalist views, but it leaves the reader wondering about the general application of such an approach. Though Skidmore makes good use of Aaron Friedberg's work (*The Weary Titan: Britain and the Experience of Relative Decline*, 1988) to consider some comparisons of the Carter years to the situation faced by Great Britain before World War I, it is just enough to whet the appetite. Would Skidmore's focus on domestic political structure, interest groups, and elite opinion hold up in a more extensive comparison? One suspects that it would and that such a comparison would bolster the general application of his theory.

Carter may have had the right foreign policy, but he clearly was unable to sell it at home. Skidmore shows us just how high and deep was the opposition, how difficult the task. And though the task may well have been impossible, one wonders just how it was perceived within the administration. Were alternative strategies pursued to defeat the opposition? Were the best strategies employed? Impossible tasks require extraordinary means. The only way out, it seems, was for the Cold War to come to a sudden end.

Skidmore concludes with a discussion of the opportunity presented to Bill Clinton and the United States in the wake of the end of the Cold War. There is an implicit assumption that the American people are now ready and willing to endorse an activist, liberal-internationalist foreign policy. But is that a safe assumption? Are not the same political-cultural forces that drove Truman and others to use extreme rhetoric in an effort to legitimate an activist foreign policy still in place? Is it so clear that we are no longer a nation powerfully inclined to isolationism? If so, then it is all the more important to study how Carter tried to sell his policy—and how that effort failed.

Skidmore convincingly argues that how foreign policy is sold to the American people is crucial, since "the vested interests that form around current policies as well as the strategy chosen for building legitimacy around these policies can alter domestic structures in such a way as to constrain future policy choices" (p. 179). In other words, as you sow, so shall you reap. Certainly, that is a useful lesson for any policymaker, particularly for those making American foreign policy in the post–Cold War era.

Anarchy, Order, and Integration: How to Manage Interdependence. By Harvey Starr. Ann Arbor: University of Michigan Press, 1997. 165p. $39.50.

Steve Chan, *University of Colorado at Boulder*

Much of the international relations literature has been taken up by the debate between the proponents of the realist and liberal perspectives. Instead of setting up these perspectives as dichotomous and exclusive explanations, Harvey Starr argues persuasively that it will be far more fruitful to investigate the scope conditions under which a particular theory should be most useful for accounting for specific empirical phenomena. In this endeavor, the ecological approach formulated by Harold and Margaret Sprout ("Environmental Factors in the Study of International Relations," in James N. Rosenau, ed., *International Politics and Foreign Policy*, 1969) offers an especially useful way to think about the interaction between foreign policy actors and their environments. While certain aspects of actors and environments in international relations endure over time, others change.

The central thesis of Starr's book is that international relations can be best understood as the manner in which actors seek to adapt to their changing environment. This environment is characterized by a variety of systemic attributes including structural anarchy. Contrary to the arguments of realists, however, the formally anarchic nature of the international system is not a very helpful analytical pivot for engaging in research or policy design. The Westphalian institution of sovereign states has been in existence for some time, and the imperative of self-help that this tradition has supposedly engendered is a historical constant that obviously cannot in itself account for variations in international relations. Rather, the key insight to understanding international relations is that while states remain the most consequential actors, they must face and learn to adapt to a changing environment. The dominant feature of this reality is growing interdependencies.

Accordingly, Starr offers a perspective that is dynamic rather than static, and one that stresses the interaction between agent and structure rather than environmental determinism. In this perspective, learning and adaptation play an important evolutionary role whereby actors attempt to overcome the problems of collective action and security dilemma. Consequently, actors are not hopelessly trapped by situational imperatives. They are instead capable of making choices and adjusting behavior so that concerns about defection and free-riding can be alleviated and cooperation to increase mutual gains can become possible. International laws, regimes, and institutions facilitate such mutual partisan adjustment. At the same time, however, Starr's formulation acknowledges that actors are not completely free agents. Although they are able to select policies and adjust conduct, their menu of choice is limited by historical and structural conditions. Thus, both willingness (an actor's volition) and opportunity (constraints imposed by the environment) matter (Benjamin A. Most and Harvey Starr, *Inquiry, Logic and International Politics*, 1989). Either alone is inadequate for understanding international relations.

Significantly, the relevant environment for foreign policy actors is not limited to international conditions. Starr's discussion includes two-level games and the democratic-peace proposition. Thus, the reader is presented with not just an outside-in view of foreign policy, but also an inside-out one. That is, Starr shows that reciprocal influence operates between domestic and international politics. Thus, for example, democratic institutions and cultures have had a pacifying effect on the foreign relations of those countries sharing these attributes. The peaceful relations and "relative acceptance" among such like-minded countries are, as Starr suggests, symptomatic of the more general processes of integration formulated by Karl Deutsch and his colleagues about four decades ago (*Political Community and the North Atlantic Area*, 1957).

As already noted, Starr offers a dynamic view of international relations whereby complex feedback loops link actors and their environments. Actions have consequences that shape and reshape the environment, and the environment in turn presents new challenges and opportunities that influence subsequent behavior. The recent diffusion of democracy across international borders provides one such example. The initiation of democratization by some countries encourages others to follow suit. Thus, foreign events can have domestic consequences. Conversely, as more and more countries adopt democratic institutions and practices, their changing domestic situation can have a global effect in promoting new norms and patterns of international relations. A world populated by a majority of democratic states might reflect very different "rules of the game" than one composed predominantly of autocracies (Bruce Russett, *Grasping the Democratic Peace*, 1993).

Despite its compact size, this book presents an amazingly comprehensive analysis of international relations. It offers an informed discussion of contemporary problems of international cooperation in the post–Cold War world, and it embeds this discussion in the context of enduring issues and classic theories of international relations. Moreover, it presents a very effective synthesis of the writings of some of the most influential scholars of international relations (e.g., Axelrod, Bull, Deutsch, Putnam, Rosenau, Rosecrance, Russett, the Sprouts). This is an outstanding book that all graduate students of international relations should be required to read.

Interpreting Chinese Foreign Policy: The Micro-Macro Linkage Approach. By Quansheng Zhao. Oxford: Oxford University Press, 1996. 281p. $55.00 cloth, $19.95 paper.

Yufan Hao, *Colgate University*

Few subjects are more complicated and mysterious than Chinese foreign policy. During the Cold War, China was the only country that had confronted (militarily at one time) both superpowers simultaneously and used force to pursue its national interests in a way unanticipated by many in the West. As in any major country, foreign policy in China is a multidimentional puzzle, and Western scholars have been trying to explain it since the establishment of the People's Republic. So far there has been little consensus and much frustration in this field of study, to say nothing of the failure to bring it into the mainstream of theoretical inquiry.

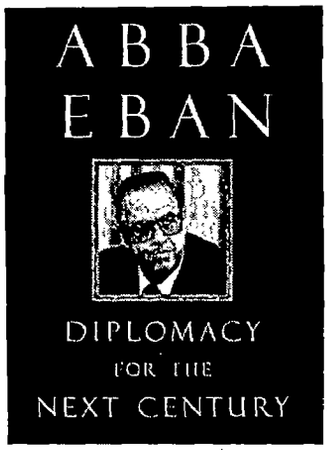
In an ambitious attempt to bridge the gap between Western social science theories and the analysis of Chinese foreign policy, Zhao tries to apply a micro-macro approach adapted from sociological theory (George Ritzer, ed., *Frontiers of Social Theory*, 1990). He argues that one should not look at single-level constraints only (either international or domestic) but, rather, should link different levels of analysis, including the micro levels of individual decision makers and the macro levels of state institutions and the international system (p. 22). In building his multilevel model, Zhao modifies the evolutionary model of Bernhard Giesen ("Beyond Reductionism: Four Models Relating Micro and Macro levels," in *The Micro-Macro Link*, ed. Jeffrey Alexander et al., 1987) and arranges his macroanalysis into three dimensions.

(1) The symbolic macrostructure emphasizes Beijing's interpretation of international and domestic developments and how changes in interpretation have affected changes in priorities since 1978 (chap. 3). (2) The institutional macrostructure stresses change in the Chinese policymaking apparatus and how the shift from one-man domination (vertical authoritarianism) to a decentralized structure with the growth of bureaucratic participation (horizontal authoritarianism) affected Chinese foreign policy making (chap. 4). (3) The power/regime macrostructure examines the sources of Chinese power politics and regime legitimacy as well as individual leaders' policy preferences (chap. 5).

This book is a welcome addition to the literature. Its contribution lies in its effort to call attention to constraints on various levels affecting Chinese foreign policymaking and to bring Western social science theories into the field. It also provides some useful information on how professionals and bureaucrats gained more opportunities to participate in the making of Chinese foreign policy in Deng's era. The suggested framework seems to be well constructed and could be potentially valuable. Unfortunately, the way it is applied is problematic. Zhao seems to have handled the balance between historical facts and his model in a hasty manner. There is hardly any analysis of the influence of individual decision makers on the microlevel in the three chapters dealing with macro changes. The author seems to believe that the "facts" speak for themselves; he simply introduces the model, collects some related information, and presents it without adequate analysis. The reader cannot discern how the microlevel variables interact. The model seems to explain some of the changes in Chinese foreign policy in the late 1970s and early 1980s, but some fundamental questions remain unanswered. How can the change in China's policies toward the two superpowers during the Cold War be explained by this approach? What about the changes in Chinese policy toward neighboring countries, the Third World, Europe, and Japan during and after the Cold War? Why has China resorted to force in pursuing national interests? How and why was the policy toward Taiwan made in the way it was both during and after the Cold War? The three macro chapters are followed by an interesting but not particularly relevant chapter on Japanese development assistance to China, leaving the whole structure of the book in question.

Some historical facts are treated carelessly. For example, Zhao states that China's decision to enter the Korean War in 1950 "prompted President Truman to order the U.S. Seventh Fleet into the Taiwan Straits to guarantee Taiwanese security" (p. 43). But the decision to neutralize the Taiwan Strait was made several months before the Chinese intervention and, to a great extent, prompted Mao's decision to intervene, not vice versa.

Despite the good case study of Japanese development assistance to China, the book falls short of devising a "comprehensive" theory of Chinese foreign policy. But it does suggest an interesting approach waiting to be tested. It is also a commendable attempt to bridge the gap between social science theory and Chinese studies. Zhao's contribution to our understanding of Chinese foreign policy would have been greater had he squarely and accurately incorporated the historical facts into his model, and then used it to interpret major events in China's diplomacy over the past half century.



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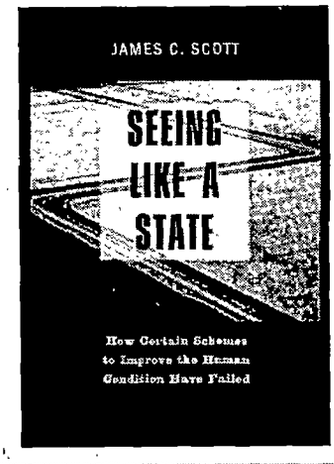

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Individuals, Institutions, and Public Preferences over Public Finance

JOHN MARK HANSEN *University of Chicago*

This study examines public preferences over deficits, taxes, and spending. Using responses to public opinion questions designed for the purpose, the article assesses the state of preferences as expressed by individuals and as represented in government. One section examines the characteristics of individual preferences—their completeness, consistency, and coherence. Public opinion is remarkably well structured and overwhelmingly partial to the policy status quo. A second section explores the properties of mass preferences as they are aggregated by several different kinds of institutional voting rules. Institutions matter, at least to a point: Consistent institutional differences over federal budget policy trace directly to the diverse means by which institutions represent the public's positions. The conclusion assesses the meaning and import of the public's resistance to budget policy change.

Of the many ideas that animate democracy, in both theory and practice, one of the most audacious demands that the preferences of ordinary citizens count for something in the creation of public policy. To this end, democracies contrive that citizens express their views and that public officials attend to them, not least within the elaborate institution of competitive elections.

To critics of democracy, friendly though they are, its confidence in the aptitude of the people and its faith in the capability of institutions are misplaced. To some critics, the problems of democracy stem from the incompetence of the citizenry. People lack complete, consistent, and realistic preferences on most public policy issues, and especially on the most important. They are simply not equipped to choose (e.g., Campbell et al. 1960, chapter 10; Converse 1964; Lippmann [1922] 1997; McClosky 1964; Stokes 1963). To other critics, the problems of democracy inhere in the incapacity of democratic institutions. Whether or not voters are capable, democratic institutions can never realize coherent, decisive, and responsive collective choices, no matter how elegant their design. It takes more than the votes of the people to produce a "democratic" decision (Arrow [1951] 1963; Mueller 1989).

The charges against democratic citizens and institutions echo in the contemporary American debate over the public budget. In the last two decades, the United States has demonstrated its simultaneous susceptibility to the charms of public expenditure and tax reduction, resulting in the 1980s in the greatest peacetime recourse to debt finance in American history. It is not surprising that these developments have drawn considerable interest from politicians, the press, and academe. Their diagnoses of the "budget mess" are varied, but for many, the pathogen lies in democracy itself, in public preferences and the institutions that aggregate them.

By one line of argument, the federal budget is intractable because the public's preferences over spending, taxes, and deficits are fundamentally irrational. In the most malign variant, people hold positions on public budgeting that are blatantly impractical. They believe in the free lunch; they want something for nothing. In the more benign variant, people hold positions on public budgeting that are incomplete or internally inconsistent. They are unable to tell whether they prefer one proposal to another. They are unable to choose among budget alternatives without contradicting themselves. Whatever the cause, whether willfulness or cognitive incapacity, citizens' preferences over the federal budget options are logically incoherent (Buchanan and Wagner 1977; Citrin 1979; Free and Cantril 1967, chapter 2; Ladd et al. 1979; Modigliani and Modigliani 1987; Mueller 1963; Sears and Citrin 1985).

By a second line of argument, the federal budget is intractable because the American institutions that amalgamate public preferences produce only irresolution and stalemate. In the dominant view, the constitutional system of separate institutions sharing powers is a recipe for gridlock, as the president, the Senate, and the House of Representatives register the same public preferences differently. Gridlock freezes the budgetary status quo, even in instances in which majority rule produces a winner. But even majority rule, we know, cannot be counted upon to yield coherent collective choices, particularly when options are multifaceted. The state of the federal budget illustrates not irrationality of preferences but irrationality of process (Brady and Volden 1997; Inman and Fitts 1990; Krehbiel 1996; McCubbins 1991; Roubini and Sachs 1989; Tabellini and Alesina 1990; for a review see Peters 1991, chapter 4).

This article takes a hard look at individuals, institutions, and their preferences over public finance. Using responses to public opinion questions designed for the purpose, it assesses the state of public preferences as expressed by individuals and as represented in government. The next section examines the characteristics of individual preferences—their completeness, consis-

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This research originated during a fellowship at the Center for Advanced Study in the Behavioral Sciences, with support from the National Science Foundation, grant SES-9022192. For assistance with the research, I am indebted to Meredith Rolfe. For helpful comments, I am especially grateful to Stephen Ansolabehere, Larry Bartels, James Fearon, Taeku Lee, Laura Stoker, Craig Volden, John Zaller, the members of the Planning Committee for the 1995 American National Election Pilot Study, and the anonymous referees.

tency, and coherence. Public opinion is remarkably well structured and overwhelmingly partial to the policy status quo. The following section explores the properties of mass preferences as they are aggregated by several different types of institutional voting rules. Institutions matter, at least to a point: Consistent institutional differences over federal budget policy trace directly to the diverse means by which institutions represent the public's positions. Still, the analysis raises questions even as it answers them. The conclusion confronts one lingering issue, the meaning and import of the public's resistance to budget policy change.

INDIVIDUAL PROPERTIES OF PREFERENCES OVER SPENDING, TAXES, AND DEFICITS

Despite the perennial importance of federal budget making, information about the views of the public is sparse and unsystematic. Most of the survey data concern voters' inclination to spend more, less, or the same on a host of public programs (and reveal that majorities wish to spend more money on just about everything; see Advisory Commission on Intergovernmental Relations 1972, 1974–94; Page and Shapiro 1992, chapter 4; Thomas 1991). More seriously, even the best survey instruments have neglected the essential aspect of the public budgeting problem: the tradeoffs inherent in establishing public priorities. Typically, survey questions offer respondents unrealistic choices among incomplete sets of options (Page and Shapiro 1992, 161; Sanders 1988; Tuchfarber, Oldendick, and Bishop 1984; Welch 1985).

As an alternative, the 1995 Pilot Study for the 1996 American National Election Study (NES) asked 12 questions to a national probability sample of 486 Americans of voting age, randomly resampled from the respondents to the 1994 NES (Rosenstone et al. 1995).[1] By design, the questions offered respondents choices among a complete set of budget tradeoffs. They divided the budget options into two exhaustive and mutually exclusive revenue categories—taxes and deficits—and two exhaustive and mutually exclusive expenditure categories—domestic and defense. By design, moreover, they presented respondents with realistic budget choices, that is, with tradeoffs. For each pair of policies, the questions asked people to accept or reject a budget proposal that made them better off on one dimension (by lowering taxes, reducing the deficit, raising domestic outlays, or increasing defense spending) but left them worse off on another (by raising taxes, enlarging the deficit, reducing domestic programs, or cutting back on defense).[2] The questions, in short, derived from the structure of the public budgeting problem. By putting everything on the table and by posing tradeoffs, the questions reproduced the fiscal choices before the nation. The complete battery, with marginals, is given in Table 1. (For more details about the study and its administration, see the Appendix.)

By virtue of the correspondence between the questions and the policy problem, respondents' choices among the tradeoffs revealed their preferences for spending and extraction relative to the status quo. On the most basic level, each acceptance indicated a preference for a new policy, and each rejection indicated a preference for existing policy (Samuelson 1938, 1948; Sen 1973; Varian 1982). Reviewing Table 1, the most striking feature of the responses is the prevalence of tradeoffs refused, that is, the popularity of the status quo. Majorities accepted every tradeoff that cut defense spending but rejected every other, usually by overwhelming margins. Only one respondent in three was willing to raise taxes or increase the deficit to finance greater domestic spending. Just one in five was inclined to cut domestic spending or widen the deficit to lower taxes. No more than one in eight was disposed to do anything at all to increase defense spending.[3] On the face of it, the problem the public creates for policymakers is that it does not want what its leaders seem to want (cf. White and Wildavsky 1989).

Otherwise, however, the public's opinions on the federal budget were hardly troublesome. First, as Table 1 shows, most individuals had complete preferences. Together, "don't know" responses, refusals, and administrative failures to ascertain did not exceed 3% on any of the 12 items; the average was 1.5%. Nearly 89% of the respondents gave valid replies to every question. Of the remainder, only one-third (4% in all) failed to give an answer more than once. Second, most individuals had coherent preferences. Because the question battery balanced each policy proposal with its direct opposite, respondents' choices within each pair declared their rankings of the two alternatives and the status quo. Individuals who accepted one side of a tradeoff and rejected the other revealed a preference for one option over the other and the status quo. Similarly, respondents who rejected both sides of a tradeoff revealed their preference for the status quo.

If we can assume that people's preferences are convex (single-peaked), then their choices in tradeoffs revealed still more. Individuals who accepted one side of a tradeoff and rejected the other displayed their preference for one policy package over the status quo over the opposite package. In so ordering the outcome

[1] To be sure, the sample size is not large. Six of the twelve items were extended onto the 1996 NES, those dealing with tradeoffs among deficits, taxes, and domestic spending. Although the cuts halve the administration time, they seriously undermine the ability to make inferences about citizens' budget preferences, as the options presented are no longer exhaustive.

[2] These are gross categories, of course, and the analytical possibilities would undeniably have been richer had the categories been finer. Unfortunately, the complete set of tradeoffs requires $k \times (k - 1)$ questions for k budget categories, and survey time is limited. The division of federal expenditure and federal revenue into two categories each, I believe, was a reasonable balance between realism and detail, on the one hand, and cost, on the other.

[3] Particular hostility to defense spending but overall partiality to the budgetary status quo has emerged in a number of other studies as well, here and abroad (Cullis and Lewis 1985; Hibbs and Madsen 1981, 428–9; Kay et al. 1992; Page and Shapiro 1992, chapter 4; Thomas 1991; *Times Mirror* 1989). Favoritism toward the status quo has been more generally observed as well (Samuelson and Zeckhauser 1988). I have more to say about both subjects later in the article.

TABLE 1. Question Wording and Marginals

Each year the government in Washington has to make decisions about taxes, spending, and the deficit. We'd like to know your opinions about what the government should do about the budget. I'm going to read you three proposals for cutting the federal budget deficit, and I'd like you to tell me whether or not you would favor each of them.

	Yes, Favor	No, Do Not Favor	Don't Know	Refused, Not Ascertained	Total (*N*)
Do you favor increases in taxes paid by ordinary Americans in order to cut the federal budget deficit?	23.7%	75.3	0.6	0.4	100.0% (486)
Do you favor cuts in spending on domestic programs like Medicare, education, and highways in order to cut the federal budget deficit?	21.8%	76.1	1.9	0.2	100.0% (486)
Do you favor cuts in spending on national defense in order to cut the federal budget deficit?	55.4%	42.6	1.4	0.6	100.0% (486)

Now I'm going to read you three proposals for cutting taxes, and I'd like you to tell me whether or not you favor each of them.

	Yes, Favor	No, Do Not Favor	Don't Know	Refused, Not Ascertained	Total (*N*)
Do you favor cuts in spending on national defense in order to cut the taxes paid by ordinary Americans?	52.5%	45.3	1.9	0.4	100.1% (486)
Do you favor cuts in spending on domestic programs like Medicare, education, and highways in order to cut the taxes paid by ordinary Americans?	19.8%	78.8	1.2	0.2	100.0% (486)
Do you favor an increase in the federal budget deficit in order to cut the taxes paid by ordinary Americans?	16.7%	81.1	2.3	0.0	100.1% (486)

Now we'd like to ask you a few more questions about what the government in Washington should do about the budget. I'm going to read three proposals for increasing spending on domestic programs like Medicare, education, and highways. I'd like you to tell me whether or not you favor each of them.

	Yes, Favor	No, Do Not Favor	Don't Know	Refused, Not Ascertained	Total (*N*)
Do you favor cuts in spending on national defense in order to increase spending on domestic programs like Medicare, education, and highways?	59.7%	38.3	1.0	1.0	100.0% (486)
Do you favor an increase in the federal budget deficit in order to increase spending on domestic programs like Medicare, education, and highways?	30.9%	67.7	1.2	0.2	100.0% (486)
Do you favor increases in the taxes paid by ordinary Americans in order to increase spending on domestic programs like Medicare, education, and highways?	34.0%	64.6	1.0	0.4	100.0% (486)

Now I'm going to read you three proposals for increasing spending on national defense. I'd like you to tell me whether or not you favor each of them.

	Yes, Favor	No, Do Not Favor	Don't Know	Refused, Not Ascertained	Total (*N*)
Do you favor an increase in the federal budget deficit in order to increase spending on national defense?	14.6%	84.0	0.8	0.6	100.0% (486)
Do you favor increases in the taxes paid by ordinary Americans in order to increase spending on national defense?	12.7%	86.8	0.4	0.0	99.9% (486)
Do you favor cuts in spending on domestic programs like Medicare, education, and highways in order to increase spending on national defense?	9.3%	90.1	0.2	0.4	100.0% (486)

TABLE 2. Revealed Preferences in Tradeoffs (Percentage of Yes/No Responses)

Tradeoff Dimension	Prefer First Named	Prefer Status Quo	Prefer Second Named	Prefer Both at Once	Total (*N*)
Lower deficits versus lower taxes	19.2%	64.0	11.9	4.9	100.0% (470)
Lower deficits versus greater domestic spending	19.6%	49.2	28.7	2.6	100.1% (470)
Lower deficits versus greater defense spending	52.3%	32.8	10.6	4.3	100.0% (470)
Lower taxes versus greater domestic spending	15.4%	50.3	29.6	4.7	100.0% (473)
Lower taxes versus greater defense spending	50.7%	36.4	9.7	3.2	100.0% (473)
Greater domestic spending versus greater defense spending	59.0%	31.7	7.4	1.9	100.0% (473)

states, they revealed priority or strict preference for one budget option over the other. For instance, as can be seen in Table 2, by agreeing to defense cuts in order to lower taxes and then objecting to higher taxes in order to raise the military budget, nearly 51% disclosed their preference for lower taxes over the status quo over more defense. Similarly, respondents who rejected both sides of a tradeoff displayed their preference for the status quo—for the current balance of the two options—over both alternative policies. And although respondents provided no information about their rankings of the two alternatives relative to each other, it will be useful to the investigation—and conservative—to treat them as having revealed indifference between the two policy options. Thus, by rejecting cuts in domestic spending in order to close the deficit and then rejecting larger deficits in order to fund domestic programs, 49% expressed their satisfaction with the existing balance between deficit finance and domestic expenditures. Finally, respondents who accepted both sides of a tradeoff revealed inconsistency in their preference between the two alternatives and the status quo. By agreeing first to reallocate money from domestic programs to defense and then agreeing to reallocate money from defense to domestic purposes, 2% indicated their inability to keep their preferences straight.[4]

As Table 2 shows, most individuals made consistent choices across the six budget policy pairs. It is not surprising, given the marginals, that indifference between policy alternatives and support for current policies predominated. Majorities strictly preferred smaller deficits, lower taxes, and greater domestic expenditure to larger defense outlays or the status quo. For every other policy pair, majorities or near-majorities endorsed the existing state of affairs.

Perhaps more surprising is that only a small portion of the public revealed policy choices that were inconsistent. Just 5% simultaneously subscribed to tax finance for deficit reduction and deficit finance for tax reduction, and they were the most numerous. Across the six pairs of budget tradeoffs, moreover, only 12% made inconsistent policy choices even once, and most—nearly three-quarters—did so only once, as can be seen in Table 3, column 1. The consistency is all the more remarkable because of the difficulty of the task the battery presented to respondents. For four out of the six pairs of budget alternatives, the opposite sides of tradeoffs were separated by 15–20 minutes of survey content. (See the Appendix.) The instrument gave respondents ample opportunity to forget their answers to the first side of a tradeoff by the time they encountered the second.[5] Thus, the portion of the electorate that seeks something for nothing is decisively in the minority. Most people recognize that they cannot have budget tradeoffs both ways.[6]

[4] For elaborations of the underlying choice theory, see Hansen (1995) and Ordeshook (1986, 33–7). The inferences clearly depend upon the convexity assumption. In this context, I believe that the assumption is justified. The questions structure each tradeoff on a single dimension that is conceptually continuous, and it is difficult to contrive a "sensible story" about preferences over taxes, deficits, and spending that implies multiple local ideal points on a single continuous dimension. Even if the assumption is unjustified, however, its effect is most probably conservative. It will bias the study against the finding that individual budget preferences are consistent with the axioms of rationality.

[5] The two tradeoffs whose opposite sides were administered a few minutes apart, deficits paired with taxes and domestic paired with defense spending, should be most susceptible to remembered responses. But as Table 2 shows, the first pair had the largest proportion of inconsistent responses, and the second pair had the smallest. In fact, inconsistencies were more likely to involve taxes than other options. As Table 2 also shows, the two tradeoffs with the greatest incidence of inconsistent responses paired taxes with deficits and with domestic spending, both nearly 5%. (The pairing between taxes and defense spending had the fourth highest incidence.) By conjecture, I attribute this tendency to the stubbornness of "supply-side" views, even though supply-side arguments deny a tradeoff between tax *rates* and revenues, not between tax revenues and other options.

[6] Inconsistencies appear to be a mix of mistaken response (that is, measurement error) and willfulness. I shall present evidence of measurement error below. Other analyses show that the probability of an inconsistent response is predicted most strongly by education (educated people are more likely to recognize the tradeoffs) and by intensity of preference *for* spending (people who score highest on the NES domestic and defense spending batteries are less likely to observe the tradeoffs). The probability of an inconsistent response was unrelated to interest in politics, political knowledge, and the interviewer's assessment of the respondent's intelligence (Hansen 1996, 3–4 and Table 5).

TABLE 3. Incidence of Transitive, Intransitive, Inconsistent, and Incomplete Preference Orderings

	As Originally Revealed	With One Amendment[a]
Transitive orderings	40.3%	80.5%
Strict transitive	3.9	7.8
Weak transitive	36.4	72.6
Intransitive orderings	36.0	5.5
Inconsistent orderings	12.4	6.7
Incomplete orderings	11.3	7.2
Total	100.0%	99.9%
(*N*)	(486)	(486)

[a]For details of the amendment procedure, see the Appendix.

Finally, the greatest number of individuals revealed preferences in pairwise choices that were consistent with each other. Taken together, respondents' replies to the 12 questions established their preference orderings over the four budget policy options. With four preference objects—smaller deficits, lower taxes, greater domestic spending, and greater defense spending—there are 24 strict preference orderings plus another 51 weak orderings, which include what I have provisionally defined as indifference, for a total of 75 transitive orderings.[7] As shown in Table 3, column 1, a plurality of the respondents, 40.3%, revealed preference orderings that were completely transitive. Consistent with the survey marginals, preference rankings that included indifference were most numerous: Weak preference orderings exceeded strict preference orderings by a ratio of 9 to 1 (36.4% versus 3.9%). And although the data are not reported in the table, indifference predominated in the most frequent responses. Overall, one in every twelve respondents answered "no" to every proposal, revealing indifference over the four budget options or, equivalently, satisfaction with current budget allocations. But the most common response profile combined indifference and strict preference. One respondent in every ten accepted every proposal that reduced defense spending and rejected every other, revealing indifference among deficit reduction, tax cuts, and domestic spending growth but strict preference for each over military spending.

The remainder of the respondents, 36%, had complete and consistent but intransitive preferences over public budgets. (See Table 3, column 1.) Even here, however, the problems were not very worrisome. Not a single individual revealed a strict preference cycle: None preferred A to B, B to C, and C to A. Only a bare handful—about 5%—violated strict preference transitivity because of an indifference relation: They preferred A to B, considered B equivalent to C, but still preferred C to A. Consistent with previous research, almost all the people who revealed intransitivities violated the transitivity of what I have assumed to be the indifference relation: They found A equivalent to B and B equivalent to C, but they preferred A to C. The phenomenon of intransitive indifference has been given a number of interpretations (Fishburn 1970), but the most common holds that the difference between two options must surpass a "preference threshold" in order for people to discern it (Armstrong 1950; Luce 1956).[8] I will develop a somewhat different interpretation when I return to the question in the conclusion.

In sum, mass preferences over public finance are remarkably well structured, all in all. Most people have no trouble determining their position on budget policy proposals. Most people have no difficulty avoiding blatant inconsistencies in their choices. Many people have preferences that are complete, consistent, and transitive—this is the modal category—and most of the rest fall prey to only the mildest form of preference intransitivity. The view that ordinary Americans are irrational in a fundamental sense is difficult to sustain.

But perhaps this conclusion is the product of low expectations, of the pessimist's relief that the glass is only half empty or the optimist's joy that the glass is half full. After all, the proportion of the electorate with fully "rational" preferences is well short of a majority, and substantial numbers of people reveal inconsistent or incomplete preferences.

The objection is well taken, but it does not get very far. The estimates of completeness, consistency, and transitivity reported here are more likely than not conservative. It is not possible to tell for sure from these data, but if individuals' preferences are not, as assumed, single-peaked, then actual violations of consistency and transitivity were almost certainly less frequent than stated. Moreover, if individuals' preference for the status quo in a pair of tradeoffs does not also imply indifference between the two budget alternatives, as I have assumed, then intransitivities of indifference may not exist at all. Again, it is impossible to judge from these data, but if all the cases interpreted here as indifference are instead unobserved preference,

[7] Without much more information, infeasible in a survey, I cannot determine the respondents' preference orderings over outcome states. I cannot tell, for example, whether they prefer lower taxes and less defense expenditure to greater domestic spending and higher taxes, or vice versa. With the information and principles of inference at hand, however, I can assess the internal consistency of their implied preference orderings over options, as I do here. The ability to identify orderings over options but not over outcome states probably has a conservative effect on the conclusions. There may be people whose preferences over objects appear inconsistent even though their preferences over states are entirely coherent and even though they have reported their preferences correctly. There may also be people whose preferences over objects appear consistent but whose preferences over states are incoherent, but it seems less likely. Given the nature of their preferences, they would find it difficult to report their preferences without either admitting their confusion or choosing randomly.

[8] Intransitive indifference is so common that it has spawned its own literature, capably reviewed by Fishburn (1970). It also has given rise to a special designation for these almost-transitive preference orderings: quasi-transitivity (see Radcliff 1993). Nearly 72% of the respondents have quasi-transitive preferences. Of course, even intransitivities of the indifference relation violate strict preference transitivity in one sense: If A is preferred to C, C is equivalent to B, and B is equivalent to A, then A is implicitly preferred to A. Brady and Ansolabehere (1989) also found substantial incidence of indifference intransitivity, and their data (choices from among sets of presidential candidates) allow them (unlike me) to estimate the size of the threshold. They found the size of the threshold closely related to the amount of information people have about the candidates.

TABLE 4. Incidence of Partial Preference Orderings, in Percent

	As Originally Revealed	With One Amendment[a]
All four options ordered	40.3%	80.5%
Two of four options ordered	3.3	3.1
One of four options ordered	18.3	10.1
None of four options ordered	38.1	6.4
Total	100.0%	100.1%
(*N*)	(486)	(486)

[a]For details of the amendment procedure, see the Appendix.

and if the unobserved preferences are in fact internally consistent, then not 40% but 76% of the respondents had complete, consistent, and transitive preferences over the four budget options.

But suppose the assumptions I have made in drawing inferences from these data are merited. The objection still does not go far. First, survey measurement is notoriously error prone, and with twelve questions asked in the context of dozens of others over a three-quarter-hour span, people have many opportunities to make mistakes, to reply "no" when they mean "yes," and vice versa. In one of their experiments, Brady and Ansolabehere (1989, 149–50) allowed subjects to review their choices from among presidential candidates. Their respondents invariably amended their answers in the direction of greater—often *much* greater—congruity.

The Pilot Study did not allow these 486 individuals to review their responses, but we know that 81% of the respondents were within a single answer of transitivity.[9] (For the evidence, see Table 3, column 2; for the method of calculation, see the Appendix.) Ninety-one percent of the individuals whose preferences were complete and consistent but intransitive could have achieved transitivity by amending their answer to a single survey question, most often by changing a "no" (indifference) to a "yes" (strict preference).[10] An overwhelming majority of the American public is very nearly fully rational in its budget preferences.

Second, the vast majority of the respondents expressed preferences in pairwise comparisons that were logical and informative, even if they did not comply fully with the axioms of rationality. As Table 4 indicates, despite incompleteness, inconsistency, or intransitivity, at least one budget option can be ordered relative to the other three in 62% of the cases. (At worst, that is, we can tell that one option is first or last in priority, even if we cannot order the other three. Or we can tell that two options are both superior, both inferior, or one superior and one inferior to the other two options, even if we cannot tell which of the two others the respondent prefers.) With my amendment of a single survey answer, at least one option can be positioned in 94% of the cases. (I will refer to these consistent pairwise orderings as a "rational binary preference" and employ them throughout the analysis of institutional aggregation in the next section.) Put differently, individuals revealed a coherent ordering in at least half the pairwise comparisons almost two-thirds of the time, without any amendment whatsoever.

Finally, presented with the difficult task of keeping their preferences straight on 12 questions asked among 200 items over 45 minutes, people did much better than chance. Leaving aside the option of nonresponse, probability theory predicts that just 2% of the 4,096 possible patterns of yes-no answers to the 12 questions will correspond to complete, consistent, and transitive preference orderings, yet 40% of the respondents exhibited them. Guesswork should yield 21% of the sample no more than one answer short of a transitive orderings, but 81% of the respondents were in this category. The respondents, that is, were much better than lucky. Given the demands of the task, ordinary citizens exceed high expectations.

The conclusion is optimistic but should come as no great surprise, at least on one level. It squares with the findings of Brady and Ansolabehere (1989) and Radcliff (1993), who investigated the rationality of choices from fields of candidates,[11] as well as the more limited findings of Sanders (1988), who investigated attitudes on public spending. Clearly, the strategies of low-information rationality are very well suited to the assessment of budgetary tradeoffs (Page and Shapiro 1992; Popkin 1991; Sniderman, Brody, and Tetlock 1991). People do not need to know very much about the substance of the federal budget to address alternatives to it. They need to know only a little about the rough contours of current policy to decide whether they like it (and they may not need to know even that much). Moreover, in assessing what they might like better, citizens have lots of free information to help them. First, because the media cover budget debates extensively, as they did especially in 1995, people can draw upon their vague knowledge of the alternatives on the agenda and who is for and against them (Carmines and Kuklinski 1990).[12] Second, because the political parties have distinct positions on budget issues, people can assess alternatives through the lens of their own partisanship (Campbell et al. 1960, chapter 6; Popkin 1991, chapter 3). Finally, and most important, because people pay taxes, borrow, and enjoy domestic and defense services from government, they can rely upon their own experiences. Taxes, spending, and deficits are issues about which lots of people know a fair amount, at least enough to tell whether they are satisfied.

[9] Ninety-two percent—given missing data, nearly all—were within two answers.

[10] Of course, there may be some respondents who reveal transitivity by mistake, but given that so few of the Brady and Ansolabehere (1989) subjects increased incongruity upon reflection, I doubt there are many.

[11] In fact, they found even better performance, but they gave their respondents an easier task: fewer questions, fewer alternatives, arguably fewer evaluative dimensions, and certainly a more familiar exercise—choosing a candidate.

[12] In fact, the public's performance in 1995 may well represent the high end of the possibilities. If there ever was a time when people should have been aware of budget policy and its tradeoffs, it was during the Pilot Study period of August and September 1995. The new Republican Congress had made its plan to balance the budget in seven years the centerpiece of its legislative program. No doubt, the hard choices involved in making budgets were unusually salient.

At the same time, however, the conclusion is at odds with an earlier body of research that found reasons to doubt the coherence of Americans' fiscal policy preferences (Citrin 1979; Free and Cantril 1967; Ladd et al. 1979; Modigliani and Modigliani 1987; Mueller 1963; Sears and Citrin 1985). The source of the difference is apparent. Questions about tradeoffs do elicit more circumspect opinions on budgetary alternatives, as Page and Shapiro (1992, 161), Tuchfarber, Oldendick, and Bishop (1984), and Welch (1985) conjectured.[13] And well such questions should. They introduce "considerations" of both the benefits and the costs of fiscal policy options; they cause respondents to stop and think, at least momentarily (Zaller 1992; Zaller and Feldman 1992).

Having stopped and thought, a large fraction of the survey respondents opted for the status quo. In fact, more than half the individuals with complete, consistent, and transitive preferences achieved "rationality" by rejecting nine or more of the twelve tradeoffs.[14] By itself, the public's partiality toward the status quo does not raise an issue of rationality. Even wrong-headed views can be well structured. But it may suggest that the balanced form of the questions caused respondents to blunder into transitive response profiles through no design of their own. Other evidence indicates this is not the case. On the one hand, as I have shown elsewhere (Hansen 1996, 2–4 and Table 5), the propensity to accept the status quo is unrelated to education, political knowledge, interest in politics, and the interviewer's assessment of the respondent's intelligence. On the other hand, partiality toward current policy is strongly and inversely related to strength of partisanship and to the strength of preference *for or against* domestic and defense spending. Support for the status quo, that is, appears more deliberate than accidental. The structure of public preferences over public finance is related to the conservatism of those preferences, but the structure seems to be substantive.

Thus, the conclusion holds up. The public has the ability to make budget policy choices with reasonable discernment. Allowed by survey questioners to be practical in their views, ordinary Americans are practical in their views. They have well-formed and well-behaved preferences. Ordinary citizens may not have preferences over spending, taxes, and deficits that experts consider "correct," but their opinions are coherent.

[13] The four budget categories employed in these questions capture only gross policy options, and the public's performance would certainly suffer were the categories finer and the options more numerous. Yet, the evidence here suggests that the task not only is within the cognitive capacities of ordinary citizens but also—and more important—is comprehensible to them. Other approaches can be and have been taken to the measurement of fiscal policy preferences, but I have not been able to discern in them a theoretical basis for inferences about the coherence of mass preferences (Hansen 1995). Whatever the design of the questions, we must (as here) have a model that relates preferences to the answers people give in surveys before we can tell whether they are choosing rationally or irrationally.

[14] Nine rejections is exactly halfway between the extremes. By the rules of inference I have employed in this section, respondents with strict preference orderings should reject six tradeoffs and accept six tradeoffs. Respondents who are satisfied with the status quo should reject all twelve.

INSTITUTIONAL PROPERTIES OF PREFERENCES OVER SPENDING, TAXES, AND DEFICITS

Coherent democratic policy choices require more than coherent individual preferences. Because political institutions intervene between individual and "social" preferences, public choices may be logically inconsistent, even when every individual's private preferences are the model of rationality. As is well known, a majority vote on one among an array of options on a single policy dimension produces a settled outcome only when individuals' preferences are single-peaked. The news gets worse from there. Except under conditions that would seem to be unattainable, majority rule cannot guarantee a determinate social preference when policy choices differ on multiple dimensions (Mueller 1989, chapter 5).

Because of the structure of public preferences in 1995, however, majority rule in this instance causes no such difficulties. Table 5 displays the outcomes of votes on all six of the budget tradeoffs, using rational binary preferences as expressed in the survey and after one of my amendments.[15] As the table shows, the proposal to cut defense spending is the Condorcet winner: A majority favors reducing military outlays, whether to lower the deficit, to decrease taxes, or to increase domestic spending. And assuming there are no restrictions on proposals, whenever individuals vote sincerely in a sequence of majority votes between two options, the Condorcet winner, if it exists, inevitably triumphs (Ordeshook 1986, chapter 6).

Thus, in a majority vote, citizens will opt to cut the defense budget, but to what purpose? As Table 5 shows, they prefer to increase expenditures on domestic programs: Taking individuals who prefer the status quo to be indifferent, a majority favors domestic spending to deficit reduction, domestic spending to tax reduction, and deficit reduction to tax reduction. Hence, if the status quo is voted last, as is commonly the case, then sequential majority rule produces a winner: Reallocate spending from defense to domestic programs. The overwhelming consensus on reductions in military spending combines with some rudimentary rules of parliamentary procedure to produce a majority rule outcome (Shepsle 1979).[16] The individual preferences that are coherent as expressed are also coherent as aggregated by majority rule.

[15] The most dramatic difference between the rational binary preferences as stated by respondents and those I reached with an amendment is the size of the majority supporting defense cuts. In the amended case, the backing for reallocations away from the Pentagon diminishes noticeably, which suggests greater (but still not substantial) latent support for defense. Nevertheless, no matter how much trouble ordinary people have in expressing their preferences consistently, the one thing most of them are sure about is that they do not like defense spending.

[16] In the example, I have assumed sincere voting, as I will throughout for the sake of simplicity. In this particular case, however, the outcome is strategy-proof, although that is not generally true for either sequential majority rule or structure-induced equilibrium (Dion 1992; Mueller 1989, chapters 5, 20). There are too few voters in favor of the defense status quo for their strategic maneuverings to be successful in preserving it.

TABLE 5. Votes in National Referenda by Majority Rule (Rational Binary Preferences)

Tradeoff Dimension	Prefer First Named	Prefer Status Quo	Prefer Second Named	Total	(N)	(Weighted N)[a]
As originally revealed						
Lower deficits versus lower taxes	22.2%	64.7	13.1	100.0%	(221)	
Lower deficits versus greater domestic spending	23.4%	44.7	32.0	100.1%	(244)	
Lower deficits versus greater defense spending	72.0%	20.7	7.4	100.1%	(271)	
Lower taxes versus greater domestic spending	20.2%	43.3	36.6	100.1%	(238)	
Lower taxes versus greater defense spending	71.5%	21.0	7.5	100.0%	(267)	
Greater domestic spending versus greater defense spending	70.9%	20.9	8.2	100.0%	(282)	
With one amendment						
Lower deficits versus lower taxes	22.7%	60.6	16.8	100.1%	(421)	(416.5)
Lower deficits versus greater domestic spending	20.7%	47.3	32.0	100.0%	(434)	(427.5)
Lower deficits versus greater defense spending	59.7%	29.5	10.8	100.1%	(442)	(435.0)
Lower taxes versus greater domestic spending	18.8%	44.8	36.4	100.0%	(437)	(426.0)
Lower taxes versus greater defense spending	59.3%	30.6	10.1	100.0%	(434)	(429.5)
Greater domestic spending versus greater defense spending	62.3%	27.9	9.8	100.1%	(450)	(445.5)

[a]For rational binary preferences as amended, weighted *N* is the total number of full and fractional votes included in the calculation (rounded to the nearest 0.1); *N* is the total number of cases with full or fractional votes included in the calculation. See the Appendix, "Institutional Aggregation of Preferences."

But the United States does not make its budget policy decisions by majority rule. Instead, three different representative institutions, the president, the House of Representatives, and the Senate, must ultimately concur in a budget, and the conflict among the three is an enduring feature of American government. All three institutions represent public preferences, but they do so in different ways. An important way—but by no means the only one—in which they differ is in the voting rules they use to aggregate preferences. The president is the nation's sole plebiscitary officer, the one actor in the legislative process elected by the nation at large. Presidents, therefore, are likely to decide their position by simple majority rule or some variant thereof. The House of Representatives is the nation's parliament, the most partisan of the three legislative actors. It vests the greatest procedural authority in its party leadership, and it elicits the greatest party discipline from the rank-and-file, especially in budgetary matters (Cox and McCubbins 1993; Fenno 1973, chapter 2; Ippolito 1981, chapter 5; Manley 1970, chapters 3, 5; Oleszek 1984, chapter 2; Schick 1980, chapters 4, 7; White and Wildavsky 1989; but cf. Krehbiel 1993). Thus, the House decides its position by parliamentary rule, by the vote of the majority party in caucus. The Senate, finally, is the congress of the nation's states, the one legislative institution whose method of voting is other than popular. With seats apportioned two to each state, the Senate effectively weights citizens' preferences by the inverse of the population of the state in which they reside. For example, a person's vote in Wyoming (voting age pop. 343,000) is worth 68 times more than a person's vote in California (voting age pop. 23 million).

To depict the institutional differences in preference aggregation, I constitute the individuals in the sample as the president, the House, and the Senate by applying three different voting rules. The president assumes the position of the national majority, voting in referendum.[17] The House takes its preference from the majority vote of those represented by members of the majority party. In the Democratic 103d Congress, the only votes that count are cast by people whose representative is a Democrat, and in the Republican 104th Congress, the only votes that count are cast by people whose representative is a Republican. The Senate, finally, determines its position by weighting votes inversely in proportion to the voting-age population of each voter's home state.[18] (For details, see the Appendix.)

I also explore two models of the way institutions

[17] Alternatively, one might weight voters' preferences by their representation in the Electoral College, that is, by the ratio of the number of electoral votes in their state of residence to the voting age population in their state of residence. The weighting, however, makes a minuscule difference to the distribution of preferences, and so I maintain the simpler aggregation rule in this analysis.

[18] One might add to the model of vote aggregation the Senate's effective supermajority requirement, the 60% of the membership required to invoke cloture. (This is a centerpiece of the analysis by Krehbiel [1996] and Brady and Volden [1997].) I shall not do so explicitly here, although I shall note instances in which outcomes in the Senate might be vulnerable to a filibuster.

TABLE 6. Preferences Expressed Through National Institutions under Two Types of Representation

Institution	Type of Representation: Downsian	Type of Representation: Partisan
President	All voters	All Democrats
House of Representatives	All voters represented by a Democrat (103d); all voters represented by a Republican (104th)	All Democrats represented by a Democrat (103d); all Republicans represented by a Republican (104th)
Senate	All voters; preferences weighted by inverse of state population	All Democrats represented by Democratic delegations, all Republicans represented by Republican delegations, all voters represented by split delegations; preferences weighted by inverse of state population

represent the preferences of their constituents. In the first analysis, lawmakers are Downsian, and they adopt the positions of the median voter (Downs 1957). In the second analysis, legislators are partisan, and they adopt the positions of the median of their fellow party members (Lewis 1996). Accordingly, the Downsian president represents the national majority; the partisan president represents the majority of Democrats nationwide. The Downsian House represents the voters whose delegates are members of the House majority; the partisan House represents the voters who share the partisan identification of a member of the House majority. The Downsian Senate represents the weighted majority of all voters; the partisan Senate represents the weighted majority of voters who share their senators' party affiliations. Table 6 summarizes the voting rules for each of the six categories, for each of three institutions crossed by two modes of representation.

Applied to the preferences of the survey respondents, the rules yield estimated policy positions for each institution on each of the six budget tradeoff dimensions. They are shown in tables 7 and 8. To economize on presentation, the tables report only the means of the rational binary preferences with one amendment. I assigned −1 to a preference for the tradeoff that favors the option listed on the left, 1 to a preference for the tradeoff that favors the option listed on the right, and 0 to a preference for the status quo over both tradeoffs. Then I applied the institutional voting rules and calculated the means. Each entry, accordingly, may be interpreted as the center of institutional opinion on a policy dimension that ranges from −1, a unanimous preference for the leftmost option over the rightmost, to 1, a unanimous preference for the rightmost option over the leftmost. In Table 7, for example, the president, at −.493, is strongly in favor of tax reduction through defense cuts but is less enthusiastic than the House (−.538) and more enthusiastic than the Senate (−.415). Conversely, the House, at .148, is more favorable to domestic spending at the expense of the deficit than either the president (.113) or the Senate (.102).[19]

On most budget policy questions, the differences between the positions of the three actors are slight, although they grow a bit more pronounced with the Republican takeover in the 104th Congress (see Table 7). The 103d House, under the control of the Democrats, is consistently the most committed to domestic expenditure; the Senate is consistently the least. Conversely, the Democratic House is distinctly the most enthusiastic about reductions in the military budget, regardless of fiscal purpose; the Senate is distinctly the least.[20] In the Senate as in the House, defense cuts are the majority position, but the margin in the Senate is much, much closer, a matter of true consequence when the effective threshold for passage is the 60% required for cloture.[21] The parliamentary mode of representation in the House amplifies the policy preferences of the liberals who dominated the constituencies of majority Democrats in the 103d Congress, while the federal mode of representation in the Senate magnifies the views of the conservatives who dominated the small

[19] Because the president takes the position of the national median, the means reported in Table 7 are simply the average of the marginals in Table 5. For example, on the tradeoff between tax reduction (−1) and defense spending (1), the president's position (drawing from preferences amended once) is (−1)(.593) + (0)(.306) + (1)(.101) = −.493 (with rounding error). On the tradeoff between deficit reduction (−1) and domestic spending, likewise, the president's position is (−1)(.207) + (0)(.473) + (1)(.320) = .113. The other means in tables 7 and 8 are computed similarly.

[20] This finding seems consistent with more casual observations of the Senate, and it suggests that the Senate's affinity for the military may trace as much to the popularity of defense spending in small states as to the Senate's prerogatives in foreign policy and the historic Republicanism in small states. As explained in the Appendix, however, the NES sampling frame does lend a modest Republican bias to the estimates of the Senate's position.

[21] The Senate majority for reallocations from defense exceeds three-fifths for all proposals voted with rational binary preferences as expressed in the survey. When amendments are made to preferences, however, the Senate is unable to muster three-fifths for any proposal.

TABLE 7. Institutional Representation of Public Preferences: Downsian Representation

	Mean of Rational Binary Preferences Amended Once					
	103d Congress, 1993–94			104th Congress, 1995–96		
Tradeoff Dimension	President (Democratic) (S.E.) (Weighted N/N)[a]	House (Democratic) (S.E.) (Weighted N/N)[a]	Senate (Democratic) (S.E.) (Weighted N/N)[a]	President (Democratic) (S.E.) (Weighted N/N)[a]	House (Republican) (S.E.) (Weighted N/N)[a]	Senate (Republican) (S.E.) (Weighted N/N)[a]
Lower deficits versus lower taxes	−.059 .031) (416.5/421)	−.036 (.040) (259.5/262)	−.075 (.030) (416.5/421)	−.059 (.031) (416.5/421)	−.084 (.040) (233.5/236)	−.075 (.030) (416.5/421)
Lower deficits versus greater domestic spending	.113 (.035) (427.5/434)	.148 (.045) (268.0/271)	.102 (.033) (427.5/434)	.113 (.035) (427.5/434)	.016 (.048) (235.5/238)	.102 (.033) (427.5/434)
Lower deficits versus greater defense spending	−.489 (.033) (435.0/442)	−.501 (.057) (272.0/276)	−.468 (.044) (435.0/442)	−.489 (.033) (435.0/442)	−.497 (.060) (242.0/246)	−.468 (.044) (435.0/442)
Lower taxes versus greater domestic spending	.176 (.035) (426.0/437)	.211 (.048) (262.0/267)	.161 (.034) (426.0/437)	.176 (.035) (426.0/437)	.076 (.048) (238.0/243)	.161 (.034) (426.0/437)
Lower taxes versus greater defense spending	−.493 (.030) (429.5/434)	−.538 (.059) (265.0/268)	−.415 (.043) (429.5/434)	−.493 (.030) (429.5/434)	−.451 (.059) (243.0/247)	−.415 (.043) (429.5/434)
Greater domestic spending versus greater defense spending	−.525 (.032) (445.5/450)	−.557 (.059) (274.5/277)	−.484 (.043) (445.5/450)	−.525 (.032) (445.5/450)	−.442 (.058) (249.0/251)	−.484 (.043) (445.5/450)

[a]For rational binary preferences as amended, weighted *N* is the total number of full and fractional votes included in the calculation (rounded to the nearest 0.1); *N* is the total number of cases with full or fractional votes included in the calculation. See the Appendix, "Institutional Aggregation of Preferences."

states (Ippolito 1981, chapter 5; Schick 1980, chapter 7; White and Wildavsky 1989).[22]

It is no surprise that the relative positions of the preferences of the president, the House, and the Senate are significantly affected by the changeover in partisan control in 1994 (see Table 7). In the 103d Congress, when Democrats reigned over all three legislative venues, Senate preferences tended nearer the *policy* center—the status quo—than presidential and House preferences. In contrast, in the 104th Congress, when Republicans gained control in both chambers, Senate preferences inclined toward the *political* center, between the GOP House and the Democratic president. As envisioned, but for different reasons, the Senate becomes the "balance wheel" of American politics.[23]

Partisan representation only amplifies the tendencies apparent in Downsian representation. As seen in Table 8, the Senate's policy centrism in the 103d Congress and its political centrism in the 104th Congress become plainer still when legislators are taken to be partisans. The upper chamber's position as guardian of the defense budget is more obvious in the 103d, even though it loses its place to the Republican House in the 104th. The GOP cast of the Senate is more evident, even under Democratic control in the 103d and particularly on domestic spending, for which Senate preferences are noticeably more conservative. The House's Democratic leanings toward domestic expenditure and the Senate's Republican inclinations toward defense are even more pronounced when representatives pay heed only to fellow partisans.

The most striking feature of Table 8, however, is the effect of partisan turnover in the House. On every dimension that does not include military spending, the mean position in the Republican House is on the opposite side of the status quo from the mean position in the Democratic House. The affinity for the Pentagon budget increases markedly in the 104th, as Table 9 summarizes, but the change is most conspicuous on domestic spending proposals. The GOP takeover moves the House mean .500 to the right on every domestic spending dimension—one-fourth of the total possible distance (see Drew 1996). In the GOP House, the only winning budget proposals cut defense to reduce the deficit and to lower taxes; the latter passes by the smallest of margins.

American institutions, then, do have consequences for federal budget making, although the consequences

[22] Fenno (1966, 581) notes that the overrepresentation of westerners in the Senate made the Department of the Interior a particular favorite of the Senate Appropriations Committee, the opposite of the situation in the House.

[23] Once again, this finding seems consistent with governmental lore dating back to Madison. But a key mechanism of balance in this model—the overweighting of votes in small states—is very different from what Madison had in mind.

TABLE 8. Institutional Representation of Public Preferences: Partisan Representation

	Mean of Rational Binary Preferences Amended Once					
	103d Congress, 1993–94			104th Congress, 1995–96		
Tradeoff Dimension	President (Democratic) (S.E.) (Weighted N/N)[a]	House (Democratic) (S.E.) (Weighted N/N)[a]	Senate (Democratic) (S.E.) (Weighted N/N)[a]	President (Democratic) (S.E.) (Weighted N/N)[a]	House (Republican) (S.E.) (Weighted N/N)[a]	Senate (Republican) (S.E.) (Weighted N/N)[a]
Lower deficits versus lower taxes	.013 (.046) (200.0/201)	.020 (.055) (139.0/140)	−.083 (.051) (194.3/269)	.013 (.046) (200.0/201)	−.095 (.055) (124.5/126)	−.119 (.050) (196.8/275)
Lower deficits versus greater domestic spending	.324 (.044) (205.5/207)	.370 (.066) (144.0/145)	.199 (.053) (205.5/278)	.324 (.044) (205.5/207)	−.177 (.069) (126.0/128)	.143 (.051) (202.0/284)
Lower deficits versus greater defense spending	−.557 (.046) (208.0/210)	−.567 (.083) (144.0/145)	−.566 (.069) (205.0/283)	−.557 (.046) (208.0/210)	−.463 (.080) (131.5/134)	−.570 (.068) (210.5/293)
Lower taxes versus greater domestic spending	.358 (.044) (206.5/209)	.392 (.069) (142.0/143)	.245 (.056) (198.8/277)	.358 (.044) (206.5/209)	−.102 (.069) (127.0/131)	.200 (.054) (201.3/285)
Lower taxes versus greater defense spending	−.602 (.043) (205.0/206)	−.634 (.087) (141.0/142)	−.468 (.067) (200.3/277)	−.602 (.043) (205.0/206)	−.397 (.078) (130.5/134)	−.453 (.065) (205.8/287)
Greater domestic spending versus greater defense spending	−.733 (.035) (211.0/212)	−.771 (.082) (146.0/147)	−.617 (.069) (209.3/287)	−.733 (.035) (211.0/212)	−.272 (.074) (135.5/137)	−.586 (.067) (214.8/298)

[a]For rational binary preferences as amended, weighted *N* is the total number of full and fractional votes included in the calculation (rounded to the nearest 0.1); *N* is the total number of cases with full or fractional votes included in the calculation. See the Appendix, "Institutional Aggregation of Preferences."

are much more pronounced when the system of separated power joins with partisan representation. Small constituencies and ideologically homogeneous parties together with a parliamentary method of preference aggregation make the House preferences more extreme than those of the Senate or the president. Not just constitutional prerogatives and partisan balances but also the divergent patterns of preference in small and in large states cause the Senate to champion defense expenditure and resist domestic expenditure. Thus, even these "thin" models of multicameral institutions, even these models of preference aggregation without representation, without elections, without deliberation, and without debate, even these barebones representations of American national legislatures reproduce many of the recent institutional divisions over the budget.

But do the institutions matter? They do, I have shown, affect the distributions of preferences that are aggregated therein, sometimes quite significantly. Yet, are the effects on distributions great enough to affect outcomes, when there is no middle ground between a "yes" and a "no"? They do, but only under some

TABLE 9. The Effect of Partisan Change on Representation: The Republican 104th Congress Compared to the Democratic 103d Congress

	Difference in Mean of Rational Binary Preferences Amended Once		
Tradeoff Dimension	House Downsian	House Partisan	Senate Partisan
Lower deficits versus lower taxes	−.048	−.115	−.035
Lower deficits versus greater domestic spending	−.132	−.548	−.056
Lower deficits versus greater defense spending	−.004	.105	−.005
Lower taxes versus greater domestic spending	−.136	−.494	−.045
Lower taxes versus greater defense spending	.088	.236	.015
Greater domestic spending versus greater defense spending	.115	.499	.031

Source: Tables 7 and 8.

Note: Each entry is the mean policy position for the 104th Congress minus the mean policy position for the 103d Congress. For example, from Table 7, which shows positions under Downsian representation, the House mean on the deficit-tax tradeoff is −.084 for the 104th Congress and −.036 for the 103d Congress; the difference is −.048. From Table 8, which shows positions under partisan representation, the Senate mean on the domestic-defense dimension is −.586 for the 104th Congress and −.617 for the 103d Congress; the difference is .031.

precise conditions. The institutions by themselves, even with their different methods of aggregating preferences, alter distributions of legislative opinion in significant ways, but little more than that. When representation is Downsian, sequential majority voting subject to the same rules as before yields the same outcome in each of the three legislative institutions—a domestic spending increase with a defense cutback—in both the 103d and the 104th Congress.[24] When representation is partisan, however, multicameralism and divided control combine to produce gridlock. In the Democratic 103d Congress, the combination of higher social spending and lower defense spending is again the consensus choice among the three legislative actors. In the Republican 104th, however, the House choice—defense cuts to fund deficit reduction—differs from the Senate and presidential choice—defense cuts to fund social spending. The latter proposal could not even pass the GOP House. But at the final stage of the legislative process, the constitutional requirement is unanimity. The president, the House, and the Senate must agree on a budget. Thus, multicameralism, divided government, and partisanship together produce gridlock (cf. Jones, True, and Baumgartner 1997). Without partisanship, separated institutions find agreement, even under divided control, at least with these mass preferences. With partisanship, they do not.[25]

These conclusions, of course, depend upon the particular distribution of budget preferences in 1995, and the conflicts among American legislative institutions might be greater or smaller with other arrays of mass opinion. An examination of the differences across the six tradeoff dimensions, however, turns up some interesting clues about the ways in which preferences and institutions interact to produce outcomes. As a baseline, first consider some similarities. The differences among legislative actors have been the focus of the discussion so far, but the parallels are at least as remarkable. An important cause of the similarity is the homogeneity of voter opinion in 1995, especially on the tradeoffs that involve defense. The majorities in favor of a scaled back military are so immense that institutional aggregation rules modify them only past the range in which additional votes matter. But an important cause of the similarity is also institutional. The president, the House, and the Senate count votes differently. After all, the institutions of American government were designed with cross-purposes in mind. All the same, they were also intended to be democratic. Consequently, the president, the House, and the Senate count the votes of the *same people*, more or less, and the differences across legislative institutions are bound to be modest, often as not. In a democratic political system, the variation in the distribution of preferences across popular institutions has a limit.[26]

Even within this limit, though, preferences and institutions interact to produce differences that are sometimes greater and sometimes smaller. One major factor in the interaction is partisanship. Democrats and Republicans divide most sharply over the proportion of the nation's resources that should be devoted to domestic programs; indeed, the correlations between individual partisanship and the tradeoffs between domestic spending, on the one hand, and deficits, taxes, and defense spending, on the other, averaged $r = .39$ (Hansen 1996, 4–5; see also Citrin 1979; Eismeier 1988; Jacoby 1994; Page and Shapiro 1992, 307–11).[27] The consequences of partisan preferences for institutional outcomes are threefold. Two are obvious. First, because elected officials may place greater weight upon the opinions of fellow partisans (Lewis 1996), partisan divisions accentuate the effects of divided control of government. The Republican takeover of the 104th Congress had its greatest effects, in both houses, on the distribution of opinion on tradeoffs with domestic expenditure, as Table 9 shows. Second, because of the parliamentary method of vote aggregation in the House, partisan divisions cause House preferences to diverge more significantly from those of the president and the Senate.[28] Third, partisan divisions produce preferences that are more heterogeneous: The only dimensions on which no proposal secures a majority, Table 5 shows, are the two tradeoffs between domestic expenditure and lower deficits or taxes. Heterogeneous preferences amplify the effects of different methods of majoritarian vote aggregation because there are more votes at the margin.

Another factor in the interaction between preferences and institutions is geography. The Senate, for instance, is distinctly favorable to defense spending, partly because of partisanship: In the 103d Congress, the Senate was the most Republican of the three institutions, and the correlations between partisanship and the defense dimensions averaged $r = .21$ (only $r = .13$ leaving aside the tradeoff with domestic spending).

[24] In the Senate, however, the winning proposal is vulnerable to a filibuster that prevents a vote against the status quo.

[25] In *very* broad outline, these findings are consistent with Krehbiel's (1996) contentions that, first, gridlock is a natural consequence of separation of powers and supermajority requirements and, second, the range of policies over which gridlock occurs is wider, the stronger is partisanship.

[26] A considerable body of research following Shepsle (1979) explores the influence of other legislative rules, and particularly committees, on outcomes. At least for budgetary issues, however, I surmise that the conclusions of this study would stand even with the addition of finer institutional detail. Both earlier work and recent scholarship find that the committees with jurisdiction over the budget—Appropriations, Budget, and Ways and Means—are least likely to comprise "preference outliers." Party leadership in the House takes considerable pains to ensure that members of these committees are representative of party opinion (Cox and McCubbins 1993, chapter 8; Fenno 1973, chapters 2–4).

[27] The stated average summarizes the correlation between partisanship measured on the seven-point NES scale and opinion on tradeoffs measured on the three-point scale shown in Table 5. The individual correlations ranged from $r = .36$ for domestic spending and defense spending to $r = .42$ for domestic spending and taxes. For the tradeoffs that did not involve domestic spending, the largest correlation with partisanship was $r = .15$, for the tax-defense dimension.

[28] A corollary is that the greater the partisan homogeneity of congressional districts, the greater this divergence should become, even under Downsian representation.

But its hawkishness also is due to the relative affinity of voters in small states for the military.

Combine these points, and a picture of the interaction of preferences and institutional voting rules begins to emerge, and it sharpens in contrast to the past. As we have seen, the Democratic 103d House had a relative preference for domestic spending, and the Senate had a relative preference for defense, but the "liberality" of the House and the "conservatism" of the Senate are not eternal features, even holding partisan control constant. In the 1950s and 1960s, the Senate was considered the more "liberal" of the two chambers—it routinely approved greater expenditures than the House—and analysts attributed the difference to the more diverse pressures of the more heterogeneous Senate constituencies (Fenno 1966, chapters 10–11; Froman 1963; Manley 1970, chapter 6; but see Kernell 1973). Clearly, the results in this study (and recent experience) indicate that Senate liberality can change. In the House, the people represented by the majority Democrats a generation ago were a more conservative lot than the voters behind the majority Democrats in the 1990s: The House majority in the 1950s and 1960s represented many more rural southerners and many fewer urban northerners. In the Senate, in contrast, the residents of small states were a more liberal group forty years ago than they are now: Democratic strength has diminished in the little sagebrush states and grown in the big industrial states. Thus, as the relationship of mass opinion to partisanship and geography change, preferences and institutions interact to produce different results. The combination of voter preferences and institutional aggregation rules that produced House conservatism and Senate liberality in the 87th Congress produced Senate conservatism and House liberality in the 103d.

The key factors in the interaction between preferences and legislative institutions, then, are the correlations between preferences, on the one hand, and individual partisanship and the size of the state of residence, on the other. This should hardly be a surprise. In this model, I have defined institutions wholly as methods of preference aggregation. Moreover, I have made the rule of vote aggregation distinctive for its partisanship in the House and distinctive for its federalism in the Senate. I have done so for good reason: The House is the American institution that behaves most like a parliament, and the Senate is the American institution that represents states rather than population.

In fact, the events of 1995 suggest that the models reproduce the main outlines of conflict over the budget. In January, Republicans assumed control of the Senate for the first time in eight years and control of the House for the first time in four decades. They immediately filled the agenda with a raft of reforms, including a plan to balance the federal budget within seven years. They met staunch resistance from the president, along lines that parallel these findings. The deep conflict between Newt Gingrich's House and Bill Clinton's White House was exactly over the issues of deficit reduction and domestic spending. Bob Dole's Senate went along with the House, but it very clearly lacked the fire for the conflict, and most often it watched uncomfortably from the middle (Drew 1996). While the positions of the three institutions were often attributed to the personalities of their leaders (and the obstreperousness of backbenchers), the findings that emerge from these blunt models suggest that the conflict had real political substance. The *way* the Democratic president represented public preferences caused the White House to put greater priority on domestic spending. The *way* the GOP House represented public preferences impelled the lower chamber to place greater weight on deficit reduction. And the *way* the Senate represented public preferences made it the centrist in the budget policy debate (Brady and Volden 1997).

At the same time, the comparison of the model with events also turns up some important discrepancies. The facsimiles of American legislative institutions I have employed sometimes fail to capture noteworthy aspects of the political situation in 1995, but the miscues turn out to be useful. They suggest that other institutions besides legislative institutions put their stamp on policy outcomes, and they provide some clues about which. Consider two questions.

First, why were cuts in the Pentagon budget, which were by far the most popular budget options, "off the table" in 1995 (Drew 1996)? Certainly, there is an array of possibilities that have little to do with public preferences: the perspectives of elites on statecraft, the aversion of national leaders to the risks of underinvestment in the military, and so forth. But the data suggest other possibilities as well. One answer might be that the public's perception of the status quo lags behind changes in policy. The defense budget had borne the brunt of deficit reduction in previous rounds of budgeting, and the bipartisan consensus in Washington was that the Pentagon had already sacrificed enough. The public was probably not fully aware that the "peace dividend" had already been spent (see Bartels 1994). Had it been, lawmakers may well have reasoned, the public would have found the defense status quo closer to its ideal than it imagined ex ante. Legislative institutions not only represent voters but also act in their stead.

A second answer points to one of the limitations of single-vote methods of preference aggregation: They ignore intensity. Defense reductions are popular because most Americans would not notice the difference in their life if the government cut its military budget substantially. For the vast majority of Americans, national defense brings only diffuse collective benefit, not direct personal benefit. Employment in the armed forces and defense-related industries is only about 1.6% of the domestic work force, only slightly larger than employment in agricultural production. With no direct stake in defense spending, substantial majorities have low-intensity policy sentiments.

Because defense consumes roughly 4% of GNP, however, it produces highly concentrated benefits for the 1.6% of the work force employed in its production, and the motivational force of concentrated government

benefits has traditionally given industries influence beyond their numbers in national politics. Moreover, the defense industry is highly concentrated geographically, especially in the Southeast and Southwest. The citizens who support defense spending, then, are intense, concentrated, and allegedly advantaged by the arithmetic of the Electoral College. The biggest prize in the presidential race, California, with its 54 electoral votes, has about 12% of the defense employment in the United States (a large amount, although roughly in proportion to its population).

In addition to the two dozen visits Clinton made to the Golden State in the first term of his presidency, the data from the 1995 Pilot Study provide some glimmers of evidence for the Electoral College proposition. Weighting preferences by Electoral College representation made only the faintest difference to the distribution of preferences, on defense spending or anything else, but the "battleground" states in the 1996 election were noticeably more favorable to national defense—or, more precisely, more supportive of the defense status quo—than less competitive states.[29] In no case were the differences substantial, but they do suggest that Electoral College mathematics works modestly to the advantage of defense spending. Once again, an institutional method of preference aggregation shapes outcomes in noticeable ways.

Second, why has deficit reduction had such urgency in Washington? This question takes us a little farther. Say what one will about the fits and starts in the battle against the deficit, the truth is that the officers of national government have fought harder than the public preferences revealed here would seem to justify (White and Wildavsky 1989). Although the public would crop defense to balance the budget, Table 5 showed, only about one-third of respondents were willing to sacrifice domestic spending or taxes at the altar of deficit reduction.

One possibility is that the public simply does not understand the implications of the deficit in the same way that it understands spending cuts and tax hikes, and the Burkean trustees in Congress and the White House have intervened for the public's own good. There is nothing in these data to rule out the possibility, but it does entail a more generous view of politicians than Americans have recently embraced.[30]

The data do suggest a second possibility, namely, that the institutions of national government have amplified the preferences of Republicans and their congressional representatives. Under Democratic control in the 103d Congress, tables 7 and 8 showed, the Senate, the most Republican institution of the three, was the most hawkish on the deficit in opposition to tax cuts. Under Republican control in the 104th Congress, likewise, the GOP House moved so far to the right on domestic spending that deficit reduction was its preferred use for the savings from cuts in the military.

The Republican aversion to deficit spending has its roots as deeply in the interests of its partisans as does the Democratic preference for domestic spending, and this suggests yet a third possibility. Historically and currently, opposition to deficit spending is greatest among the advantaged, who are less reliant on public provision of goods and services and more invested in the financial assets most threatened by inflation (Hansen 1997; White and Wildavsky 1989, chapter 18). They are also the part of society best able to make themselves heard in democratic politics. As is well known, political participation in America has a class bias upward, a bias that becomes more substantial as political action becomes more organized, more costly, and (presumably) more effective (Rosenstone and Hansen 1993, chapter 8; Schattschneider [1960] 1975; Verba, Schlozman, and Brady 1995, chapter 16). Politicians balance the demands of remote and ill-informed constituents against the preferences of proximate and well-versed elites in the corridors of government, academia, the media, and K Street (White and Wildavsky 1989). In many instances, the advantaged need not even act to influence: Public budget makers rarely stir without first taking the pulse of the financial markets (see Lindblom 1977, Part V). Effectively, then, the preferences of the part of the public that opposes deficit finance are overrepresented in the budget process, in particular, overweighted relative to the egalitarian aggregation formulas I modeled in this analysis.

Table 10 supports the point. It shows the mean policy position when preferences are weighted equally, by schooling, and by dollars. (See the Appendix for details.) They tell a consistent story. When one year of education counts as one vote, the support for domestic spending diminishes, the sensitivity to taxes increases, and the pressure for deficit reduction builds. When one dollar of income counts as one vote, the support for social spending diminishes still further, the sensitivity to taxes increases still more, and the pressure for deficit reduction builds still higher. The twenty-five-year war on the deficit may well bespeak that some preferences in America are weighted more equally than others.

CONCLUSION

For years, the difficulty in bringing the government budget under control has sullied the reputation of democracy. Democratic citizens, say the critics, are simply incompetent; republican institutions, say the critics, are simply incapacitated. As the analysis in this article indicates, however, the diagnosis is not so straightforward. Citizens' preferences over the public budget are remarkably well structured. Most people have no difficulty determining their views. Most have no difficulty recognizing tradeoffs. And most have no difficulty maintaining consistency. The preferences of ordinary Americans are strikingly coherent. American democracy does not want for the competence of its citizens.

The capacities of American institutions are a differ-

[29] I defined "battleground" in two ways: the states with the smallest Bush or Clinton plurality in 1992, and the states carried by Bush in 1988 but by Clinton in 1992. The conclusion is slightly stronger with the latter definition.

[30] In all fairness to the public, it is not clear that policy experts have appreciably greater understanding of the economic implications of the deficit, although the experts may have different priors.

TABLE 10. The Effect of Weighting Public Preferences by Education and Income

	Mean of Rational Binary Preferences Amended Once		
Tradeoff Dimension	One Person, One Vote (S.E.) (Weighted *N*/*N*)[a]	One Year of Schooling, One Vote (S.E.) (Weighted *N*/*N*)[a]	One Dollar, One Vote (S.E.) (Weighted *N*/*N*)[a]
Lower deficits versus lower taxes	−.059 (.031) (416.5/421)	−.087 (.031) (414.0/419)	−.104 (.032) (386.0/401)
Lower deficits versus greater domestic spending	.113 (.035) (427.5/434)	.063 (.035) (425.0/430)	.007 (.035) (398.5/403)
Lower deficits versus greater defense spending	−.489 (.033) (435.0/442)	−.516 (.047) (433.5/441)	−.546 (.047) (404.5/412)
Lower taxes versus greater domestic spending	.176 (.035) (426.0/437)	.142 (.036) (420.5/430)	.103 (.036) (393.5/402)
Lower taxes versus greater defense spending	−.493 (.030) (429.5/434)	−.503 (.046) (426.5/433)	−.517 (.047) (398.0/404)
Greater domestic spending versus greater defense spending	−.525 (.032) (445.5/450)	−.515 (.044) (442.5/447)	−.485 (.045) (413.5/418)

[a]For rational binary preferences as amended, weighted *N* is the total number of full and fractional votes included in the calculation (rounded to the nearest 0.1); *N* is the total number of cases with full or fractional votes included in the calculation. See the Appendix, "Institutional Aggregation of Preferences."

ent matter. As we have seen, the institutional composition of the three legislative actors in American government has put a distinctive stamp on the policy choices of American government. The plebiscitary president, the parliamentary House, and the federal Senate count the same votes, but differently. Depending on the votes they count and the ways they count them, republican institutions are united despite their differences—in institutional structure—or divided despite their similarities—in constituency content. At times, when representation is partisan and government is divided, multicameralism produces conflict; more often, it produces accord. It is important that separated legislative institutions represent the same people *differently*, but it is equally important—if not more important—that they represent the *same people*.

Accordingly, the problem in public budgeting, if there is a problem, lies not in the rationality of public preferences and not in the method of their representation in public institutions. It lies in their content. After all, a major factor in the public's consistency, and in the performance of legislative institutions, is the public's partiality for the policy status quo. From one perspective, the public's affinity for current policy indicates that all is as it should be. Satisfaction with the status quo ensues from a policy process that responds to public preferences (Bartels 1991; Hartley and Russett 1992; Page and Shapiro 1983, 1992; Stimson, MacKuen, and Erikson 1995; Wlezien 1995). People like what they see because they have gotten what they wanted.[31]

From another perspective, however, the public knows what it wants but is *substantively* mistaken. Its errors are varied: The public underestimates the hazard to the future of financing current consumption with borrowed money; it underappreciates the importance of investment in the physical and economic well-being of its children; it misjudges the power of the economic forces unleashed when the government extracts less from the private economy; and so forth. As much as the democratic faith counsels that the people know best what is best for the people, critics of recent American fiscal practice cannot but conclude that ordinary Americans just have it all wrong.

But current fiscal policy may be popular not necessarily because it is better for the public. It may be popular because people are too conflicted over the values inherent in budgeting to choose one over others

[31] Among the normative advantages of democratic responsiveness that have not been noted are two suggested by this study. First, responsive policy processes reduce the cognitive demands of judging policy. They produce greater satisfaction with existing policy, and individuals who are satisfied face fewer difficulties in ordering their preferences than do individuals who are not. People who like the status quo in most of its elements have fewer objectives to reconcile. Second, and thereby, responsive policy processes reduce the possibilities for institutional conflict. They promote convergence on incremental policy change.

(Feldman and Zaller 1992; see also Alvarez and Brehm 1995, 1997). People may support the status quo because they apprehend the tradeoffs all too well.

Alternatively, current policy may be popular just because it is familiar. People know what their life is like under the current fiscal regime, but they can only guess what it would be like with lower taxes, higher spending, or smaller deficits. The public may reject superior options out of aversion to risk, as it does in elections (see, among others, Alvarez and Franklin 1994; Bartels 1986, 1988, chapter 4; Brady and Ansolabehere 1989; Quattrone and Tversky 1989).[32] People may reject superior options because information about alternatives is costly (Samuelson and Zeckhauser 1988, 33–5). Or they may reject superior options because they cannot identify themselves as winners or losers ex ante (Fernandez and Rodrik 1991). Either way, the safety of the status quo and the insecurity of innovation bias people against change. In choices among spending, taxes, and deficits, the devil they know is better than the devil they do not know.

The hope of democratic politics, however, is the possibility that the people may like a new state of affairs as well or better—once they are there.[33] The preferences of the citizens or the procedures of the institutions may hasten or impede, but this is where the capacity of democratic leadership is tested. In a democracy, it is up to those who are convinced that the public is wrong to show the public a better way.

APPENDIX: DATA AND CODING

The data for this study come primarily from the American National Election Study 1995 Pilot Study, conducted by the Center for Political Studies, Institute for Social Research, University of Michigan (Rosenstone et al. 1995). The pilot sample was drawn randomly from respondents to the 1994 NES who had telephones. Of the 704 respondents selected for reinterview, 486 participated. They were interviewed by telephone between August 3 and September 10, 1995. The 1994 sample was selected randomly from the Survey Research Center's national area probability sample.

The twelve budget policy questions and their marginals were presented in Table 1. In administration, they were divided into four units of three. The first set grouped proposals for deficit reduction, the second proposals for tax reduction, the third proposals for domestic spending increases, and the fourth proposals for defense increases. The deficit and tax batteries appeared in close proximity, separated by four questions (plus follow-ups); likewise, the domestic and defense spending batteries appeared nearby, separated by two questions (plus follow-ups). The two pairs of batteries, deficit/tax and domestic/defense, were separated by 60–90 questions, or about 15–20 minutes. (To accommodate split-sample experiments, the Pilot Study instrument was administered in two forms.)

The most difficult issue in the construction of the questions was the wording of the "domestic programs" prompt. We believed we needed a concrete example, and "Medicare, education, and highways" steers a middle course between representativeness and the potential for reactivity. Education and highways, together with Social Security and health—both closely related to Medicare—are the four largest components of the domestic budget. We felt that "Medicare" was less likely to provoke "hot button" responses than "Social Security." For more discussion of issues of wording, see Hansen (1995). See also Smith (1987).

For evidence on validation of these measures, see Hansen (1996).

Individual Preference, Indifference, Consistency, and Transitivity

As noted in the text, I examined choices in tradeoff pairs—each policy proposal and its opposite—for strict preference, "indifference," and consistency. The coding may be summarized as follows:

- accepted the policy "superior x, inferior y" and rejected "inferior x, superior y": strict preference for x over y;
- rejected the policy "superior x, inferior y" and rejected "inferior x, superior y": strict preference for the status quo policy and (provisionally) indifference between x and y;
- accepted the policy "superior x, inferior y" and accepted "inferior x, superior y": inconsistency in preference between x and y.

Taking a pair of rejections to constitute indifference, I compared the pattern of choices over the six pairwise tradeoffs to the 75 patterns of transitive preference orders over the four objects to identify instances of transitivity and intransitivity as well as their nature. For elaboration of the underlying choice theory and a discussion of its assumptions, in particular the assumed convexity of utility functions, see Hansen (1995) and Ordeshook (1986, 33–7).

For intransitive, inconsistent, and incomplete orderings, I made first one amendment and then two, comparing the amended orderings to the 75 transitive orderings. Each amendment altered a single answer to a single question, intervening either to change a "yes" to a "no," or vice versa, or a nonresponse to a "yes" or to a "no." Single amendments frequently transformed an intransitive, inconsistent, or incomplete ordering into more than one transitive ordering. One respondent, for example, revealed preference for deficit reduction over all other options, indifference between tax cuts and domestic spending and between domestic spending and defense spending, and strict preference for tax cuts over defense spending. By his answers, he revealed a preference ordering that violates indifference transitivity. But by changing just one survey answer, he could have revealed three different transitive preference orderings. (1) By accepting rather than rejecting lower domestic spending in order to cut taxes, he could have shown a preference for deficit reduction over tax reduction over spending, but indifference between domestic and defense spending. (2) By accepting rather than rejecting smaller defense spending in order to increase domestic outlays, he could have revealed a first preference for deficit reduction, a last preference for defense spending, and indifference between tax cuts and domestic spending. (3) By rejecting rather than accepting lower military spending in order to cut taxes, he could have shown a first preference for deficit reduction and indifference among the three less preferred options. In every analysis that employs "amended" preferences, I have accordingly weighted choices and orderings by the inverse of the number of transitive orderings that are one (or two) amendments away from the answers given. In this example, each choice or ordering was weighted by one-third. Where I have made two amendments to the

[32] Fishburn (1970) draws an analytical link between risky choice and apparent indifference intransitivities, which were also prevalent in these data.

[33] Or maybe not; see Fernandez and Rodrik 1991.

answers given, I have ruled out changes that reverse expressed preferences. I do not change a "yes" and a "no" on a tradeoff pair to a "no" and a "yes," and vice versa. Likewise, I do not change an inconsistent response on a tradeoff pair ("yes" and "yes") to an indifference response ("no" and "no").

Institutional Aggregation of Preferences

As noted in the text, institutions are defined as rules for aggregation of individual preferences, and the respondents are the voters whose preferences are subject to the rules. The rules outlined in Table 6 influence outcomes either by excluding subsets of voters or by differentially weighting the preferences of voters. In each case, the valid cases are respondents with "rational binary preferences" over the two options in the tradeoff.

Partisan Representation. Respondents were classified as Democrats, Republicans, or independents from the (standard) partisan identification question, asked in 1995:

> Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what? [If Republican (Democrat):] Would you call yourself a strong Republican (Democrat) or a not very strong Republican (Democrat)? [If Independent:] Do you think of yourself as closer to the Republican Party or to the Democratic Party?

Coding: Democrats (Republicans) include all who professed Democratic (Republican) identifications in the first question and all who professed independence in the first question but Democratic (Republican) proclivities in the follow-up. The inclusion of independent "leaners" in partisan categories maintained the size of subsamples (just 27 of 486 respondents were "pure" independents) and accords with earlier findings about the political behavior of leaners (Keith et al. 1992).

Majority or President. For each individual i, the vote is

$$\sum_{t=1}^{T_i} \frac{p_{it}s_i}{T_i},$$

where

p_{it} = i's rational binary preference in the tth amended preference ordering, coded -1 if the preference is for the first option named, 1 if for the second, and 0 if for the status quo;

T_i = number of rational binary preferences that are zero, one, or two amendments away from i's preferences as expressed (for zero amendments, $T_i = 1$; for one amendment, $T_i \in \{1,2,3\}$; for two amendments, $T_i \in \{1, \ldots, 6\}$); and

s_i = 1 if representation is partisan and i shares the president's partisanship; 0 otherwise.

House of Representatives. The 1995 sample includes respondents from 138 of the 435 congressional districts, with frequencies that range from 1 to 15 per district (median = 3). Because of the tiny size of the congressional district subsamples, and because the district subsamples are not self-representing, I did not aggregate opinion by districts; instead, I aggregated it nationally according to the rules specified in Table 6. Accordingly, for each individual i, the vote is

$$\sum_{t=1}^{T_i} \frac{p_{it}m_i s_i}{T_i},$$

where p_{it} and T_i are defined as before, and

m_i = 1 if i's representative is a member of the House majority party, 0 otherwise; and

s_i = 0 if representation is partisan and i does not share her representative's partisanship; 1 otherwise.

The national aggregation works well. In the 103d Congress, Democrats held 60% of the seats; by my measure, 61% of the sample was represented by a Democrat in the 103d. In the 104th Congress, Republicans held 54% of the seats; by my measure, 56% of the sample was represented by a Republican in the 104th.

Senate. The sample includes respondents from 33 of the 50 states, with frequencies that range from 3–47 (median = 12). Again, because of small size and statistical unrepresentativeness of state subsamples, I aggregated nationally using the rules specified in Table 6. For each individual i, the vote is

$$\sum_{t=1}^{T_i} \frac{p_{it}s_i}{v_i T_i d_i},$$

where p_{it}, s_i, and T_i are defined as before, and

v_i = voting age population in i's state, from *Statistical Abstract of the United States 1996*, Table 459; and

d_i = 2 if representation is partisan and i's Senate delegation includes one senator from each party, 1 otherwise.

For this aggregation, the NES sampling frame happens to have a modest Republican bias, which worsens in the 104th Congress. In the 103d Congress, 34% of the voting age population was represented by a GOP senator; by my measure, 39% of the sample was so represented (counting representation by two Republican senators as 1 and representation by one GOP senator as ½). In the 104th Congress, 48% of the voters were represented by a GOP senator; by my measure, 60%.

Education and Income. Education and income were determined by standard questions asked in 1994. For education, the question read: "What is the highest grade of school or year of college you have completed?" Coding: actual years of education, ranging from 0 to 17, with 17 indicating postbaccalaureate education.

For income, the question read:

> Please look at this page and tell me the letter of the income group that includes the income of all members of your family living here in 1993 before taxes. This figure should include salaries, wages, pensions, dividends, interest, and all other income.

Coding: each category was assigned its midpoint in thousands of dollars, with the maximum assigned 112.

For each individual i, the vote is

$$\sum_{t=1}^{T_i} \frac{p_{it}c_i}{T_i},$$

where p_{it} and T_i are defined as before, and

c_i = years of education or thousands of dollars of income.

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A New Approach to the Study of Ticket Splitting

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A new solution to the ecological inference problem is used to examine split-ticket voting patterns across states and congressional districts in the 1988 elections. Earlier studies of ticket splitting used either aggregate data, which suffer from the "ecological fallacy" and threaten individual-level inferences, or survey data from small, unrepresentative samples. We produce more accurate estimates of the proportions of voters splitting their ballots in each state and district, which enables us to examine variations across geographical units. We also clarify the connection between ticket splitting and divided government and test several competing theories about the causes of both. We find, contrary to balancing arguments, that voters are not intentionally splitting tickets to produce divided government and moderate policies. In most cases split outcomes are a by-product of lopsided congressional campaigns that feature well-funded, high-quality candidates versus unknown competitors.

The 1990s have witnessed a flurry of research on split-ticket voting and divided government in the United States (Alesina and Rosenthal 1995; Alvarez and Schousen 1993; Beck et al. 1992; Born 1994; Cox and Kernell 1991; Fiorina 1996; Frymer, Kim, and Bimes 1997; Galderisi 1996; Jacobson 1990). Both are more common features of the political landscape today than in previous periods of American history and thus warrant this scholarly attention. Fifteen of the last 22 national elections have produced divided government, and surveys show that split-ticket voting rates are higher than in previous decades. There is little consensus on what causes ticket splitting. Some researchers argue that it is done intentionally in an attempt to produce the proper mix of policies or keep the political parties in check (Alesina and Rosenthal 1995; Fiorina 1996; Ingberman and Villani 1993). Others believe it is the unintentional by-product of other factors, such as the competitiveness of congressional elections (Jacobson 1997; Kimball 1997), the blurring of ideological boundaries between the parties (Born 1994; Kimball 1997; Soss and Canon 1995), and ballot mechanisms (Beck 1997; Campbell and Miller 1957). Whatever the eventual answer, it is clear that scholars need to rethink voting behavior to produce a theory that explains ticket splitting (Sundquist 1988). Indeed, Fiorina (1996, 129) claims in his definitive *Divided Government* that "ticket-splitting and other aspects of the relationships between voting for different offices should become a central focus of research."

We take up Fiorina's charge by using a new statistical method (King 1997) to produce more accurate estimates of the direction and magnitude of split-ticket voting in each state and congressional district in 1988. We apply the method to presidential and congressional voting behavior by estimating president/House and president/Senate ticket splitting. The estimates are then used in a series of analyses to compare our ecological estimates to other estimates and to explain why ticket splitting is so common in some places and nonexistent in others. The 1988 elections are an ideal context in which to study split-ticket voting. Unlike earlier periods in American electoral history, which produced divided government during midterm elections, the 1980s witnessed split outcomes in presidential years as well. In fact, it was the simultaneous election of George Bush with the maintenance of the Democratic status quo in Congress in 1988 that interested many researchers in split-ticket voting (Fiorina 1996; Jacobson 1990).

Though this case is theoretically appropriate, inferences about the direction and magnitude of ticket splitting have been hindered because of data limitations. Aggregate measures have been used in the literature for several decades but are flawed by the well-known "ecological fallacy," which refers to the hazards of making individual-level inferences from aggregate data. Survey data also have been used, but national samples are not large enough to make inferences about voting behavior in particular districts. We employ King's technique to produce more accurate estimates of split-ticket voting within districts and states. Unlike earlier studies based on just a few data points (indexed by time), our data set contains hundreds of observations (indexed by district), making it easier to generalize. We demonstrate the validity of these estimates and then combine them with other data to determine what causes American voters to split tickets. We find strong evidence for the contention that candidates—not voters—are primarily responsible for producing divided government. Most variation in the

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A longer version of this paper was presented at the 1997 meeting of the Midwest Political Science Association, Chicago, IL. The authors thank Laura Arnold, Larry Baum, Neal Beck, Janet Box-Steffensmeier, Aage Clausen, Morris Fiorina, Paul Frymer, Gary King, Dean Lacy, Herb Weisberg, and Chris Zorn for their comments. We are also grateful to Peter Radcliffe for helping us access FEC data, Jeff Milyo for challenger quality data, and Keith Poole for NOMINATE estimates. The authors contributed equally and are listed alphabetically; correspondence should be directed to the first author. The computer program used in this article is publicly available on the Internet at http://gking.harvard.edu/stats.shtml/. Data used in the analysis may be obtained from the ICPSR publication-related archive (http://www.icpsr.umich.edu/cgi/archive.prl).

extent of ticket splitting across districts and states is not due to policy balancing on the part of voters.

It is worth studying ticket splitting at the district and state level because election outcomes are determined within these electoral units (Wright 1989). Voters within these geographical boundaries, not the national population, determine the composition of Congress and the Electoral College. Our approach allows us to link ticket splitting directly with divided government because we can compare levels of ticket splitting to the election outcome in each state or district.

USING ECOLOGICAL INFERENCE TO STUDY TICKET SPLITTING

Given the ubiquity of divided government in American national politics, it is important to understand how many voters split their ballots and why. Accurate estimates will help us understand the extent to which divided government is the will of the voters. Why voters split tickets is a continuing research question in studies of electoral behavior. Unfortunately, the data used to answer the question have not been up to the task. Here, we consider the two traditional approaches—using individual survey and aggregate election data—and demonstrate why the new method is an improvement.

In addition to King's method, several other techniques developed in political science, sociology, statistics, and other fields may be helpful in drawing individual inferences from aggregate data. Perhaps the earliest is the "method of bounds" (Duncan and Davis 1953), which uses a deterministic relationship between aggregate quantities to narrow the range of possible values of the individual-level parameters of interest. Some have applied that method while adding external assumptions or substantive knowledge to narrow even more the logical bounds around the individual-level parameters (Achen and Shively 1995; Shively 1991; Sigelman 1991). The most commonly used ecological inference method is probably Goodman's (1959) regression technique. Though Goodman warned against its use, one may estimate individual-level parameters though simple linear regression, assuming that the parameters do not vary across districts (or vary randomly across districts around their global values).

Approaches to the ecological inference problem have become remarkably complex since that time. Achen and Shively (1995) survey the latest techniques, many of which allow for some relaxation of Goodman's constancy assumption or incorporate other substantive assumptions about the individual-level parameters and their relationships with aggregate-level variables. Most of these techniques estimate only one global set of individual-level parameters, however, not a pair for each electoral district, which is essential for our purposes. King's method provides district-level estimates of the quantities of interest and the uncertainty associated with them without making unreasonable assumptions about the constancy of the parameters we are estimating. It also permits us to work *between* the individual and aggregate levels of analysis, which helps us link ticket splitting to split-party election outcomes. In addition, while others have noted a connection between the method of bounds and Goodman's regression (Achen and Shively 1995), King's approach may be the first to combine the insights of both earlier methods.

Individual Data

As with other voting studies, analyses of ticket splitting rely primarily on survey data. The National Election Study (NES), for instance, routinely asks a sample of Americans how they voted in the most recent elections. These data have been informative for examining the national trend in ticket splitting over the last 45 years and some of its determinants. A serious problem is that one cannot decompose national survey samples into smaller units of aggregation, such as states or voting districts, because of the number of cases required for such an analysis (Voss, Gelman, and King 1995). The NES interviews about 2,000 people each election year; so relatively few respondents come from each state, and even fewer are drawn from any given House district. This makes estimates inefficient, if they are possible at all. Furthermore, a representative national sample usually is not representative of voters from each state or congressional district (Westlye 1991).[1]

Compounding the problem is the potential for inaccurate reports of voting behavior in surveys (Abramson and Claggett 1986; Campbell et al. 1960; Silver, Anderson, and Abramson 1986; Wright 1993). Proportions based on survey data are only unbiased if respondents report their voting behavior accurately.[2] For example, researchers have pointed out that the NES overstates support for the winning (Wright 1990) and incumbent (Eubank and Gow 1983; Gow and Eubank 1984) congressional candidates.[3] This is due either to forgetfulness over time, social desirability pressures, or a question wording artifact (Box-Steffensmeier and Jacobson 1995; Jacobson and Rivers 1993; Wright 1990). Others argue that surveys tend to overstate voter turnout (Abramson and Claggett 1986; Campbell et al. 1960; Silver, Anderson, and Abramson 1986). Thus, a study of voting data at an aggregated level is a worthwhile supplement to self-reported voting patterns derived from surveys.

[1] Some may advocate the use of exit polls to estimate ticket splitting within each state and district, but these usually do not include data to parcel respondents into congressional districts, and until recently many exit polls did not ask people to report their House vote. In any event, the representativeness of respondents in exit polls is frequently in question (Asher 1995).

[2] Technically, estimates may be unbiased if reporting errors are random. Systematic misreporting in a particular direction induces bias.

[3] To the extent that incumbency contributes to ticket splitting (Alvarez and Schousen 1993; Born 1994), a proincumbency bias means that the NES may overestimate the extent of split-ticket voting in national elections in the 1980s (Wright 1990).

TABLE 1. Voting in Maryland's Sixth Congressional District, 1988

Presidential Voting Decision	House Voting Decision			Total
	Byron (D)	Halsey (R)	Abstain	
Dukakis (D)		*Dukakis splitters*		82,781
Bush (R)	*Bush splitters*			158,808
Total	166,753	54,528	20,308	241,589

Aggregate Data

Aggregate election data also have been used to examine split-ticket voting. Burnham's (1965, 1970) comprehensive analysis of American political realignments cites ticket splitting as a sign of large-scale political change. His operationalization of split-ticket voting is the simple difference between the highest and lowest Democratic percentage of the two-party vote among races within each state. Burnham admits that his indicator is "measured rather crudely," although it was one of the best available at the time. As is well known, the problem with such a measure is that making individual inferences from aggregate data may lead to false conclusions. Furthermore, Burnham's method can only estimate total ticket splitting, not its direction; the method cannot distinguish those who select a Republican president and a Democratic congressional candidate from those who choose a Democratic president and a Republican for Congress.

Rusk (1970) uses the same aggregate measure while pointing out two of its flaws. First, "it would not detect mutual crossovers between the two parties" (p. 1224); second, differences in the type of election being held in the states the same year threaten interstate comparisons, a problem Rusk calls the "race composition factor." The latter is a concern, but consider the more immediate ramifications of mutual crossover. Even if two states have the same contests on the ballot, Burnham's measure understates ticket splitting, since ballots split in opposite directions cancel out one another. For instance, a value of zero may mean that no split-ticket voting occurred (as Burnham and Rusk assume) or that every voter is casting a split ticket. The truth is likely to lie somewhere in between. Much may be happening at the individual level, but aggregate data cannot reveal it, a consequence of the ecological fallacy that has plagued social science for decades (King 1997; Robinson 1950).

Another aggregate technique relies on the two-office split result, which assumes that a difference in party outcomes for two offices in the same district is evidence of widespread ticket splitting. The increasing proportion of congressional districts with split results in presidential and House contests is used to infer that ticket splitting has increased over time (Cummings 1966; Fiorina 1996). But Feigert (1979) shows that this technique can produce inaccurate estimates and instead finds a high correlation between split districts and ballot roll-off.[4] Below, we show that once the direction of ticket splitting is properly estimated, there is a conditional relationship between split-ticket voting and split outcomes.

With the development of a solution to the ecological inference problem, we can produce district- and state-level estimates of the proportion of voters casting split ballots that avoid the problems described above. Since we use aggregate data, survey misreporting is not a concern; only collective (and anonymous) votes are needed. In addition, King's method allows us to account for both types of ticket splitting, thus avoiding the crossover problem in previous aggregate measures. Before turning to the estimation itself, we briefly describe the ecological inference problem.

The Problem as a 2 × 3 Contingency Table

The general ecological inference problem may be conceptualized as a standard contingency table with missing data. The marginals of the table are known, since they are based on aggregate data, but the cell percentages are unknown (King 1997; Markus 1974). Table 1 lays out the ecological inference problem in estimating ticket splitting, using 1988 congressional election results from Maryland's Sixth District as an example. We choose this district for illustration because it produced a split-party result that matched the national pattern: a Republican (George Bush) at the presidential level and a Democrat (Beverly Byron) at the congressional level. Since the presidential contest usually appears at the top of the ballot and is the more visible race, we assume that voters make their presidential choice first. Voters then have three options in the congressional races that appear farther down the ballot: (1) vote Democratic, (2) vote Republican, or (3) abstain.[5] From election returns we know how many voters in Maryland's Sixth District chose Bush in the presidential contest (158,808), and we know how many voters selected Byron, the Democratic candidate for the House (166,753), but we do not know how many of the Bush voters split their ballot by also selecting Byron. Presumably, this was common, since each candidate carried the district by a hefty margin. We do know that the number of Bush voters who split their ballot ("Bush splitters") is between 83,972 (the Byron

[4] Feigert (1979) actually recommended the use of ecological inference techniques in further study of ticket splitting but lamented that valid methods did not exist at the time. While ecological inference techniques have been used to examine the extent to which voters switch parties from one election to the next (Achen and Shively 1995; Gosnell 1937), to our knowledge no one has used these techniques to study ticket splitting.

[5] We ignore votes for third-party candidates. Few voters chose such candidates in 1988.

total minus the Dukakis total, or .528 of all Bush voters in the district) and 158,808 (all Bush voters), a wide interval. Similarly, we have reason to believe that slightly more than 20,000 presidential voters "rolled off" by not casting a vote in the House race, but we do not know whether abstention in the House contest was more prevalent among Bush or Dukakis voters.

Our estimation of ticket splitting is a two-stage process that divides Table 1 into two 2 × 2 tables that are analyzed sequentially for every district. First, we estimate the fraction of Dukakis and Bush voters who also cast a vote in the House contest (or the proportions that do not fall in the "Abstain" column). Those estimates are then combined with the other known aggregate information to estimate ticket splitting ("Bush splitters" and "Dukakis splitters").[6] It is important to control first for ballot roll-off in the congressional contest, because voters who abstain from that race cannot split their ballot. In addition, if there are partisan differences in roll-off, then aggregate measures of ticket splitting that fail to account for roll-off are biased.[7]

When discussing our estimates of split-ticket voting, we will use a two-letter abbreviation to indicate the four possible voting patterns; the first letter is the party of the presidential candidate chosen, and the second letter is the party of the congressional candidate chosen. For instance, an RD voter is one who voted for Bush and the Democratic congressional candidate.

DATA AND METHOD

The main data set consists of only the number of votes received by the presidential, House, and Senate candidates in each congressional district or state (from Barone and Ujifusa 1989) to provide the known quantities in Table 1.[8] Voters in 1988 could have split their ballot in one of two ways: Bush and a congressional Democrat, or Dukakis and a congressional Republican. "Bush splitters" denotes the former, and "Dukakis splitters" denotes the latter. Ultimately, we are more interested in Bush splitters because of the divided government pattern of the 1980s.[9]

The first run included only presidential and House data, with 74 uncontested House races excluded from the procedure. In the uncontested districts, voters only had one choice for the House; the exclusions do not affect the estimates for the other districts. In addition, a South dummy variable was used as a covariate to account for possible aggregation bias and to improve the estimates. Because the degree of split-level partisanship in the South is unique, we suspect that aggregating votes may conceal more ticket splitting in the South than elsewhere. The second run produced estimates of president/Senate ticket splitting within states. Since there were only 33 Senate races in 1988, estimates based on so few observations would be inefficient. Thus, the House data were included to improve the Senate estimates.

Our estimates reveal that ticket splitting varies substantially across districts and states, even within the same election year. After removing the uncontested House races, the proportion of Bush splitters in a district ranges from .03 to .90, with a standard deviation of .19. The proportion of Bush splitters was under .1 in 23% of the districts, but it exceeded .5 in 27.3% of the districts. There was less variation in the district-level fraction of Dukakis splitters, which ranges from .02 to .72, with a standard deviation of .14. The proportion of Dukakis splitters was under .1 in 42.1% of the districts and above .3 in 29.6% of the districts. The relationship between the two types of ticket splitting is negative and strong (r = .94); in districts with many Bush splitters, there were few Dukakis splitters. Nevertheless, some form of ticket splitting was quite common in many congressional districts in 1988.

Because these are maximum likelihood estimates, King's method also produces standard errors for the two ticket-splitting parameters for each congressional district. In this case it yields fairly precise estimates of ticket splitting. The mean standard error of our district-level estimates for Bush splitters is .04, while for Dukakis splitters it is .05. Thus, traditional 95% confidence intervals drawn around our estimates would cover a range of about 15–20 percentage points.

THE VALIDITY OF OUR ESTIMATES

The Relationship between Split-Ticket Voting and Divided Outcomes

Does ticket splitting matter if only a minority of voters are involved? Can a small proportion of voters who split their ballot produce divided government? Are higher levels of ticket splitting associated with split outcomes when votes are aggregated at the district level? Table 2 provides the mean level of each type of

[6] With King's *EzI* program (version 1.21), we used the EI command to make the first-stage estimates and the EI2 command to make the second-stage estimates. See King (1997, chapter 15) and the Appendix for a more detailed description of this method.

[7] According to our estimates, Democrats were somewhat more likely than Republicans to abstain from the House contest. In addition, ballot roll-off was quite high in some congressional districts, especially if an incumbent ran unopposed or faced token opposition. As it turns out, our ticket-splitting estimates did not change substantially when we did not control for ballot roll-off. For an example that controls for roll-off in studying racially polarized voting, see Palmquist and Voss (1996).

[8] Eleven districts (from Arkansas, Florida, Louisiana, and Oklahoma) that featured uncontested House races were excluded from our analysis because election totals were not available. The state either did not put the House contest on the ballot (as in Louisiana when a candidate won a majority of the vote in the open primary) or did not tabulate the House vote in these districts. Survey respondents from these districts (61 total) were dropped from the 1988 NES sample for the comparisons of ticket-splitting estimates presented here. The Appendix offers a brief description of King's ecological inference method.

[9] The RD pattern of national government common in the 1970s and 1980s is remarkable. Indeed, according to Fiorina (1989, 140), "the emergence of a two-tier system in which Republican presidential majorities coexist with Democratic congressional majorities is the single most important feature of contemporary politics."

TABLE 2. Mean President/House Ticket Splitting by District Outcome, 1988

Ticket-Splitting Type	District Outcome			
	RR	RD	DR	DD
Bush splitters	.11	.57	.09	.59
	(.08)	(.24)	(.05)	(.24)
Dukakis splitters	.41	.09	.43	.07
	(.22)	(.06)	(.13)	(.05)
Number of districts	163	135	12	125

Note: Standard deviations of the point estimates are in parentheses.

President/House ticket splitting in different types of districts in 1988. Ticket splitting by Bush voters is quite high (.57) in districts that chose Bush and a Democrat but low (.11) in RR districts. Nevertheless, ticket splitting and divided outcomes are not entirely synonymous, though they are sometimes treated as such. Substantial numbers of voters split their ticket even in districts where candidates of the same party are elected. For example, .59 of Bush voters (on average) split their ticket in DD districts. Notice that Dukakis splitters are most common in DR and RR districts, and Bush splitters are most frequent in RD and DD districts. In accord with the argument we make below, this suggests that the primary determinant of ticket splitting is the local congressional race, not a national mandate for split control.

Consider that Bush can be expected to defeat Dukakis in more conservative districts. When a Democratic congressional candidate wins in such a district, it is likely due to a high rate of ticket splitting among Bush voters. If divided outcomes and split-ticket voting were unrelated, then one would expect no connection between the fraction of Bush splitters in a district and an RD election result.

To test this connection, we examine the relationship between the level of ticket splitting among Bush voters and the probability of districts having a unified Republican electoral outcome (RR) or a divided outcome (RD). The estimation is a logit model in which the dependent variable is the probability of selecting a Democratic congressional candidate, conditional on the selection of a Republican for president. The results, found in Table 3, show that the relationship between ticket splitting among Bush voters and the election of a Democrat to the House is remarkably strong. The classification rate of the one-variable model improves upon the naive baseline significantly, mispredicting less than 2% of districts based only on our knowledge of ticket splitting among Bush voters. The logit equation predicts that if more than .29 of the Bush voters split their ballot, then there is a better than 50% chance that the district will elect a Democrat to the House. Thus, both the direction and frequency of ticket splitting determine divided outcomes. This strong association between divided outcomes and our estimates of ticket splitting further validates the use of King's method in this case.

TABLE 3. Logit Equation Predicting Divided Outcomes (RD versus RR Districts) by Split-Ticket Voting

Variable	*b*
Bush splitters	63.84***
	(16.29)
Constant	−18.71***
	(4.77)
n	298
Correctly predicted	98.3%
Log-likelihood	−11.82

Note: Standard errors are in parentheses. ***$p < .001$, two-tailed *t* test.

Comparisons with Individual Estimates

It is useful to compare our ecological-based estimates with those from a more traditional individual-level source. Table 4 presents both for a quick comparison, along with estimates using Goodman's regression to show how much we improve on previous ecological inference methods. Using the 1988 NES, the estimated proportion of Bush voters who also chose a Democrat for the House (Bush splitters) is .341, as compared to our estimate of .331. The corresponding proportion of Dukakis splitters is a little less than half that, at .151, as compared to our ecological estimate of .198. At the national level our ecological estimates of ticket splitting are close to those produced by the NES, with smaller standard errors to boot. The NES estimates have standard errors of about .025, or 2.5%, so the 95% confidence interval around the national point estimates includes our estimates, making them statistically indistinguishable.

For the 33 Senate races in 1988, split-ticket voting was about as common as in the House contests. Roughly one in three Bush voters chose a Democratic Senate candidate, and about one in five Dukakis voters chose a Republican for the Senate. To compare the ecological estimates with individual data, we estimated split-ticket voting from the 1988 Senate Election Study (SES), which drew representative samples of about 75 people from each state with a Senate race that year. As Table 5 indicates, the ecological estimates of president/Senate ticket splitting are a bit lower than those produced by the SES, but we did not weight the SES data by state population, which may explain why those estimates are higher. Also, small states (where ticket splitting was more common in 1988) are overrepresented in the SES. Finally, while the Goodman estimates are close to the other two in this case, the large standard errors indicate a high level of uncertainty in the Goodman estimates.

EXPLAINING VARIATION IN TICKET SPLITTING ACROSS DISTRICTS AND STATES

In the previous section we found tremendous variation in ticket splitting across congressional districts and states. In this section we attempt to explain why split-ticket voting is more common in some electoral units than in others. We present results of multivariate

TABLE 4. Three Estimates of President/House Ticket Splitting, 1988

	Proportion Splitting Ballots		
Source	Bush Voters	Dukakis Voters	All Voters
Ecological estimate	.331 (.006)	.198 (.007)	.271
NES estimate	.341 (.021)	.151 (.016)	.254
Goodman estimate	.033 (.032)	−.108 (.037)	.018

Note: Standard errors are in parentheses.

analyses using our district and state point estimates of split-ticket voting as dependent variables. Our analysis provides a direct comparison of competing theories of split-ballot voting.

On the one hand, intentional theories are based on the notion that some voters prefer divided government and pursue it. Ideological moderates purposely vote for presidential and congressional candidates of different parties so that the separate branches of government will check each other (Alesina and Rosenthal 1995; Erikson 1988; Fiorina 1996; Jacobson 1990). Thus, split-ticket voting is consciously used to produce divided government. Some survey respondents claim that they prefer divided to unified control of government (Fiorina 1996). Ticket splitting may be a viable strategy for balancing the extreme positions of opposite party candidates, thereby producing moderate policies favorable to the median voter.

On the other hand, unintentional theories posit that split-ticket voting is a by-product of other forces, such as weak partisan attachments (Petrocik and Doherty 1996), ballot mechanisms (Beck 1997; Campbell et al. 1960; Campbell and Miller 1957), and the level of competition in congressional contests (Jacobson 1990). The lack of intention is attributed only to voters; candidates play an important role in producing split-ticket voting. For example, Bush voters in a district with an unchallenged Democratic candidate for Congress in 1988 have little choice but to vote for the Democrat or abstain. Furthermore, as party attachments in the electorate have weakened, campaigns have become more candidate centered (Wattenberg 1991), making it more likely that voters will choose candidates from different parties, even if not motivated by the specific goal of divided government. Jacobson (e.g., 1997) demonstrates that well-funded candidates with previous political experience are most adept at attracting voters from the opposite party. Voters simply may select the most visible or most appealing candidate for each office; given the low level of competition in many congressional contests, this decision rule often leads to ticket splitting.

Since the direction of ticket splitting is important in determining the election outcome, we treat the two types of ballot splitting (RD versus DR) separately. We estimated two linear regression equations to determine the factors that influence the fraction of Bush voters who select a Democrat for Congress and the fraction of Dukakis voters who select a Republican for Congress, using states or districts as our units of analysis. We focused most of our attention on RD splitting, since the 1988 election produced a Republican president and a Democratic Congress. We included two explanatory variables to account for the nature of the congressional campaign. One is a dummy variable for the party of the incumbent, who often develops a personal bond with constituents that crosses party lines (Fenno 1978). We expect that RD ticket splitting will be higher in contests with a Democratic incumbent and that DR splitting will be more common when the incumbent is Republican. The other variable is a measure of the Democratic candidate's spending as a fraction of total spending in the congressional contest. Campaigns are contests of competing messages crafted to attract voters, and the spending variable measures the extent to which the Democrat's campaign message is dominating that of the Republican. In addition, the spending measure allows us to control for the fact that campaign costs vary from one district to another. We expect that campaign spending will be positively associated with RD ticket splitting and negatively associated with DR splitting.

We also included a dummy variable for districts or states in the South. Since World War II, the South has supported Republican presidential candidates and Democratic congressional candidates more often than the rest of the country. Frymer (1994) and Frymer, Kim, and Bimes (1997) argue that RD ticket splitting has been more common in the South because Democratic candidates for Congress have been able to position themselves toward the conservative end of the ideological spectrum, thus capturing the support of many Republican presidential voters. It is argued that

TABLE 5. Three Estimates of President/Senate Ticket Splitting, 1988

	Proportion Splitting Ballots		
Source	Bush Voters	Dukakis Voters	All Voters
Ecological estimate	.285 (.017)	.187 (.020)	.237
SES estimate	.333 (.020)	.255 (.022)	.298
Goodman estimate	.359 (.190)	.262 (.226)	.313

Note: Standard errors are in parentheses.

TABLE 6. Regressions for RD Ticket Splitting

	President/House			President/Senate		
Variable	OLS *b*	WLS *b*	OLS β	OLS *b*	WLS *b*	OLS β
Ideological distance	—	—	—	−.029	−.042	−.088
				(.039)	(.049)	
Democratic incumbent	.107***	.095***	.279	.096*	.077	.297
	(.015)	(.023)		(.044)	(.055)	
Spending ratio	.350***	.443***	.664	.290**	.379**	.511
	(.021)	(.031)		(.084)	(.105)	
Ballot format	−.032***	−.074***	−.083	−.067	−.071	−.197
	(.008)	(.011)		(.037)	(.045)	
South	.052***	.116***	.116	.037	.017	.090
	(.009)	(.012)		(.046)	(.053)	
Constant	.065***	.049***		.138	.132	
	(.008)	(.011)		(.082)	(.098)	
Standard error	.073	.101		.093	.108	
n	361	361		32	32	
Adjusted R^2	.855	.845		.678	.686	

Note: Uncontested races are excluded. Cell entries are regression coefficients with standard errors in parentheses. *$p < .05$, **$p < .01$, ***$p < .001$, two-tailed *t* test.

moderate to conservative voters use simple proximity decisions to select congressional Democrats and presidential Republicans. We likewise expect RD ticket splitting to be more common in the South.

It has been suggested that ticket splitting is less common in states with a straight-party option on the ballot (Beck 1997; Campbell and Miller 1957; Rusk 1970). As Campbell et al. (1960, 275) note, "any attempt to explain why the voter marks a straight or split ballot must take account of the physical characteristics of the election ballot." They found that split-ticket voting was about 8% higher in states without a straight-party option in the 1950s. In 1988, 20 states had a ballot that allowed voters to pull one lever or check one box to vote a straight ticket (Brace 1993). Thus, we included a dummy variable for states with a straight-party option, and we expect it to be negatively associated with both types of ticket splitting.

The intentional theories offer few hard predictions, but Fiorina (1996) presents a clear hypothesis. Based on a simple spatial voting model in which moderate voters are located between an extremely conservative Republican Party and an extremely liberal Democratic Party, he finds that classic proximity voting may no longer be rational when candidates are far from a voter's ideal point. Instead, candidates from opposite parties are chosen for different offices so that they will be required to compromise and produce moderate policies after taking office. A key tenet of this argument, which Fiorina says is "probably the most important point," is the following prediction: "When the parties are relatively close, near the center of gravity of the electorate, ticket-splitting declines. When the parties move away from each other . . . they open up a large policy range in which ticket-splitting is the voter response" (Fiorina 1996, 81).[10]

We have a unique opportunity to test this hypothesis with available Senate data. The SES asked voters in states with Senate races to place the presidential and senatorial candidates on a seven-point ideological scale.[11] Taking the mean placements of the four candidates in each state, we computed the mean distance between Bush and the Democratic Senate candidate. Based on the prediction from Fiorina's balancing perspective, we should find a positive relationship between this distance and RD voting. In other words, RD ticket splitting should increase as the distance between Bush and the Democratic Senate candidate increases.

Table 6 presents regression results using RD ticket splitting as the dependent variable. The first column reports traditional ordinary least squares (OLS) estimates and the second column presents weighted least squares (WLS) estimates to account for the differing standard errors associated with our point estimates of ticket splitting.[12] In the WLS analysis, observations are weighted by the inverse of these standard errors, thus giving greater weight to observations with more precise estimates of ticket splitting. Because OLS and WLS produced similar results, we retained the OLS estimates because of their intuitive interpretations. The final column reports standardized OLS coefficients to assess the relative effects of the explanatory variables. Just a few independent variables do an impressive job of explaining variation in ticket splitting. Though parsimonious, the OLS model explains 86% of the variation in the President/House results and 68% of the variance in the President/Senate results.[13] This amount

[10] Fiorina models voter behavior as if it were conscious. Our test appropriately examines voter behavior without peering inside the "black box" to check the assumptions about process.

[11] "Unless we have data on both the Democratic incumbent and her Republican challenger, and unless we can put the presidential candidates and the voters on the same scale, we cannot differentiate among such different situations and hence decide whether ticket-splitting is consistent with an ideological consistency or ideological balancing model" (Fiorina 1996, 155). The SES data satisfy these criteria.

[12] The substantive results are unchanged if two-sided tobit models are estimated to account for a dependent variable censored at zero and 1.0.

[13] The Senate analysis is based on 32 states because the 1988 SES did

TABLE 7. Regressions for DR Ticket Splitting

	President/House			President/Senate		
Variable	OLS b	WLS b	OLS β	OLS b	WLS b	OLS β
Ideological distance	—	—	—	.002	−.0004	−.007
				(.036)	(.044)	
Republican incumbent	.085***	.101***	.300	.044	.008	.200
	(.011)	(.014)		(.036)	(.043)	
Spending ratio	−.252***	−.265***	−.659	−.225***	−.315***	−.610
	(.015)	(.018)		(.059)	(.068)	
Ballot format	.003	.005	.011	.034	.032	.153
	(.006)	(.006)		(.028)	(.033)	
South	.007	.005	.023	−.001	−.004	−.005
	(.006)	(.008)		(.032)	(.039)	
Constant	.309***	.313***	—	.277***	.353***	—
	(.013)	(.016)		(.070)	(.084)	
Standard error	.052	.059		.069	.078	
n	361	361		32	32	
Adjusted R^2	.863	.852		.585	.602	

Note: Uncontested races are excluded. Cell entries are regression coefficients with standard errors in parentheses. ***$p < .001$; two-tailed t test.

of explained variance is high in comparison to other voting behavior models but is especially surprising given that the dependent variable is merely a point estimate based on ecological data.[14]

The House and Senate equations differ only in that the Senate includes a measure of ideological distance between Bush and the Democratic senatorial candidate. Note that its coefficient is negative and statistically insignificant. The lack of significance is likely due to the small number of cases ($n = 32$) and corresponding degrees of freedom since the zero-order correlation between RD ticket splitting and the ideological difference between the candidates is −.370 ($p < .05$). Contrary to the balancing hypothesis, this means that RD ticket splitting does not increase as the parties polarize and that it may actually decrease. Split ballots are most common when Bush and the Democratic candidate are nearest each other, blurring their ideological differences enough to make partisan considerations in voting behavior less important (Frymer 1994; Kimball 1997). This finding fits with the Downs (1957) expectation that, in an attempt to win votes, party platforms will be ambiguous and overlapping.

In the House equation, RD voting is more common in the South. According to the OLS results, about 5% more Bush voters chose the Democratic House candidate in the South than elsewhere. Assuming for the moment that Democratic candidates are relatively conservative in the South, the distance between their position and that of Bush should be relatively small (Frymer, Kim, and Bimes 1997). Despite this, ticket splitting is higher, not lower. Again, intentional theories that attribute ideological balancing motives to split-ticket behavior are not supported.

The other variables in the equation are all significant and have the expected signs. The presence of a Democratic incumbent boosts RD ticket splitting by roughly ten percentage points in both House and Senate contests, holding other variables constant. Furthermore, a straight-party ballot mechanism decreases RD splitting about three percentage points in the House races and almost seven percentage points in the Senate contests. The greatest influence by far, however, is campaign spending. Recall that this variable is operationalized as the Democratic proportion of all congressional campaign spending. The OLS coefficient of .35 indicates that a Democratic House candidate who matches the Republican opponent dollar for dollar can increase RD ticket splitting by a whopping 17.5 percentage points over a Democrat who spends no money on the campaign. The parameter estimate of .29 in the Senate equation indicates that a Democratic senatorial candidate who matches the Republican opponent in spending will increase RD voting by nearly 15 percentage points over a Democrat who spends no money on the campaign. Furthermore, the relative effect of spending is overwhelming, as the standardized regression coefficient is roughly twice as large as that of the next most important variable.[15]

not obtain candidate placement information from Nevada respondents.

[14] One may argue that our ideological distance measure will not work properly if both Senate candidates fell on the same side of the median voter in a particular state. Using the SES, however, in almost every state the mean ideological placement for respondents fell in between the mean placements of the two Senate candidates (Erikson 1990).

[15] One may object that we have not accounted for the effect of the presidential candidates on ticket splitting. It is possible that some voters defect from their identified party in the presidential contest. To control for the presidential contribution, we included Bush's fraction of the vote in the regression models presented here. This did not improve the fit of the models and did not alter the effects of the other variables. In addition, it produced substantively weak and inconsistently signed regression coefficients. For example, the Bush variable had a positive coefficient ($p = .03$) in the first House model in Table 6, but a negative coefficient ($p = .24$) in the first House model in Table 8. The effect in Table 6 (although substantively small) suggests that Bush contributed to ticket splitting by attracting some Democratic votes.

Before exploring this finding, let us consider a similar regression, shown in Table 7, in which the dependent variable is the fraction of Dukakis voters who chose a Republican congressional candidate. The independent variables remain the same except that ideological distance is now measured between Dukakis and the Republican senatorial candidate, and the incumbency dummy now denotes a Republican. The analysis again points to the importance of the candidates in the congressional contest. For example, a Republican incumbent increases DR voting in House contests by about nine percentage points, other things being equal. In addition, spending remains the most critical factor, whether judged by the standardized coefficient or substantively, by interpreting the unstandardized coefficient. As a Republican candidate's share of total congressional campaign spending goes from zero to 100%, DR ticket splitting increases by about 25 percentage points in the House contest and about 23 percentage points in the Senate race. That is, the percentage of Dukakis splitters may jump by more than 20 points based on congressional spending patterns alone. Certainly, campaign spending is a proxy for other factors, such as the relative quality and experience of the candidates, but it also is critical to ticket splitting. Candidates spend most of their funds on advertising to increase their visibility and emphasize positive, familiar images in the minds of voters (Hernson 1997; Jacobson 1997). To the extent that a congressional candidate can dominate advertising and frame the campaign, s/he will attract more votes from the opposite party.

With regard to the Senate, as in Table 6, the results for ideological distance in Table 7 reveal that this is not a statistically significant influence on DR ticket splitting. In our model, increased distance between Dukakis and the Republican senatorial candidate does not produce more ticket splitting, contrary to predictions of the balancing perspective. Furthermore, DR voting is no more common in the South than in other regions of the country. The South in the 1980s was unique for one type of ticket splitting: a Republican for president and a Democrat for Congress. Finally, while a straight-party ballot option reduced RD ticket splitting in both Senate and House races, it did not have a noticeable effect on DR outcomes in 1988.

A more interesting setting may be the House elections. After all, government was divided along president/House lines throughout the 1980s, while both Senate and White House were controlled by Republicans for six years. It is more difficult for voters to split government control using their Senate vote, because there are only 33 races in a given election year. Our test of ideological balancing in presidential and senatorial elections did not support intentional theories, but comparable survey data were not available for the House. Instead, we used ideological scores for Democratic incumbents, since Democrats were a large majority in 1988, although this necessarily limits our test to RD splitting.[16] Our measure is the Poole and Rosenthal (1997) NOMINATE score, which is based on statistical analyses of roll-call voting.[17] In contrast to the SES data, based on citizen surveys, NOMINATE is more clearly exogenous to the vote decision. We used first-dimension point estimates from the 100th House to measure the ideological position of incumbent candidates. Values range from −1 (most liberal) to +1 (most conservative). If voters are more likely to split their ballot when the parties are polarized, then the ideological position coefficient ought to be negative. In other words, if motivated to balance extreme positions, then Bush voters would be more likely to split their ticket for a liberal than a conservative Democratic House candidate.

We included this ideology measure in a regression equation similar to the ones in tables 6 and 7, except that the incumbent dummy was removed by necessity. We also added a dummy indicating the presence of an "experienced" Republican challenger in the House contest (one who previously had held elective office or had run for a House seat). Table 8 reports the results. Though spending remains the most important influence on RD voting, the extremism of the Democratic incumbent plays a part as well. The coefficient is

TABLE 8. Factors Influencing RD Splitting in Districts with a Democratic Incumbent

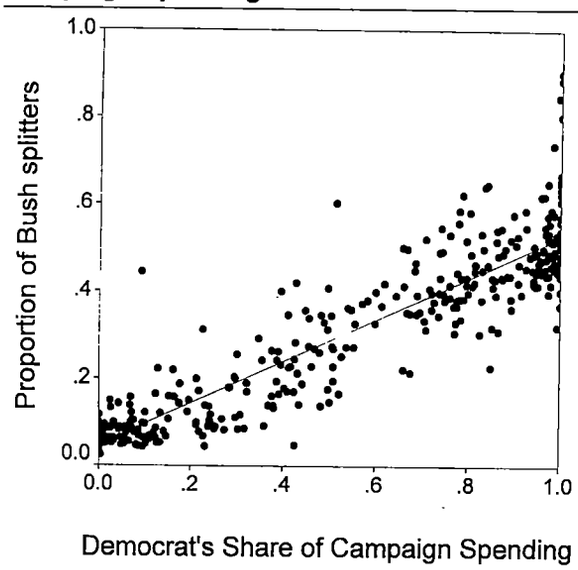
Variable	OLS b	WLS b	OLS β
Conservatism of Demoncratic Incumbent	.132** (.039)	.255*** (.053)	.22
Spending ratio	.327*** (.041)	.508*** (.056)	.47
Experienced Republican challenger	−.014 (.013)	−.031 (.018)	−.07
Ballot format	−.065*** (.012)	−.122*** (.016)	−.31
South	.041** (.016)	.092*** (.020)	.17
Constant	.250*** (.039)	.170** (.055)	—
Standard error	.083	.106	
N	190	190	
Adjusted R^2	.369	.655	

Note: Analysis only includes districts with a challenged Democrat. Cell entries are regression coefficients with standard errors in parentheses. ***$p < .01$, ***$p < .001$, two-tailed t test.

[16] RD splitting was the more common form in 1988 and corresponded to the election of Bush and a Democratic House. When we examined DR splitting, the results were similar to those presented in Table 8: Ticket splitting by Dukakis voters was more frequent in districts with a Republican incumbent toward the liberal end of the spectrum.

[17] NOMINATE is a maximum likelihood technique that uses roll-call data ("yeas" and "neas") to estimate the ideal points of legislators. It is based on a multidimensional spatial model in which legislators take roll-call positions that are nearest their ideal point with some error, thus making it probabilistic (Burden 1997). As is common practice, we use first-dimension NOMINATE scores, which explain more than 80% of the variance in roll-call voting patterns. Furthermore, we use a particular dynamic variant, D-NOMINATE. Substantive results are no different if other forms of NOMINATE are used; they correlate with one another at well above .95. See Poole and Rosenthal (1997) for a comprehensive review.

FIGURE 1. Split-Ticket Voting by House Campaign Spending.



positive and significant, indicating that the proportion of RD voters increases in accord with Democratic conservatism. Assuming that Bush adopts a right-of-center position, RD voting is more likely if the House Democratic candidate is closer to Bush, which means that voters seek ideological consistency rather than balance. This is consistent with arguments made by Frymer (1994) and Petrocik and Doherty (1996).

Substantively, the coefficient on the ideological variable means that the RD proportion increases by 13 percentage points when the Democrat moves one unit in the conservative direction on the [−1,1] NOMINATE interval. This is not a small effect and, in standardized terms, ranks only behind ballot format and the influence of money. Furthermore, the ideological variable reduces the magnitude of the coefficient for the South variable. This suggests that RD ticket splitting is higher in the South at least partly because the Democratic candidates position themselves closer to the conservative end of the spectrum.

The most robust finding from Table 8 and the other regression analyses is that congressional campaign spending has a dramatic influence on the percentage of voters who split their ballot. As the Democratic candidate's share of spending rises, so does the proportion of Bush voters who vote for that candidate. This relationship remains the strongest force in ticket splitting, even when controlling for such related and theoretically important variables as challenger quality. In fact, the challenger quality coefficient, while having the expected negative sign, is not statistically significant. That effect is clearly captured in the spending variable, as experienced challengers are most adept at cutting into an incumbent's spending advantage (Jacobson 1997). As we elaborate below, it is not the inherent quality of candidates that matters but their ability to represent a credible alternative to the incumbent.

To illustrate the critical role of spending, Figure 1 presents a simple scatterplot showing the share of Democratic spending and the proportion of Bush splitters in contested House districts. The least-squares regression line is superimposed. The relationship is clearly linear, positive, and strong considering the crudeness of the data ($r = .91$, $p < .001$). Though money has been shown to be an important variable in congressional campaigns (Jacobson 1997), its power to induce ticket splitting has been largely unexplored (cf. Kimball 1997).

We suggest that campaign spending increases voter defection from the opposite party by drowning out the opponent's message. As the spending proportion of the Democrat nears 100%, the Republican's self-promotion is scarcely noticed by voters. Lopsided spending patterns—near both ends of Figure 1—indicate one-sided messages in a campaign. As Zaller (1992) has shown, a campaign or other media message can have a substantial effect on public opinion when countervailing messages are absent. The dominant campaign's repeated advertising raises the number of favorable "considerations" associated with that candidate in the minds of potential voters. In the voting booth, they have little incentive to vote for the unknown candidate.

This examination of the causes of split-ticket voting at the district and state level finds that most of the influences are outside the voter's control, which supports the unintentional theories of divided government. This does not mean that divided control is an accident. Voters appear to be rationally choosing the candidates who seem the most competent, defined by the cues of incumbency and campaign spending, the two largest influences on split-ticket voting. Though ideological position is a factor when congressional incumbents are on the ballot, voters do not intentionally balance a conservative presidential candidate with a liberal congressional candidate to achieve moderate policy outcomes. Our evidence indicates that split-ticket voting actually decreases as candidates move farther apart. Voters choose sincerely in presidential and congressional campaigns but must resort to the best—sometimes the only—congressional candidate when intercandidate quality differences are large.

CONCLUSION

Amid the flurry of research on divided government and split-ticket voting, this article makes two contributions. First, it uses a new ecological inference technique to produce more accurate and efficient estimates of the direction and magnitude of ticket splitting in different states and congressional districts. Previous research had to depend on aggregate data—thereby inducing the ecological inference problem—or individual-level survey data that could not be used to estimate district-level parameters. These new estimates alone are valuable, for they demonstrate considerable variation in the direction and magnitude of ticket splitting from one electoral unit to the next. Though our application was to 1988, we believe that our conclusions apply to most modern presidential elections.

The second contribution lies in explaining the newly

TABLE A-1. Ecological Inference Notation for District *i*

Presidential Voting Decision	House Voting Decision: Democrat	Republican	Vote (Subtotal)	Abstain	Fraction of All Voters
Democrat (Dukakis)	β_i^b	$1 - \beta_i^b$	λ_i^b	$1 - \lambda_i^b$	X_i
Republican (Bush)	β_i^w	$1 - \beta_i^w$	λ_i^w	$1 - \lambda_i^w$	$1 - X_i$
Fraction of voters	T_i	$1 - T_i$	V_i	$1 - V_i$	1

revealed split-ticket voting patterns. Accounting for some intentional and unintentional forces that affect voting behavior, we find that the latter exert a tremendous influence. Though voters act reasonably considering the array of candidates offered to them, they apparently do not consciously choose divided government in the face of feasible alternatives. Elite decisions, including the decision of particular candidates to run for Congress, coupled with the financial advantages that most incumbents possess drive voters toward split ballots.

APPENDIX: KING'S METHOD

King's ecological inference technique involves a combination of procedures. It begins by treating the problem as a $2 \times C$ table in which the marginals are known from aggregate data but the cells are not. Notation for the known and unknown quantities can be found in Table A-1. First, the *method of bounds* is employed to restrict estimates of β_i^w and β_i^b, the cell quantities of interest, to a narrower region than the [0,1] interval (Duncan and Davis 1953). In our application, β_i^w is the proportion of Bush voters who split their tickets, and $1-\beta_i$ is the proportion of Dukakis voters who split their tickets. Knowing these deterministic bounds increases the amount of information that can be used in a statistical model. King's method posits a probabilistic distribution of the quantities of interest that assumes that β_i^w and β_i^b are distributed truncated bivariate normal (TBN), conditional on X_i.

Using the TBN distributional assumption, one can create a tomography plot (β_i^w by β_i^b), in which each line represents a district. The true values of β_i^w and β_i^b must fall somewhere on the line. The normality assumption helps detect a mode on the tomography plot where most of the lines cross and improves estimates by "borrowing strength" from other districts. Intuitively, the most likely values for β_i^w and β_i^b are nearest the mode, which is the peak of the three-dimensional normal bell shape.

After adding a couple of modest assumptions, five intermediate parameters that define the TBN distribution are estimated via a maximum likelihood function (i.e., two means, two standard deviations, and a correlation that make up the vector ψ). The parameters are then transformed and rescaled. Values of these parameters are drawn from the posterior distributions using importance sampling. The means of the distributions of β_i^w and β_i^b are used as point estimates, and standard errors (and confidence intervals) are based upon the variation in the simulated values. This is a real benefit, as earlier methods suffered from an inability to report accurate levels of uncertainty associated with point estimates (King, Keohane, and Verba 1994). This is the intuition; see section III of King (1997) for a complete description of the method.

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Information Effects in Collective Preferences

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Contrary to much of the literature on collective opinion, I find that the low levels and uneven social distribution of political knowledge in the mass public often cause opinion surveys to misrepresent the mix of voices in a society. To assess the bias introduced by information effects, I compare "fully informed" collective preferences simulated from actual survey data to collective preferences revealed in the original data. Analysis of policy questions from the 1988 and 1992 American National Election Studies shows that group differences in knowledge, along with the public's modest average level of political knowledge, can cause significant distortions in measures of collective opinion. The mass public may appear more progressive on some issues and more conservative on others than would be the case if all citizens were equally well informed. To the extent that opinion polls influence democratic politics, this suggests that information effects can impair the responsiveness of governments to their citizens.

A number of path-breaking studies in recent years have suggested that the mass public's inattention to politics may have less bearing on the quality of its collective opinions than previously thought (e.g., Converse 1990; Page and Shapiro 1992; Popkin 1991; Sniderman, Brody, and Tetlock 1991; Wittman 1995). These studies emphasize that while most individuals tend to be ill informed about the political world, the availability of heuristic shortcuts and the filtering process of statistical aggregation may help compensate for this lack of knowledge in measures of collective opinion, such as election results or opinion surveys. If this line of thinking is correct, then we can conclude with Page and Shapiro (1992, 385) that opinion surveys provide a "good deal of coherent guidance about policy." If the mass public is unable to compensate effectively for its lack of political knowledge, then the use of surveys and other measures of collective opinion as inputs to the political process may be rightly questioned.

In this article I extend recent work by Delli Carpini and Keeter (1996) and Bartels (1996) on the measurement of information effects in collective preferences. Contrary to much of the literature on collective opinion, this study finds that the low levels and uneven social distribution of political knowledge in the mass public often cause opinion surveys to misrepresent the mix of voices in a society. Correcting for information asymmetries reveals that many collective policy preferences would look quite different if all citizens were equally well informed about politics.

Because knowledgeable respondents are better able to form opinions consistent with their political predispositions (Bennett 1995; Converse 1964; Delli Carpini and Keeter 1996; Lockerbie 1991; Stimson 1975; Zaller 1992), and because they tend to give opinions more frequently than other people (Althaus 1996a; Delli Carpini and Keeter 1996, 230–1; Krosnick and Milburn 1990), the demographic characteristics of well-informed people—who tend to be more affluent, older, white, and male compared to the ill informed—can cause collective preferences to reflect disproportionately the opinions of some groups more than others. These two dynamics can create information effects in measures of public opinion. By *information effect*, I mean a bias in the shape of collective opinion caused by the low levels and uneven social distribution of political knowledge in a population. While others have examined the individual-level effects of political knowledge on response stability (e.g., Delli Carpini and Keeter 1996, 231–4; Feldman 1989, following Converse 1964) and the role of political knowledge as a link between political predispositions and policy or voting preferences (e.g., Bennett 1995; Delli Carpini and Keeter 1996, chapter 6; Zaller 1992), the focus here is more narrowly on the macrolevel impact of these effects on measures of collective opinion.

Building on the simulation approach developed independently by Delli Carpini and Keeter (1996) and Bartels (1996), this article examines the substantive impact of information effects in policy preference data from the American National Election Studies (NES). Simulating "fully informed" collective preferences from actual survey data shows that group differences in knowledge, along with the public's rather modest average level of political information, can cause significant distortions in measures of collective opinion. More important, the direction and magnitude of these distortions fall into predictable patterns. The uneven social distribution of political knowledge causes the mass public consistently to appear more progressive on some issues and more conservative on others than might be the case if all citizens were equally well informed about politics. To the extent that opinion polls influence democratic politics, this finding suggests that information effects may impair the responsiveness of governments to their citizens.

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Earlier versions of this article were presented at the 1996 annual meeting of the American Political Science Association and the 1997 annual meeting of the International Communication Association. I am grateful to the ICPSR and the University of Michigan Center for Political Studies for making available the NES data. I am indebted to Ben Page, Susan Herbst, Jane Mansbridge, Patricia Conley, Scott Keeter, Michael Delli Carpini, Dennis Chong, Paul Gronke, and Steven Klepper for their helpful comments and suggestions over the course of this project. All analyses were conducted using SPSS for Windows 7.0.

HOW POLITICAL KNOWLEDGE AFFECTS COLLECTIVE OPINION

Survey after survey has shown that citizens are often at a loss to relate basic facts about the players, issues, and rules of the game that structure American political life (Delli Carpini and Keeter 1996). The extent of this ignorance led several early and influential studies to suggest that the public's political opinions are often fickle and not to be trusted (Almond 1950; Berelson, Lazarsfeld, and McPhee 1954; Converse 1964; 1970). Yet, in recent years this pessimistic view of public opinion has been challenged on several fronts. The first challenge stems from work in cognitive psychology showing that people who are ill informed about public affairs can nonetheless form opinions consistent with their political predispositions. They can do this by taking cues from trusted political elites about which policies they should prefer and by harnessing a variety of heuristic strategies to deduce their political preferences, thus avoiding the need to infer preferences from factual bits of knowledge stored in long-term memory (e.g., Ferejohn and Kuklinski 1990; Lupia 1994; Mondak 1994; Popkin 1991; Smith and Squire 1990; Sniderman, Brody, and Tetlock 1991; Zaller 1992). From this perspective, the public's low levels of information may not be a significant problem because many people apparently can compensate for their lack of knowledge with information shortcuts.

The second challenge comes from a line of work suggesting that aggregate opinion may be able to reflect the public's interests even when the opinions of most individuals are ill informed, ambivalent, indifferent, or inconsistent (e.g., Converse 1990; Ferejohn and Kuklinski 1990; Page and Shapiro 1992). This view suggests that the process of aggregating opinions should tend to cancel out the more or less random opinions given by ill-informed respondents. To the extent that this occurs, measures of collective opinion should reflect the nonrandom opinions of knowledgeable respondents. A related argument stemming from Condorcet's jury theorem suggests a similar conclusion. Based entirely on statistical probabilities, the theorem shows that, under certain conditions, groups tend to provide more informed decisions than individuals (Condorcet [1785] 1972; Grofman and Owen 1986; Ladha 1992; Miller 1986; but see Austen-Smith and Banks 1996). From this perspective, it is the aggregation process itself which generates meaningful collective opinions.

Further support for "revisionist" challenges comes from experimental studies suggesting that the common methods used to measure information about politics may actually test recall ability rather than knowledge-in-use. Known as the "on-line" or "impression-driven" model of information processing (Lodge, McGraw, and Stroh 1989; Lodge, Steenbergen, and Brau 1995), this view suggests that many people process information at the time they are exposed to it, update their opinion accordingly, and then quickly forget the information itself while retaining the updated summary judgment. Thus, people may express informed preferences even though they may be unable to recall the factual information used to shape those preferences. From this perspective, the public's apparently low levels of political knowledge may be a red herring. Its judgments may be more informed than they seem in light of the apparently poor performance of those citizens on knowledge tests.

Despite the note of optimism sounded by these revisionist arguments, empirical evidence in support of their claims is quite modest. A number of studies have detailed how people *can* use on-line processing and various information shortcuts to make up for a lack of factual knowledge, but there is surprisingly little evidence that large numbers of people *do* use these strategies effectively, regularly, and across a wide range of situations and issues. More glaring still is the lack of support for the collective rationality hypothesis. I am aware of only two attempts to test this idea empirically. One study (Bartels 1996) found that aggregation helped voters act as if they were somewhat better informed than they actually were, but not as if they were fully informed. The other (Althaus 1995) found that statistical aggregation had quite limited information-pooling qualities.

Revisionist perspectives also tend to overlook an important fact: Low information levels are only half the problem. Just as important is the observation that some kinds of people tend to be better informed than others. Knowledge of politics is concentrated among those who are politically and socially advantaged. College graduates and relatively affluent people tend to be consistently well informed, while high school dropouts and relatively poor people tend to be consistently ill informed (Delli Carpini and Keeter 1996; Neuman 1986; Sigelman and Yanarella 1986). Political knowledge also is distributed unevenly among groups with distinctive and potentially competing political interests. For instance, whites tend to be more informed than blacks, men more than women, and older people more than younger people (Bennett 1988; Delli Carpini and Keeter 1996; Neuman 1986; Sigelman and Yanarella 1986).

There are two ways that information asymmetries among groups can undermine representation in opinion surveys. The first is by affecting the demographic correspondence between a survey sample and the group of people who give substantive responses. Those who are poorly informed about politics tend to give "don't know" and "no opinion" responses at much higher rates than more knowledgeable people (Althaus 1996a; Delli Carpini and Keeter 1996, 230–1; Krosnick and Milburn 1990). This tendency leaves the group of opinion givers disproportionately well educated, affluent, male, white, and middle aged relative to the population they are supposed to represent (Althaus 1996b). Because these voices tend to be overrepresented in the ranks of opinion givers, the particular needs, wants, and values expressed by some groups may come to be represented disproportionately in collective preferences.

The second way that information asymmetries affect representation is by influencing the quality of opinions

that respondents provide. Our ability to form preferences consistent with our political predispositions is often mediated by the quality and quantity of political information we can bring to bear on an issue. The importance of knowledge to the formation of policy preferences comes from the way that values are connected to attitudes through beliefs: beliefs about the state of the world, cause-and-effect processes, what government is currently doing, and the likely outcomes of government actions (Delli Carpini and Keeter 1996, chapter 6; Downs 1957, 79–80). Some ill-informed people may believe a policy is the "correct" solution without knowing whether it is consistent with their predispositions or whether it is the best way to achieve a given end. Others may believe a policy serves their interests, while someone with more perfect or complete information can see that the policy is diametrically opposed to them. Because the well informed are likely to have more accurate beliefs than the ill informed, they are more likely to express policy preferences consistent with their political predispositions (Converse 1964; Lockerbie 1991; Stimson 1975; Zaller 1992). As a result, the interests of respondents who are relatively well informed may come to be more accurately reflected in measures of collective opinion. In other words, such measures may reflect the needs, wants, and values of whites better than those of blacks, men better than women, and the rich better than the poor.

It would seem that the low levels and uneven social distribution of political knowledge may indeed have an important bearing on the quality of surveyed public opinion. The problem is how best to measure any potential distortion in collective preferences brought about by information asymmetries. One must find a way to estimate how the opinions people express in surveys might change if respondents were more completely informed about the issues.

MEASURING INFORMATION EFFECTS IN COLLECTIVE PREFERENCES

Ever since Marx suggested that "false consciousness" distracts workers from their material interests, students of politics have grappled unsuccessfully with how to determine whether people's interests are at odds with their opinions. Some, like Marx and Edmund Burke before him, argue that political interests are objective and can be identified for any group of people without regard to their stated preferences. Some, like Jeremy Bentham and John Stuart Mill, claim that interests are subjective and thus inseparable from the expressed wishes of individuals speaking for themselves. But in light of inherent problems with each of these definitions, more recent work has focused instead on interests as "fully informed" or "enlightened" preferences (Bartels 1990; Connolly 1972; Dahl 1989, 180–1; Delli Carpini and Keeter 1996; Lippmann 1955, 42; Mansbridge 1983, 24–6). In this perspective, as Jane Mansbridge (1983, 25) puts it, interests are revealed in "the preferences that people would have if their information were perfect, including the knowledge they would have in retrospect if they had a chance to live out the consequences of each choice before actually making a decision." By equating interests with hypothetical fully informed preferences, this perspective provides a useful approach for determining when the preferences expressed by individuals may be at odds with their "fully informed" needs, wants, and values.

The difficulty lies in sorting out the best among several possible ways of operationalizing this definition of interests. Two traditional approaches have been used to explore what collective opinion might look like if opinion givers were relatively better informed than the general public. The first is to purge ill-informed respondents from among the ranks of opinion givers through the use of filtering questions. While this is a particularly blunt method—it ignores completely how individual opinions might change with more political knowledge—it is nevertheless widely used by survey organizations to isolate "informed" public opinion.

The second approach manipulates the amount and quality of information available to people who express preferences of various kinds. Experimental methods for assessing the role of information in preference formation have been used extensively in social and cognitive psychology. These methods have also been used to explore what fully informed collective policy preferences might look like. To date, the most ambitious use of such methods has been by James Fishkin (1991, 1995) in experiments with deliberative opinion polls, which bring a random sample of ordinary citizens to a central location where they are provided with detailed policy information and an environment in which to discuss issues. In this way, argues Fishkin (1995, 171), "the deliberative poll can be thought of as an actual sample from a hypothetical society—the deliberative and engaged society we do not have."

Yet, even experimentation provides an unsatisfactory measure of fully informed collective preferences. It is possible in an experimental setting to provide people who are normally oblivious of the political world with information that they can use to formulate preferences, but ill-informed subjects cannot be equipped with other important traits normally associated with being well informed: the cognitive styles and information processing strategies characteristic of politically knowledgeable people (Fiske, Lau, and Smith 1990; Krosnick and Milburn 1990; McGraw and Pinney 1990; Sniderman, Brody, and Tetlock 1991), the knowledge stored in long-term memory that affects how new information is perceived and used to update attitudes (Delli Carpini and Keeter 1996; Krosnick and Milburn 1990; Zaller 1992), and the confidence, developed through experience, that one is able to understand complicated political issues and events (Krosnick and Milburn 1990). Moreover, typical experimental settings also fail to duplicate the social contexts in which political information is acquired and used to form preferences (Huckfeldt and Sprague 1995). It would seem that while experimental methods are especially useful in differentiating informed from ignorant people, they are less suited to predicting the sorts of policies a fully informed public might prefer.

Where filter questions and experimental methods fall short, a third way of simulating fully informed preferences offers promise. Independently pioneered in work by Delli Carpini and Keeter (1996, chapter 6) and Bartels (1996), this approach uses multivariate regression to simulate how individual opinions might change if opinion givers were better informed about politics. Unlike filter questions and experimental methods, this approach is explicitly premised on the concept of "enlightened preferences" and on the social construction of political interests. Estimates of fully informed opinions are generated by assigning the preferences of the most highly informed members of a given demographic group to all members of that group, simultaneously taking into account the influence of a wide range of demographic variables. For instance, if policy preferences of well-informed respondents from union families differ from those of ill-informed respondents from union families, then this approach assigns the mix of fully informed preferences to all respondents from union families. But instead of considering only the bivariate relationship between union membership and policy preferences, this method looks at union respondents who are women, from a certain income level, who live in eastern states, are married, own homes, of a certain age, and so on. If the most informed people sharing all these characteristics have different preferences from the least informed people, then their mix of fully informed preferences is assigned to everyone who shares their demographic characteristics.

The present study extends this basic approach in several ways. First, I use logistic regression to avoid the restrictive assumption that political information must have a linear relationship with preferences.[1] Second, I estimate fully informed preferences for people who give "don't know" and "no opinion" responses in the actual data, following the assumption that as information levels rise, the proportion of people who give opinions or turn out to vote should also rise. Third, while Bartels examined vote choices in presidential elections and Delli Carpini and Keeter analyzed five scales representing different policy domains, I conduct simulations on individual policy questions representing a broad range of political issues. Although the individual questions analyzed here may be less reliable measures of attitudes than scales composed of multiple questions, analyzing the marginals of specific questions has two distinct advantages. It can reveal the influence of question wording on information effects and, since marginal percentages seem to be the lingua franca of opinion surveys in the political sphere, how the surveyed opinion that "counts" politically may be skewed by the social distribution of political knowledge.

The logit model I use for simulating fully informed opinions is structured as follows:

$$\text{prob}\,(Y_i = 1) = \alpha + \beta_1 I_i + \sum \beta_k D_{ik} + \sum \delta_k (I_i * D_{ik}) + e_i,$$

where Y_i is respondent i's dichotomous policy preference (e.g., 1 = "favor," 0 = "oppose"); I_i is respondent i's score on a scale of political information; D_{ik} is respondent i's score on the kth demographic characteristic; $I_i * D_{ik}$ is the product of respondent i's information score multiplied by respondent i's score on the kth demographic characteristic; and e_i is the error term for the ith observation. In this equation, β_1 is the coefficient for the information variable, β_k is the coefficient for the kth demographic characteristic, and δ_k is the coefficient for the kth interaction term.

The measures of political information are scales developed for NES data by Delli Carpini and Keeter (1993, 1996). These scales, which are detailed in Appendix A, are built primarily from direct measures of factual knowledge, in contrast to the exclusive reliance by Bartels on subjective ratings of interviewers.[2] The political information scale for the 1992 NES has a maximum value of 23, with a mean of 12.7 and standard deviation of 5.8; the scale for the 1988 NES has a maximum value of 20, mean of 10.3, and standard deviation of 5.0. These scales have respective alpha reliabilities of .893 and .876.

The D_{ik} terms account for the effects of *Education*, *Income*, *Age*, *Partisanship*, *Race*, *Gender*, *Marital Status*, *Occupation*, *Religious Affiliation*, *Union Membership*, *Homeowner Status*, *Parental Status*, *Financial Status*, *Region*, and *Type of Community* (see Appendix A for coding details). For the 1992 data I also included *Receiving Welfare Benefits* and *Receiving Other Benefits*.[3] These characteristics represent all the available demographic variables that tend to be relatively stable features of a respondent's makeup and that may be expected to have some bearing on policy preferences. Excluded from the analysis were attitudinal variables that may be determined by or confounded with levels of political information. The resulting mix of demographic variables is quite similar to that used by Bartels (1996) and by Delli Carpini and Keeter (1996), with one significant exception. I include party identification because it is a relatively stable trait, which puts it on par with the other demographic variables; more important, partisanship is a widely used cueing mechanism

[1] Delli Carpini and Keeter (1996) use ordinary least squares to estimate parameters in their simulation; Bartels (1996) uses a probit model to estimate parameters, which are then transformed in a way that assumes linearity of information effects.

[2] Interviewer ratings of respondent knowledge levels discriminate well relative to factual knowledge scales (Zaller 1985), but correlations between these measures run between .57 and .68, which demonstrates that the two are not synonymous (Delli Carpini and Keeter 1992, 1993; see also Luskin 1987). While interviewer ratings are frequently used as components of political knowledge scales (Delli Carpini and Keeter 1996; Zaller 1985), these findings suggest that they may be poor stand-alone substitutes for direct measures of political knowledge.

[3] These two variables were not available in the 1988 data. Including the constants and interaction terms, there were 52 parameters estimated in the 1988 data and 56 parameters in the 1992 data. While the number of parameters is substantial, the large sample size of the NES studies (1,775 completed interviews in 1988; 2,255 in 1992) should ensure the asymptotic properties of efficiency, lack of bias, and normality (Aldrich and Nelson 1984, 53; also see King 1989, 74–80).

and information shortcut for issue positions (Campbell et al. 1960; Page 1978; Rahn 1993; Rahn and Cramer 1996). To the extent that party identification serves as a heuristic shortcut, excluding it from analysis could exaggerate the apparent importance of factual information to policy preferences (cf. Dimock and Popkin 1995).

This model was applied to a set of policy questions asked in the 1988 and 1992 NES (Miller et al. 1991, 1993). Since a logistic regression (logit) model was used to estimate coefficients, the data set consists of all policy questions from these studies with binary response options and any other policy questions that can be collapsed straightforwardly into dichotomous distributions. This made for a total of 45 usable questions out of the approximately 100 available.

The simulation proceeds in four steps. First, policy preferences are regressed on the full set of information, demographic, and interaction variables. This step estimates the relationships among these variables and provides a set of coefficients for simulating fully informed opinions. Second, each respondent's score on the political information scale is changed to the highest possible value. In the 1992 NES, for instance, that value was 23 points, so all respondents in the 1992 study were assigned a score of 23. Third, each respondent's fully informed opinion is calculated by plugging the coefficient values obtained from step one into each respondent's actual demographic characteristics, substituting only the new values of the altered information variable and interaction terms. Fully informed opinions are also estimated in this step for respondents who gave "don't know" or "no opinion" responses in the actual survey. Thus, respondents counted as "missing" in the actual data were assigned the same mix of fully informed preferences as the people who share their demographic characteristics and who actually gave opinions. In essence, this is a sophisticated way of weighting the preferences of opinion givers who have a certain combination of demographic characteristics by the number of respondents who share those characteristics. Fourth, all the individual fully informed opinions, including those of people who originally responded "don't know" and "no opinion," are aggregated into a fully informed collective preference. I use the mean of the Y_i probabilities to construct fully informed marginal percentages that can be compared directly to actual marginal percentages. Using marginals to gauge the substantive influence of information effects provides what is probably the most easily interpretable test statistic that can be generated with logistic regression methods.

It is important to note that the marginals resulting from this simulation process can be compared directly to the actual marginals only because they are operationalized as the mean of the individual probabilities that $Y_i = 1$ instead of the sum of the predicted values of Y_i. For instance, if 60% of all respondents sharing a certain demographic profile favor a certain policy, each respondent has a probability of .6 for favoring the policy. The mean of these probabilities recovers the actual percentage in favor (.6 = 60%), but since the predicted value of Y_i for each respondent is 1 (because $.6 > .5$), estimates from predicted values would mistakenly show a group that is 100% in favor of the policy. The upshot is that most ordinary least squares applications (including the simulation method used in Delli Carpini and Keeter 1996) are less suited for estimating information effects than maximum likelihood applications because they introduce large amounts of error into such estimates.

The end results of this four-step transformation are uniformly high information levels across demographic groups and substantive opinions for all respondents. Using only the observed differences between well- and ill-informed respondents, this method imputes to all respondents the information processing strategies and cognitive styles employed by well-informed people. It also allows political information to interact with demographic characteristics in ways that may move preferences in one direction for some groups and in the opposite direction for other groups. This flexibility allows the model to reflect accurately the social diversity of needs, wants, and values. Of course, increasing levels of information may lead instead to greater consensus of opinion across groups, and the simulation method leaves that possibility open as well. Most important, this method is not predisposed to finding any information effects at all: If well- and ill-informed respondents give essentially the same mix of preferences, then the shape of the resulting fully informed preference should be about the same as the actual one (Bartels 1996, 208–9).

INFORMATION EFFECTS IN COLLECTIVE PREFERENCES

The appropriate test for the significance of information effects in the model used here is the likelihood ratio test (Bartels 1996, 209).[4] This test was found to be significant at the $p < .01$ level in 84.4% of the 45 questions and at the $p < .05$ level in 88.9%. In other words, the unrestricted model, which takes information effects into account, tends to provide a substantially better fit to the data than does the restricted model, which assumes no information effects. Information effects in policy questions appear to be the norm rather than the exception.

[4] Similar to the F-test used to compare improvement of fit in OLS equations, the likelihood ratio test compares the log likelihoods of an unrestricted logit model, which includes all the variables, and a restricted logit model, which lacks the information and interaction terms, to determine the significance of the differences between them. For example, the -2 log likelihood of the unrestricted equation in Table 1 is 2063.7, while that of the restricted equation without interaction terms (not shown) is 2159.0. The result is a χ^2 value of 95.3, with $m = 28$ degrees of freedom, where m is equal to the number of parameters in the restricted model. This value is significant at the $p < .00001$ level, so the null hypothesis of no information effects can be safely rejected.

TABLE 1. Logit Coefficients for Opposition to Spousal Notification Laws (1 = Oppose)

Independent Variables	Main Effects		Interaction Effects (× Information)	
Information (1–23)	−.027	(.089)	...	...
Education (years)	−.043	(.072)	.013**	(.005)
Income (percentile)	.011	(.008)	−.000	(.001)
Age (years)	.009	(.014)	−.001	(.001)
Republican	.568	(.443)	−.075*	(.033)
Democrat	−.149	(.408)	.015	(.031)
Black	.663	(.447)	−.057	(.034)
Female	.062	(.358)	.053*	(.024)
Married	.783*	(.367)	−.072**	(.026)
Union family	−.353	(.460)	.028	(.031)
Homeowner	−.948**	(.358)	.048	(.025)
Parent w/child at home	.069	(.352)	−.003	(.024)
Receive welfare benefits	.045	(.472)	.020	(.034)
Receive other benefits	.397	(.428)	−.046	(.034)
Financially worse off	.178	(.321)	−.015	(.022)
Protestant	.186	(.473)	−.041	(.032)
Catholic	.188	(.518)	−.063	(.035)
Other religion	−.353	(.587)	−.006	(.040)
East	−.328	(.524)	.004	(.034)
Midwest	−.683	(.479)	.024	(.031)
South	−.645	(.456)	.030	(.030)
Urban	−.194	(.352)	.021	(.023)
Rural	−.575	(.398)	.026	(.028)
Retired	−.021	(.670)	−.018	(.047)
Homemaker	−.058	(.499)	−.004	(.038)
Executive/ Professional	−.244	(.573)	.020	(.036)
Clerical	.606	(.531)	−.036	(.039)
Technical/Sales	.873	(.585)	−.050	(.039)
Constant	−1.808	(1.262)		

Note: Standard errors of parameter estimates are in parentheses. Beginning log likelihood = −1221.9; ending log likelihood = −1031.8. Beginning correct classifications = 65.4%; ending correct classifications = 73.1%. N = 1,894. *$p < .05$, **$p < .01$.

Information Effects in Attitudes toward Spousal Notification Laws

Before discussing the general findings from these simulations, it will be helpful to take a close look at an example of how information asymmetries can bias collective preferences. A question from the 1992 NES asked: "Would you favor or oppose a law in your state that would require a married woman to notify her husband before she can have an abortion?" To illustrate how the fully informed collective preferences discussed later were obtained, the logit coefficients for this question are displayed in Table 1. Although this equation correctly predicts responses for nearly three-quarters of opinion givers and represents a statistically significant improvement over the restricted model,[5] the relative paucity of significant coefficients is notable. A similar pattern was found in the Bartels simulation of information effects in vote choices: Only between 14% and 24% of coefficients achieved conventional levels of significance (Bartels 1996, calculated from tables 1 and 4–8). The reason for this pattern appears to be a multicolinearity problem stemming from the abundance of interaction terms correlated with political information scores as well as with the demographic variables.[6] Examining the restricted form of this equation confirms that, absent the information and interaction terms, almost half the coefficients attain conventional levels of significance (data not shown). Rerunning the regression using only the significant variable clusters identified in Table 1 produces no unexpected changes in the size or direction of coefficients. These findings suggest that the relatively large standard errors of many of the coefficients in the unrestricted equation should have little or no effect on the unbiasedness of the coefficients themselves.

A more substantial concern with the data shown in Table 1 is the potential for specification error that comes from excluding attitudinal variables. Given the need to exclude such obviously relevant measures as attitudes toward abortion and women's rights, in the analyses that follow I avoid any suggestion that the model used here can *explain* individual-level opinions. Instead, I focus on what the model is intended to capture: differences in opinion between groups as well as the relationship between information and policy preferences within groups.

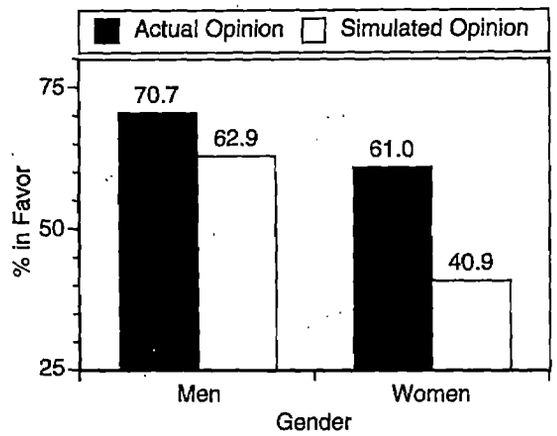
Figure 1 provides a detailed comparison of the actual marginal percentages for this question and those simulated for a public with uniformly high levels of political knowledge. In the actual marginals, nearly two-thirds of respondents said they favored a spousal notification law. Yet, in contrast to this apparently strong majority opinion, the fully informed collective preference is almost evenly divided on the issue. The only difference between these two measures is that the latter controls for the uneven social distribution of political knowledge.

As a precaution, I also tested whether the presence of insignificant coefficients in Table 1 affected the simulated marginal percentages. A second measure of fully informed collective opinion was estimated using a regression containing only the significant variable clusters shown in Table 1 (information, education, partisanship, gender, marital status, and home ownership). The result of this alternative simulation was a fully

[5] See footnote 4.

[6] Most of this multicolinearity comes from the nature of the demographic variable clusters. For instance, in the 1992 NES, Republican partisanship is correlated with Democratic partisanship at −.78. Likewise, living in a suburban area is correlated at −.57 with living in an urban area and −.42 with living in a rural area. The rest is due to associations between different variables. For example, the scale of political information is correlated at .56 with education and .42 with income. Being married is correlated at .42 with income and .33 with being a homeowner. All of this is complicated by the set of interaction terms, which are linearly but imperfectly related to both the information scale and the set of demographic terms. Despite these associations, the theoretical approach to information effects adopted here dictates that all these variables be included in the logit model. The result is a large number of insignificant coefficients, but the coefficients themselves should nonetheless be valid for the purposes to which they are put.

FIGURE 1. Actual and Fully Informed Opinion on Spousal Notification Laws



Total Sample	Favor	Oppose	N
Actual marginals	65.5%	34.5	100% (1894)
Simulated marginals	51.2%	48.8	100% (1969)

informed collective preference that was 52% opposed and 48% in favor of spousal notification laws. Deviating only about three percentage points from the simulated marginals of the full equation, this result confirms the accuracy of the model shown in Table 1, despite the frequency of insignificant coefficients.[7]

Even more interesting than the substantive shift in the collective preference are the underlying dynamics of change among male and female respondents. Figure 1 shows that the direction of change from actual to simulated opinion is identical for both genders: Controlling for information asymmetries led to decreased support among both men and women for spousal notification laws. But the magnitude of this change was much smaller for men than for women. Opinion among male respondents shifted only eight percentage points, compared to twenty points among females. This smaller change among males was primarily due to a ceiling effect from their higher average information scores and to the weaker relationship for males between political information levels and opposition to notification laws (data not shown).

After assigning all respondents equally high information levels, the group preference of fully informed men (i.e., support for notification laws) remained unchanged from that of actual opinion. Thus, the fifteen-point swing in collective opinion came mostly from changes in the opinions of female respondents. As shown in Figure 1, group opinion among women shifted twenty percentage points in a pro-choice direction once information levels were raised and standardized. This resulted in an almost perfect reversal of majority opinion among female respondents: Whereas 61% of women said they favored a spousal notification law in the actual data, 59% said they opposed it in the simulated data.

These findings suggest that the information imbalances between men and women suppress the magnitude of pro-choice opinion revealed in opinion surveys. The fully informed majority preference of women on the issue of spousal notification laws is diametrically opposed to that of men, while their actual majority preference is the same as that of men.

General Patterns of Information Effects in Collective Preferences

With this insight into the effects of information disparities on collective preferences, we now turn to the general findings from the simulation data. Table 2 displays these overall results for each of four issue categories and for all questions together (Appendix B contains results for individual questions). Foreign policy covers the use of military force abroad, economic sanctions, arms agreements, and whether the United States should become more involved in solving problems around the world. The fiscal category addresses levels of government spending and taxation. Operative issues deal with the size and scope of the federal government, the election of legislators, and government regulation of the economy. Social policy covers such topics as abortion, gay rights, the death penalty, and affirmative action.

The first column shows the mean percentage point change between actual and fully informed opinion for all questions in each category. A score of five, for example, indicates a five-point shift in the marginals of one of the response options, as when collective opinion changes from 50% to 55% in favor. Table 2 shows that the average difference between actual and fully informed opinion was about seven percentage points. Questions dealing with fiscal issues had the largest average point change, while social policy questions had the smallest average difference between actual and simulated opinion measures.

These averages give a somewhat misleading picture, however, as the distribution of point differences is skewed toward the low end of the scale. Twenty of the 45 questions (44%) had a difference of between zero and five points, 13 (29%) differed between six and ten points, nine questions (20%) had differences of between eleven and fifteen points, and three (7%) differed by more than fifteen points. In the more than half of observations in which nontrivial changes were observed—that is, changes on a magnitude greater than five percentage points—the differences between actual and fully informed opinion averaged 10.8 points. When collective opinion changed by more than a few points, it tended to shift quite dramatically.

[7] In this particular case, a shift on the magnitude of three points nevertheless signals a reversal in the collective preference simulated for a fully informed public. Although a shift of this magnitude does not count as a change in preference ordering for this study (it would still be counted as a tie: see footnote 9 below), it demonstrates the importance of examining the size of point changes in conjunction with any changes in preference ordering.

TABLE 2. Differences between Actual and Fully Informed Collective Preferences

Category	Average Point Change	*N* with Different Preference Order	Fully Informed Opinion Relative to Actual Opinion
Foreign policy issues (*N* = 8)	6.13 (3.80)	1 of 8	More dovish and interventionist
Fiscal issues (*N* = 6)	11.61 (7.28)	3 of 6	More willing to pay for more services and deficit reduction, less willing to maintain high defense spending
Operative issues (*N* = 8)	9.22 (6.41)	2 of 8	More opposed to government control of economy, more in favor of free market approaches
Social policy issues (*N* = 23)	5.49 (4.16)	3 of 23	More progressive in general, but slightly less progressive on some racial issues
All issues	7.08 (5.35)	9 of 45	

Source: Pooled data from 1988 and 1992 NES.
Note: Standard deviations are in parentheses.

Earlier it was suggested that the point differences between actual and simulated opinion might be exaggerated if party identification variables were omitted from the simulation equations. This expectation turned out to be incorrect. Rerunning the simulations without party identification variables (data not shown) produced estimates of information effects that were essentially identical to the simulation results reported here.[8]

While Table 2 shows that correcting for information asymmetries can lead to substantial shifts in collective opinion, changes of equal size may not be equally meaningful. A change from 80% to 90% in favor of a policy is in some ways less important than a change from 45% to 55% support; the former merely reinforces the majority opinion, while the latter indicates a substantive shift in majority opinion. The relative importance of the differences between actual and fully informed opinion can thus be clarified by noting when they cause collective policy preferences to change.

The middle column of Table 2 shows the number of questions in each category for which the preference order of fully informed opinion differed from that of actual opinion. Three possible collective preferences were considered: a majority in favor, a majority opposed, or a tie between options.[9] Any case in which the collective preference in actual opinion differed from that in the simulated marginals was coded as a change in preference order.

The frequency with which preference shifts occurred is a significant finding. After controlling for information effects, collective preferences changed in fully one-fifth of policy questions. Change was most frequent in the fiscal category and least frequent in the area of foreign policy, although the small number of questions makes it difficult to draw any firm conclusions about differences between issue categories. The important point is that controlling for the effects of information on individual responses often results in new collective preferences. This contrasts somewhat with the findings of Bartels (1996), who studied information effects in presidential election outcomes. Although Bartels found information effects in vote choices that are comparable to the patterns examined here, in every election he studied, correcting the popular vote totals for the presence of information effects would have resulted in the same outcome as was historically the case.[10] In contrast, the collective ordering of policy preferences seems to be much more sensitive than aggregate vote choices to information effects.

My interpretation of what fully informed opinion looks like relative to actual opinion is given in the third column of Table 2. Fully informed opinion on foreign policy issues is relatively more dovish and interventionist than actual opinion. For example, while 29% of respondents in 1988 agreed that the United States should stay out of problems in other parts of the world, the level of agreement dropped to just 18% of fully informed responses. An example of dovishness in fully informed foreign policy preferences comes from another question from the 1988 NES: "Do you favor or oppose using American military forces in the Middle East to protect oil shipments?" While 64% of actual responses favored military deployment, support dropped to 58% of fully informed responses.

[8] Of the simulations controlling for party identification, 57.8% (n = 26) produced smaller estimates of information effects than the simulations that omitted party identification variables, hardly an improvement over chance. Moreover, the mean difference between simulations that omitted party identification and the measures of fully informed opinion listed in Appendix B was only two-thirds of a percentage point. Contrary to expectations, controlling for party identification seems to have little influence on the size or direction of information effects in these data.

[9] A tie was defined as any marginals falling within plus or minus 3 percentage points of the 50% mark. These boundaries approximate the 95% confidence interval for point estimates from these data. It follows that a majority is defined as at least 53.5% of responses.

[10] Calculated from Table 2 of Bartels 1996 and popular vote totals from the *Statistical Abstract of the United States*. Correcting actual popular vote totals with the estimated aggregate deviations from fully informed voting reported in Table 2 of Bartels 1996 has the following results: Nixon wins by 59.0% instead of 60.7% in 1972, Carter by 51.5% instead of 50.1% in 1976, Reagan by 56.3% instead of 50.7% in 1980 and by 53.9% instead of 58.8% in 1984, Bush by 56.4% instead of 53.4% in 1988, and Clinton by 45.2% instead of 43.0% in 1992.

Simulated opinion on fiscal issues is more favorable toward paying for deficit reduction and for a larger number of governmental services than is actual opinion. The proportion of respondents willing to pay higher taxes in order to reduce the deficit rose from 31% in the actual data to an impressive 53% in the simulated data. And while 35% of actual respondents agreed that "the government ought to cut taxes even if it means putting off some important things that need to be done," agreement dropped to 22% when controlling for information effects. Likewise, only 30% of actual respondents in the 1992 NES said that they would be willing "to pay more in taxes so that the government could spend more on the services you favor," but support rose to 47% among fully informed respondents. There were only two exceptions to the tendency for fully informed opinion to favor increased federal spending. While 68% of actual respondents felt that the United States should maintain high levels of defense spending, support dropped to 64% for fully informed respondents, and in terms of satisfaction with current levels of Social Security, 40% of actual respondents said benefits were about right or too high, compared to 48% of fully informed respondents.

With regard to operative issues, fully informed opinion is relatively more opposed than actual opinion to the idea of "big government." While 63% of actual opinion givers in 1988 agreed that "the government is getting too powerful," the level of agreement rose to 71% among fully informed respondents. Likewise, in 1992, 65% of actual respondents felt "there are more things that government should be doing," compared to only 50% of fully informed respondents. In simulated opinion, this option tied with "the less government the better."

In the area of social policy, fully informed opinion is more progressive than actual opinion on most issues. Simulated opinion is consistently more pro-choice than actual opinion (39% versus 25% opposed to parental notification laws) and more supportive of gay rights (40% versus 28% favored allowing gay couples to adopt children). A similar direction of change was observed for most social policy issues, the one exception being specific programs aimed at giving preferential treatment to African Americans. On matters such as affirmative action and school integration, fully informed opinion tends to be slightly less supportive than actual opinion. At the same time, simulated opinion is slightly more progressive than actual opinion on the general need for government to guarantee equal opportunity and fair treatment to blacks (see Appendix B).

The general pattern emerging from the simulations is for fully informed opinion to be more dovish and interventionist on foreign policy, more progressive on social and fiscal issues, and more conservative on operative issues. It is interesting that the same general trends were found when the simulations were reestimated using years of education in place of the factual knowledge scales and using only statistically significant variable clusters (data not shown). Furthermore, these general patterns of differences between actual and fully informed opinion are consistent with results from other simulation methods (Althaus 1996a; Delli Carpini and Keeter 1996; chapter 6), with the broad patterns of opinion change among participants in the National Issues Convention held in January 1996 (Public Broadcasting Corporation 1996), and with the results of question filter experiments (Bishop, Oldendick, and Tuchfarber 1983). They are also consistent with previous studies comparing differences between well- and ill-informed survey respondents on a variety of policy issues (e.g., Althaus 1996a; Dimock and Popkin 1995; Key 1961, 331–41; Neuman 1986, chapter 3; Popkin and Dimock 1995).

CONCLUSION

Correcting for the low levels and uneven social distribution of political knowledge can change our understanding of collective preferences in significant ways. This study found that the effects of information asymmetries on collective opinion are both larger and more common than suggested by previous work. Controlling for information effects produces an average change of seven percentage points in question marginals and reveals that one in five policy questions might have a different collective preference if everyone were equally well informed about politics.

Obviously, these findings only suggest what collective opinion might look like in a hypothetical world of politically attentive citizens. While it is tempting to suppose that simulations of fully informed opinion somehow reflect the underlying political interests of the mass public, I draw no such conclusion from these data. The key contribution of this study lies not with speculating about the public's true interests but with demonstrating that survey results sometimes look the way they do because so many people are ill-informed about politics and because certain people tend to be better informed than others.

Interpreting the findings presented here in light of the existing literature on public opinion is a surpassingly difficult task, for the results of this study seem to both challenge and support the idea that the mass public's command of political knowledge is related to the quality of its collective preferences. On the one hand, the results can be seen as validating the hypothesis that on-line processing, heuristic shortcuts, and statistical aggregation help the mass public compensate for its lack of political knowledge. After all, the optimist might say, nearly half the question marginals shifted less than six points after correcting for information effects, and the ordering of collective preferences remained unchanged for eight in ten questions. On the other hand, the findings can be seen as challenging the view that heuristics, on-line processing, and statistical aggregation help an ill-informed public express policy preferences similar to those it might give if it were more knowledgeable about politics. After all, the pessimist might say, more than half the question marginals shifted at least five percentage points, and one-quarter shifted more than ten points after correcting for information effects. Moreover, the pessimist might add, the

ordering of collective preferences changed for one out of five questions when information effects were controlled.

Choosing between these competing interpretations requires a standard for determining when information effects are a problem. Earlier I made use of three such standards for evaluating individual questions: when likelihood ratio tests for the presence of information effects attain statistical significance, when differences between actual and fully informed collective opinions exceed five percentage points (which corresponds roughly to exceeding the 95% confidence interval for point estimates made from 1988 and 1992 NES data), and when correcting for information effects results in different preference orderings for collective opinions. Yet, such standards are of little help when moving beyond individual questions to evaluate more general patterns in groups of questions. Here the issues of interest are how often preference orderings must change for information effects to be called frequent, and how sizable mean point changes must be for information effects to be called large. As the answers to these questions vary with the vantage point and expectations of the individual observer, I submit my own assessment of the relative importance of information effects, recognizing that it is one among several valid interpretations of these results.

Given the frequency of shifts in collective preferences and the sizable point changes that occurred with some regularity when controlling for information asymmetries, I am inclined to see these results as challenging more than supporting the revisionist arguments that collective preferences are "rational" and reliable guides for public policy. Yet, the familiar warnings about the public's civic incompetence that have been made since the early 1950s are similarly challenged by this analysis. It would seem that a middle position fits best with the results presented here: Sometimes collective policy preferences are significantly influenced by the public's modest level of knowledge about politics, and sometimes they are not. Most of the time, at least in NES data, the aggregate effect of information asymmetries seems likely to be palpable but not decisive.

APPENDIX A: VARIABLES AND CODING

Demographics

Income is measured as income percentiles, *Education* is measured as years of formal schooling, and *Age* is measured in years. *Republican* and *Democrat* are coded 1 for the appropriate partisan identification (including independent "leaners") and 0 for all others. *Black* is coded 1 for African Americans and 0 for all others, *Female* is coded 1 for women and 0 for men, *Union Family* is coded 1 if any member of the respondent's household belongs to a union. *Homeowner* is coded 1 for people who own their own home and 0 for all others, and *Parent with a Child at Home* is coded 1 for respondents whose minor children reside with them and 0 for all others. *Receiving Welfare Benefits* is coded 1 for people living in a household where they or family members receive food stamps, Medicaid, unemployment, and/or AFDC and is coded 0 for all others. *Receiving Other Benefits* is coded 1 for people living in a household where they or family members receive payments from Medicare and/or Social Security. *Financially Worse Off* is coded 1 for respondents who reported being worse off financially compared to one year before and 0 for all others. *Protestant*, *Catholic*, and *Other Religion* are each coded 1 for the appropriate religious affiliation and 0 for all others, with atheists, agnostics, and those with no religious affiliation comprising the reference category for this set. *East*, *Midwest*, and *South* are coded 1 for respondents living in the respective census region and 0 for all others, with those from western states as the reference category for this set. *Urban* and *Rural* are coded 1 for those from the appropriate census-defined place of residence and 0 for all others, with suburban as the reference category for this set. *Retired*, *Homemaker*, *Executive/Professional*, *Clerical*, and *Technical/Sales* are coded 1 for the respective occupational status and 0 for all others.

Political Information

The information measures used in this study were originally constructed and tested by Delli Carpini and Keeter (1993, 1996). These scales are primarily additive measures of correct answers to factual knowledge questions (correct = 1; incorrect or no answer = 0). They also incorporate a subjective assessment of respondent knowledge level made by the interviewer at the conclusion of each interview. Three kinds of factual knowledge items were used to construct these scales: relative location tests, in which correct answers are constructed by comparing responses to two different questions; open-ended questions asking respondents to identify the job or political office held by a public figure; and closed-ended questions testing knowledge of constitutional powers, which party held majority status in both houses of Congress, and which party was more conservative than the other. An example of a correct answer to a relative location test is placing the Republican Party as relatively more conservative than the Democratic Party on a seven-point ideology scale, regardless of where on the ideology scale a respondent actually places the two parties.

Besides the interviewer rating score (v555, reverse coded), the questions for the 1988 NES information scale included identifying the offices held by Ted Kennedy (v871), George Shultz (v872), Margaret Thatcher (v875), Yasser Arafat (v876), William Rehnquist (v873), Michail Gorbachev (v874), and Jim Wright (v877); naming the majority party in the House (v878) and Senate (v879); identifying the relative ideological locations of Bush and Dukakis (v231, v232), identifying the relative ideological locations of the Republican and Democratic parties (v234, v235), and locating the relative positions of the Republican and Democratic parties on national health insurance (v321, v322), government services (v307, v308), defense spending (v315, v316), and job assurances (v328, v329).

Aside from the interviewer rating score (v4205, reverse coded), the questions for the 1992 NES information scale included identifying the offices held by Dan Quayle (v5916), William Rehnquist (v5917), Boris Yeltsin (v5918), and Thomas Foley (v5919); identifying which branch of the federal government was responsible for deciding the constitutionality of laws (v5920) and for nominating federal judges (v5921); naming the majority party in the House (v5951) and Senate (v5952); identifying which was the more conservative party (v5915); identifying the relative ideological locations of Republicans and Democrats (v3517, v3518), Bush and Clinton (v3514, v3515); and identifying the relative position of the parties on government services (v3704, v3705), Bush and

Clinton on government services (v3702, v3703), the parties on defense spending (v3710, v3711), Bush and Clinton on defense spending (v3708, v3709), the parties on job assurance (v3721, v3722), Bush and Clinton on job assurance (v3719, v3720), and Bush and Clinton on abortion (v3733, v3734).

APPENDIX B: SIMULATION RESULTS FOR INDIVIDUAL QUESTIONS

Table B-1. Levels of Actual Opinion and Simulated Estimates of Fully Informed Opinion, by Question

Question	Type	Year (Var #)	Actual Opinion	Simulated Opinion
"I am going to read you a statement about U.S. foreign policy and I would like you to tell me whether you agree or disagree: 'This country would be better off if we just stayed home and did not concern ourselves with problems in other parts of the world.' " (percent disagreeing)	Foreign	1988 (v254)	71.1	81.6
		1992 (v3604)	73.7	85.8
"The United States and the Soviet Union have recently reached agreements to reduce the number of nuclear arms. Do you approve or disapprove of these agreements?" (percent disapproving)	Foreign	1988 (v364)	12.6	8.7
"Do you favor or oppose using American military forces in the Middle East to protect oil shipments?" (percent opposed)	Foreign	1988 (v366)	35.7	42.6
"Some people think that the U.S. should increase the pressure on the South African government to change its racial laws. Others think the U.S. should not do this. What do you think—should the U.S. apply more pressure or not?" (percent opposed to more pressure)	Foreign	1988 (v861)	41.3	34.5
"The U.S. should maintain its position as the world's most powerful nation even it if means going to the brink of war." (percent disagreeing)	Foreign	1988 (v972)	42.9	48.2
"Do you think we did the right thing in sending U.S. military forces to the Persian Gulf or should we have stayed out?" (percent saying should have stayed out)	Foreign	1992 (v3608)	21.0	18.4
"Some people think that the U.S. and its allies should have continued to fight Iraq until Saddam Hussein was driven from power. Others think that the U.S. was right to stop fighting after Kuwait was liberated. What do you think? Should the war have continued or should it have stopped?" (percent saying should have stopped)	Foreign	1992 (v3630)	36.7	35.4
"In order to reduce the size of the federal budget deficit are you willing or not willing to pay more in federal taxes?" (percent unwilling)	Fiscal	1988 (v249)	68.9	46.3
"The government ought to cut taxes even if it means putting off some important things that need to be done." (percent disagreeing)	Fiscal	1988 (v944)	64.5	78.0
"Some people say the U.S. should maintain its position as the world's strongest military power even if it means continuing high defense spending." (percent disagreeing)	Fiscal	1992 (v3603)	31.9	35.6
"Would you personally be willing to pay more in taxes so that the government could spend more on the services you favor or would you rather keep your taxes the same even if this meant the government couldn't increase its spending as you would like?" (percent saying keep taxes the same)	Fiscal	1992 (v5922)	70.0	52.9
"In your opinion, are Social Security retirement benefits too low, about the right amount, or too high?" (percent saying about right/too high)	Fiscal	1992 (v6132)	40.1	48.7
"Do you favor or oppose taxes on Social Security benefits?" (percent opposed)	Fiscal	1992 (v6134)	87.0	81.0

Table B-1. (continued)

Question	Type	Year (Var #)	Actual Opinion	Simulated Opinion
"Some people have suggested placing new limits on foreign imports in order to protect American jobs. Others say that such limits would raise consumer prices and hurt American exports. Do you favor or oppose placing new limits on imports, or haven't you thought much about this?" (percent opposed)	Operative	1988 (v376)	23.7	39.7
		1992 (v3802)	33.1	38.5
"What is your feeling, do you think the government is getting too powerful or do you think the government is not getting too strong?" (percent saying government not getting too strong)	Operative	1988 (v848)	36.7	29.2
		1992 (v6016)	29.9	31.1
"Over the past year would you say that the economic policies of the federal government have made the nation's economy better, worse, or haven't they made much difference either way?" (percent saying worse)	Operative	1992 (v3541)	43.2	60.0
"A law has been proposed that would limit members of Congress to no more than 12 years of service in that office. Do you favor or oppose such a law?" (percent opposed)	Operative	1992 (v3747)	18.5	19.8
"Choose which of two statements I read comes closer to your own opinion . . . One, the less the government the better; or two, there are more things that government should be doing?" (percent saying there are more things government should do)	Operative	1992 (v5729)	64.9	50.3
"Choose which of two statements I read comes closer to your own opinion . . . One, we need a strong government to handle today's complex economic problems; or two, the free market can handle these problems without government being involved?" (percent favoring free market)	Operative	1992 (v5730)	26.5	41.5
"Do you favor or oppose laws to protect homosexuals against job discrimination?" (percent opposed)	Social	1988 (v852)	45.8	41.8
		1992 (v5923)	39.9	34.5
"Do you favor or oppose the death penalty for persons convicted of murder?" (percent opposed)	Social	1988 (v854)	19.9	24.8
		1992 (v5933)	20.6	24.0
"Some people say that because of past discrimination, blacks should be given preference in hiring and promotion. Others say that such preference in hiring and promotion of blacks is wrong because it gives blacks advantages they haven't earned. What about your opinion—are you for or against preferential hiring and promotion of blacks?" (percent against)	Social	1988 (v856)	79.9	83.1
		1992 (v5935)	79.9	84.0
"Should the government in Washington see to it that black people get fair treatment in jobs or is this not the federal government's business?" (percent saying this is not the government's business)	Social	1988 (v865)	42.7	39.7
		1992 (v5938)	42.7	42.9
"Some people say that because of past discrimination it is sometimes necessary for colleges and universities to reserve openings for black students. Others oppose quotas because they say quotas give blacks advantages they haven't earned. What about your opinion—are you for or against quotas to admit black students?" (percent against)	Social	1988 (v869)	63.7	65.8
		1992 (v5947)	67.4	68.4
"Equal opportunity for blacks and whites is very important but it's not really the government's job to guarantee it." (percent disagreeing)	Social	1988 (v965)	43.2	49.0
"Do you think the government should require companies to allow up to six months unpaid leave for parents to spend time with their newborn or newly adopted children, or is this something that should be left up to the individual employer?" (percent favoring decision left to employers)	Social	1992 (v3717)	68.8	63.0

Table B-1. (continued)

Question	Type	Year (Var #)	Actual Opinion	Simulated Opinion
"Would you favor or oppose a law in your state that would require parental consent before a teenager under 18 can have an abortion?" (percent opposed)	Social	1992 (v3735)	24.5	39.2
"Would your favor or oppose a law in your state that would allow the use of government funds to help pay for the costs of abortion for women who cannot afford them?" (percent opposed)	Social	1992 (v3737)	49.1	46.9
"Would you favor or oppose a law in your state that would require a married woman to notify her husband before she can have an abortion?" (percent opposed)	Social	1992 (v3739)	34.5	48.8
"Do you think government should provide child care assistance to low and middle income working parents, or isn't it the government's responsibility?" (percent saying government is not responsible)	Social	1992 (v3745)	38.8	40.3
"Do you think homosexuals should be allowed to serve in the United States Armed Forces or don't you think so?" (percent saying gays should not be allowed to serve)	Social	1992 (v5925)	41.4	37.2
"Do you think gay or lesbian couples, in other words, homosexual couples, should be legally permitted to adopt children?" (percent opposed)	Social	1992 (v5927)	72.3	59.0
"Do you think the government in Washington should see to it that white and black children go to the same schools, [or that it should] stay out of this area as it is not the government's business?" (percent saying government should stay out)	Social	1992 (v5932)	50.8	59.5
"Some people feel that we should use government funds only to support children who go to public schools, others feel that we should use government funds to support children's schooling regardless of whether their parents choose to send them to a public, private, or parochial school. How do you feel, or haven't you thought much about it?" (percent in favor of funding all schooling)	Social	1992 (v6023)	42.9	33.5
"Do you favor or oppose expanding Medicare to pay for nursing home care and long hospital stays for the elderly?" (percent opposed)	Social	1992 (v6136)	12.0	21.5
"Do you favor a law making English the official language of the United States, meaning government business would be conducted in English only, or do you oppose such a law?" (percent opposed)	Social	1992 (v6233)	30.6	28.7
"Do you think that immigrants who come to the U.S. should be eligible as soon as they come here for government services such as Medicaid, Food Stamps, Welfare, or should they have to be here a year or more?" (percent saying immigrants should wait a year or more)	Social	1992 (v6242)	78.9	70.8

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New Models for New Labour: The Political Economy of Labour Party Support, January 1992–April 1997

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This article uses newly available British time-series data to analyze dynamic interrelationships among Labour vote intentions, perceptions that the Labour leader would make the best prime minister, and Labour party identification. Error-correction models reveal that best prime minister perceptions and party identification have important short- and long-run influences on vote intentions. Tests of rival models indicate that personal economic expectations outperform other economic evaluations in the vote intention and party identification analyses. National retrospective judgments perform well in analyses of best prime minister perceptions, and emotional reactions to economic conditions significantly influence these perceptions as well as party identification.

Political parties can enjoy, as well as endure, remarkable reversals of fortune. The recent history of the British Labour Party provides an excellent example. In April 1992, Labour suffered its fourth consecutive general election defeat, and observers speculated that this might have been the party's "last chance" (e.g., Heath et al. 1994). Soon after that election, however, Labour support began to revive, and the governing Conservatives fell far and fast from public grace. Labour's resurgence was not ephemeral; rather, it continued and culminated in a spectacular victory in the May 1, 1997, general election.[1] In this paper, we analyze the dynamics of Labour's popularity surge by specifying new models of the political economy of party support. These models emphasize the short- and long-run effects of party leader images and party identification, as well as the influence of economic evaluations and emotional reactions to the economy, on Labour's vote intention share. The models thus depart from their predecessors, a large majority of which have relied heavily on a small set of macroeconomic indicators, especially inflation and unemployment rates, as principal explanatory factors (for reviews, see Lewis-Beck 1988; Miller 1989; Norpoth 1992). By incorporating variables that have received little or no attention in previous studies of aggregate movements in vote intentions, the new models are designed to help bridge the longstanding theoretical gap between such studies and individual-level analyses of electoral choice.

We first review four important debates concerning the specification of aggregate vote intention models. After presenting time-series data on movements in Labour vote intentions, best prime minister perceptions, and party identification over the January 1992–April 1997 period, we investigate the time-series properties of, and interrelationships among, these three variables. These analyses inform the specification and testing of alternative error-correction models of Labour vote intentions. Next, models of judgments that the Labour leader would make the best prime minister and Labour party identification are analyzed. These analyses enable us to assess the direct and indirect effects of several important variables on Labour vote intentions and to test the exogeneity of best prime minister judgments and party identification vis-à-vis vote intentions. A final set of analyses employ hitherto unavailable time-series data on emotional reactions to national and personal economic conditions to explore alternatives by which the economy affects party support. The conclusion reviews major findings and briefly discusses their implications for understanding the dynamics and determinants of party support in Britain and elsewhere.

THE DEBATABLE DYNAMICS AND DETERMINANTS OF PARTY SUPPORT

Since its introduction nearly half a century ago (e.g., Campbell, Gurin, and Miller 1954; Campbell et al. 1960), the concept of party identification has stimulated continuing theoretical and methodological controversies among students of voting behavior in the United States, Britain, and other countries. According to its original social-psychological definition, party identification is an enduring element in the political psychology of party support and, as such, exercises important direct and indirect effects on electoral choice. In this conceptualization, party identification is

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This research is funded by grants from the National Science Foundation (SES 930-9018 and SES 960-0018). We gratefully acknowledge the foundation's support. We also thank Robert Wybrow, Rory Fitzgerald, and Sheri Weber of British Gallup for their very helpful advice and generous support, and Jim Granato and the anonymous reviewers for many useful comments and suggestions. Neither the NSF nor Gallup is responsible for the data analyses and interpretations of the data presented here. The data may be downloaded from the ICPSR Publication-Related Archive (Study #7309) or our website data archive—www.psci.unt.edu/~hclarke/.

[1] Labour won 418 seats, the Conservatives 165, the Liberal Democrats 46, and all other parties 29. The distribution of the popular vote (for the United Kingdom) was: Labour 44.4%, Conservatives 31.4%, Liberal Democrats 17.2%, all other parties 7.0%.

an affective attachment that an individual forms with a political party, typically as a result of early-life socialization processes (e.g., Campbell et al. 1960, chapters 6–7; Miller 1991). Ceteris paribus, party identification tends to be reinforced and to strengthen over time, and it thereby acts as a powerful long-term force anchoring voting behavior and party fortunes in successive elections (e.g., Campbell et al. 1966; Converse 1976). This view of party identification has not received unanimous endorsement. Since the early 1980s, it has been repeatedly challenged by scholars who argue that party identification should be conceptualized as a summary "running tally" of current and past party performance evaluations, with more weight being given to recent, as opposed to earlier, assessments (e.g., Fiorina 1981, chapter 5; see also Achen 1992; Franklin 1984, 1992; Franklin and Jackson 1983).[2] Because these evaluations can vary, sometimes markedly, individual- and aggregate-level partisan changes are ongoing possibilities.

At the aggregate level, MacKuen, Erikson, and Stimson (1989, 1136) have demonstrated that American "macropartisanship," as measured by a question in monthly Gallup surveys, responds to voters' economic evaluations and presidential approval ratings. Abramson and Ostrom (1991) have argued, however, that the Gallup party identification question is flawed and that the venerable party identification measure in the biennial American National Election Studies (NES) is affected only weakly by economic evaluations and not at all by presidential approval. Although this debate has continued for several years in the United States (e.g., Bishop et al. 1994; Box-Steffensmeier and Smith 1996; Clarke and Suzuki 1994; MacKuen et al. 1992), in Britain a lack of appropriate time-series data has prevented scholars from effectively addressing long-standing disagreements over the nature and extent of partisan change (e.g., Budge, Crewe, and Farlie 1976; Crewe 1986; Crewe, Sarlvik, and Alt 1977; Heath, Jowell, and Curtice 1985; Heath et al. 1991; Heath and McDonald 1988; Heath and Pierce 1992; Sarlvik and Crewe 1983). For the same reason, party identification variables have not appeared in models of the interelection dynamics of British parties' vote intention shares.

A second debate concerns whether public reactions to party leaders affect vote intentions and electoral choice in Britain. Unlike the situation in the United States, the conventional wisdom in Britain has downplayed the significance of party leader images for explaining electoral choice (e.g., Butler and Stokes 1976, chapter 17; Crewe 1985; Dunleavy and Husbands 1985; Rose and McAllister 1990). Recent analyses of British Election Study and other election-survey data gainsay this received wisdom by demonstrating that leader images have strong effects on voting decisions (e.g., Crewe and King 1994; Miller et al. 1990; Stewart and Clarke 1992). These individual-level findings have been reinforced by the results of aggregate-level time-series analyses of the effect of prime ministerial approval ratings on governing party vote intentions (Clarke and Stewart 1995; Clarke, Stewart, and Whiteley 1997) and public perceptions of which leader would make the best prime minister (Nadeau, Niemi, and Amato 1996). Most of these aggregate-level analyses focus exclusively on governing (Conservative) party support, however, and the influence of party leader performance evaluations on opposition party vote intentions has not been studied extensively.[3]

A third controversy involves the effect of evaluations of national and personal economic conditions on party support. As noted, analysts traditionally have used inflation and unemployment rates and other objective indicators to investigate how the economy affects parties' vote intention shares. Such variables are uninformative concerning the subject referent (self/society) or time horizon (past/future) voters employ when making their party support decisions. In recent years, the inclusion of subjective economic evaluations, that is, public judgments of national and personal economic circumstances, has done much to improve model specification. In this regard, the widely cited Essex model specifies that personal economic expectations and interest rates drive governing party support (Sanders 1991, 1993, 1995; Sanders, Marsh, and Ward 1993). Interest rates also exert indirect effects on party support via their strong influence on personal economic expectations. Some studies (Clarke and Stewart 1995; Clarke, Stewart, and Whiteley 1997), however, have shown that other kinds of economic evaluations (personal retrospections, national retrospections, national expectations) also perform well in models of governing party (Conservative) vote intentions. How various types of economic evaluations fare in models of opposition party support remains to be investigated.

A fourth debate concerns the distinction between evaluation and emotion and their effects on political decision making. In studies of the political economy of party support, these concepts often are conflated in such terms as the "misery index" and the "feel good factor," which apply emotional labels to a combination of objective conditions (inflation and unemployment) and subjective evaluations (expectations about personal economic circumstances), respectively. But neither the misery index nor the feel good factor explicitly taps emotional responses to economic performance. This observation is important because some analysts forcefully argue that both evaluation and emotion should be incorporated into models of information processing and political support (Conover and Feldman 1986; see also Conover, Feldman, and Knight

[2] Formally, $PID_t = \Sigma\beta_{t-i,k}EVAL_{t-i,k} + \varepsilon_t$, where PID_t is party identification at time t, $EVAL_{t-i,k}$ is a vector of current and past party performance evaluations, and ε_t is $\sim N(0,\sigma^2)$. If party performance evaluations are viewed as an infinite distributed lag in which the effects of prior evaluations decline at a geometric rate (λ^i), a Koyck transformation may be employed for estimation purposes, that is, $PID_t = \alpha(1-\lambda) + \Sigma\beta_{t,k}EVAL_{t,k} + \lambda PID_{t-1} + \nu_t$, where $\nu_t = \varepsilon_t - \lambda\varepsilon_{t-1}$. The model may be theoretically motivated as a partial adjustment process. If so, the error process is Gaussian. See, for example, Gujarati (1988; 519–21). Individual-level analyses using these types of models in the British context include Alt (1984) and Clarke and Stewart (1984).

[3] See, for example, Norpoth (1992, chapter 9), and Nadeau, Niemi, and Amato (1996).

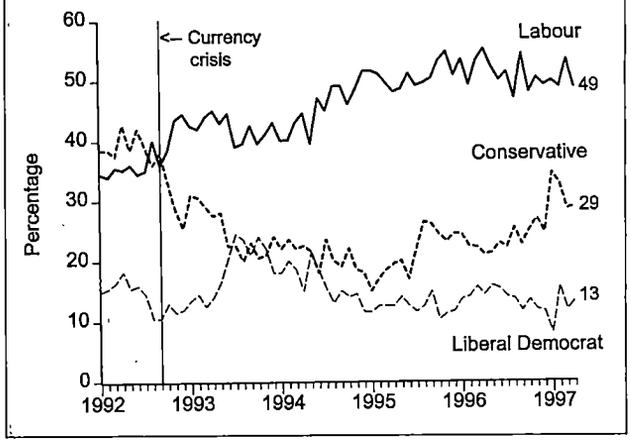
1986, 1987; Frank 1988; Marcus 1988; Marcus and MacKuen 1993; and Sniderman, Brody, and Tetlock 1991, 7–8, 20–7). These claims are provocative because models incorporating emotional reactions challenge the dominance of the rational choice paradigm, which either explicitly or implicitly undergirds much theorizing about how voters make party support decisions. As with party identification, a lack of suitable data has inhibited time-series analyses of the influence of emotional reaction variables on the aggregate dynamics of parties' vote intention shares.

We initiated a study in January 1992 to address these several debates. The study includes the standard sequence of questions on party identification from the British Election Study and a battery of statements measuring emotional reactions to national and personal economic conditions in British Gallup's monthly opinion polls (see Appendix). The inclusion of the British Election Study party identification battery avoids the controversy over question wording that has bedeviled research on the aggregate dynamics of partisanship in the United States. The emotional reaction items are ones used in pioneering research by Conover and Feldman (1986) (see also Marcus 1988, 740–1). The utility of data gathered by the party identification and emotional reaction questions is enhanced by the fact that the Gallup surveys regularly ask several questions concerning vote intentions, party leader performance judgments, evaluations of national and personal economic conditions, and perceptions of most important problems facing the country. We employ these monthly data to analyze alternative models of the determinants of Labour support over the January 1992–April 1997 period that culminated with the huge Labour victory in the May 1, 1997, general election.

MAPPING THE DYNAMICS OF LABOUR SUPPORT

Labour enjoyed large increases in public support after the party's defeat in the April 1992 general election. Although 35% voted Labour in that contest, exactly four years later 55% claimed that they would do so next time around (Figure 1). Although the *Labour Vote Intention Share* subsequently declined, in mid-April 1997 49% stated that they would cast a ballot for the party in a forthcoming general election. The substantial growth in Labour vote intentions was accompanied by large increases in the percentage of persons who thought that the *Labour Leader Would Make the Best Prime Minister* and the percentage who identified with the Labour Party. Regarding the former, one month after the 1992 election only 18% believed that former Labour chieftain Neil Kinnock was best suited to be prime minister (Figure 2). In every month after September 1992, however, Labour's leaders were favored over their Conservative counterpart, John Major. Labour's leadership edge grew dramatically when Tony Blair replaced John Smith, after the latter's untimely death in May 1994. Blair's lead over Major declined somewhat in the last seven months before the general election, but on the eve of that contest it remained a healthy 11% (39% v. 28%). As for party identification, although Labour's partisan share did not vary as much as vote intentions or best prime minister judgments,[4] it manifested substantial growth and a continuing dynamic between 1992 and 1997 (Figure 3), with the percentage of *Labour Identifiers* ranging from 30% (February 1992) to 47% (December 1994). In April 1997, 40% identified with the party.

FIGURE 1. Conservative, Labour, and Liberal Democrat Vote Intentions, January 1992–April 1997



Movements in Labour vote intentions, party identification, and best prime minister judgments were closely interrelated. Although Labour's vote intention percentage hovered above its partisan share virtually every month during the January 1992–April 1997 period, the two series tracked each other closely (Figure 4), and their overall correlation is strong ($r = +.66$). Judgments that the Labour leader would make the best prime minister moved in tandem with these two series. Correlations between these judgments, on the one hand, and Labour vote intentions and party identification, on the other, are +.87 and +.70, respectively. The substantial correlations among the three series suggest the possibility of important causal connections among them.

MODELING LABOUR VOTE INTENTIONS

Analysts studying the aggregate dynamics of party support often assume that their time-series data are stationary and, hence, can be analyzed in level form.[5] This is not an innocuous assumption because, if the

[4] The standard deviations (σ) for Labour vote intentions, perceptions of the Labour leader as making the best prime minister, and Labour party identification are 5.89, 6.65, and 3.73, respectively. As in the Labour case, the variation in Conservative and Liberal Democrat party identifications is much less than that for vote intentions and best prime minister judgments. The σ's are 6.73, 9.61, and 4.34, respectively, for the Conservatives, and 3.54, 3.85, and 2.33 for the Liberal Democrats. The percentage of Labour identifiers ranges between 30 and 47, whereas the percentage range is 19 to 38 for Conservative identifiers and 7 to 18 for Liberal Democrat identifiers.

[5] A time series is weakly stationary if its mean and variance are time invariant, that is, $E(x_t) = \mu$, $E(x^2_t) = \sigma^2$ for all t, and its

FIGURE 2. Party Leader Who Would Make the Best Prime Minister, January 1992–April 1997

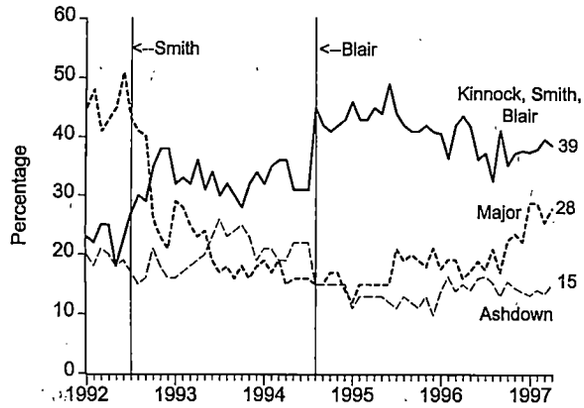
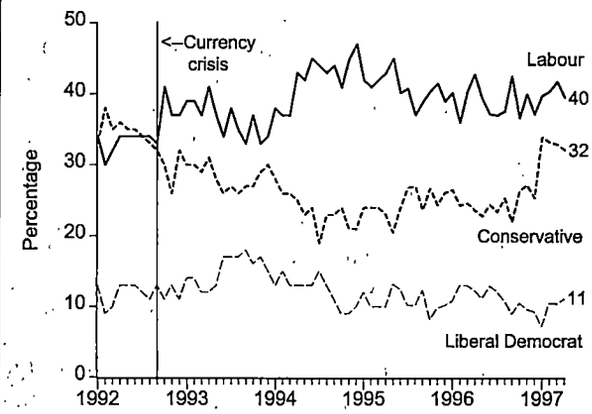


FIGURE 3. Conservative, Labour, and Liberal Democrat Party Identification, January 1992–April 1997



data are nonstationary, serious threats to inference arise. If the dependent and independent variables in a model are nonstationary, then "spurious regressions" may occur, with null hypotheses for regression coefficients being falsely rejected, even though the data generation processes for the variables being analyzed are wholly independent (e.g., Granger and Newbold 1974; Hendry 1980). Although investigators typically have reacted to this threat by differencing series believed to be nonstationary, this procedure necessarily ignores possible long-run relationships (e.g., Beck 1992, 1993). In certain circumstances, both problems may be addressed by specifying error-correction models that assess both short- and long-term relationships among variables of interest.[6] An error-correction specification is implied if nonstationary series integrated of order one (I[1]) are cointegrated (Engle and Granger 1987). Cointegrated series are in a dynamic equilibrium in the sense that they tend to move together in the long run. Shocks that persist over a single period are "reequilibrated" or adjusted by this cointegrating relationship. In an error-correction model, the coefficient for the error-correction mechanism measures the speed at which this adjustment process occurs. Evidence for cointegration is provided by demonstrating that a linear combination of the nonstationary series is a stationary variable.

Although political methodologists have shown considerable interest in the possibility of modeling nonstationary, cointegrating series in error-correction form, the prior question of whether series of interest are, in fact, nonstationary has provoked continuing controversy (e.g., Durr 1993a, 1993b; Ostrom and Smith 1993; Smith 1993; Williams 1993; see also Box-Steffensmeier and Smith 1996). The low power of diagnostic (unit-root) tests against local alternatives (e.g., Stock 1991) both stimulates these debates and hinders their resolution. Recognizing this problem, some analysts (e.g., Beck 1993) have argued on theoretical and statistical grounds that a number of the series of longstanding interest (e.g., presidential approval) likely are stationary but near-integrated[7] and, hence, long-memoried. In this regard, DeBoef and Granato's (1997a) Monte Carlo study demonstrates that in finite samples, near-integrated series mimic the behavior of nonstationary ones, so that analyzing such series in level form risks spurious regressions (see also Banerjee et al. 1993, 26–7; Hamilton 1994, 444–7). This study, however, also shows that regressions using near-integrated series in (first) differenced form avoid this threat. DeBoef and Granato's results recommend that the series considered here should not be analyzed in level form because unit-root tests (Dickey and Fuller 1979) (see Table 1) and other diagnostics (autocorrelation functions, univariate ARIMA models, visual inspection of graphs) indicate that these series are either nonstationary or long-memoried.[8]

In finite samples, both nonstationary and long-memoried series may have long-run relationships that can be usefully modeled in error-correction form (Beck

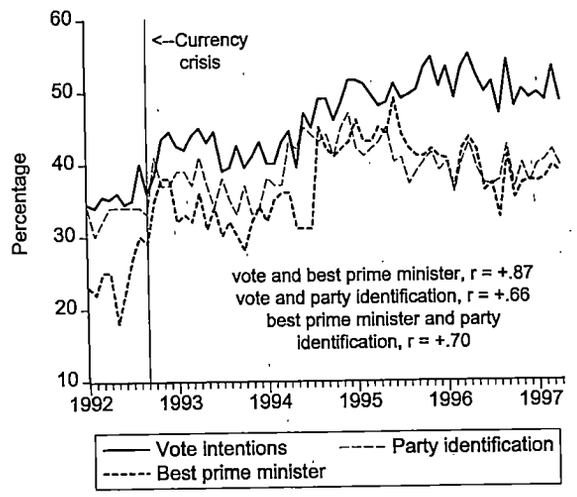
autocovariances $E(x_{t-1}-\mu, x_{t-j}-\mu)(i \neq j)$ depend only on the length of the time lag, k, separating i and j (Hendry 1995, 42).

[6] On the conceptual development and interpretation of error-correction models, see Alogoskoufis and Smith (1995); Hendry (1995, 286–306).

[7] A near-integrated process can be specified as: $y_t = \rho y_{t-1} + \zeta_t$, where ζ is a Gaussian error process, $|\rho| = 1 + \varepsilon$, and ε is a small, negative number. For example, the data-generating process (dgp) for a near-integrated series might be $y_t = .99y_{t-1} + \zeta_t$. Given this dgp, stochastic shocks erode only very slowly, and thus y_t is said to have long memory.

[8] In an analysis of governing (Conservative) vote intentions over the July 1979–April 1992 period, Clarke and Stewart (1995, 158) report similar results for unit-root tests for the economic evaluation variables. Since party identification questions were not asked in monthly Gallup surveys prior to January 1992, unit-root tests for this variable are necessarily confined to the present sample. Given the importance that some observers have ascribed to the currency crisis as a factor that precipitated the Conservatives' loss of public support, we replicated the unit-root tests using a dummy currency crisis variable to control for a possible structural break in the several series. As Perron (e.g., 1989, 1990) argues, structural breaks can confound unit-root tests, prompting analysts to conclude (falsely) that their series are nonstationary. Perron's modified unit-root tests that control for a possible structural break (at a known date, here September 1992) indicate all series are nonstationary. See Perron (1990, 158) for critical values for this test.

FIGURE 4. The Dynamics of Labour Support, January 1992–April 1997



1993, 241; DeBoef and Granato 1995, 1997b). Here, we specify an error-correction mechanism to capture such a relationship among Labour vote intentions, on the one hand, and Labour leader as best prime minister and Labour party identification, on the other. The existence of such a relationship is consonant with the aforementioned theoretical literature on the centrality of party identification in the matrix of forces affecting electoral choice, as well as the results of individual- and aggregate-level analyses of the determinants of voting behavior and vote intentions in interelection periods (see pp. 559–60 above).

Empirical evidence consistent with the presence of a long-run cointegrating relationship among these three variables is provided by Engle and Granger (1987) "two-step" and Engle and Yoo (1991) "three-step" procedures. The two-step procedure involves regressing Labour vote intentions on Labour leader as best prime minister and Labour party identification, and then testing the residuals for nonstationarity. This regression indicates that the best prime minister and party identification variables have strong, statistically significant, and properly signed (i.e., positive) effects on Labour vote intentions (Table 2). Also, as should be the case if the series in question cointegrate, a unit-root test shows that the residuals from this regression are a stationary (I[0]) variable. Estimates computed using the three-step procedure proposed by Engle and Yoo (1991, 257–63) confirm the statistical significance of the regression coefficients,[9] and, again, unit-root tests indicate the regression residuals are stationary (Table 2).

In addition to incorporating variables that capture the short- and long-term effects of best prime minister judgments and party identification on Labour vote intentions, the error-correction model includes measures of subjective economic evaluations.[10] We hypothesize that such evaluations are inversely related to support for Labour as the principal opposition party—as economic judgments become increasingly negative, Labour support grows, and, as they become increasingly positive, it diminishes. As discussed above, four types of economic evaluations (*Personal Expectations*, *National Expectations*, *Personal Retrospections*, and *National Retrospections*) that have figured prominently in debates concerning how economic conditions affect party support are employed in four variants of the model. Other economic variables used are *Interest Rates* and the percentages citing *Unemployment* and *Inflation* as the *Most Important National Problems*. Interest rates play a prominent role in the aforementioned Essex model, whereas the latter two variables index the salience in the public mind of two aspects of the macroeconomy (rates of joblessness and price hikes) that traditionally have been seen as important influences in the political economy of party support.

TABLE 1. Dickey-Fuller Unit-Root Tests for Labour Vote Intentions, Labour Leader as Best Prime Minister, Labour Party Identification, and Economic Series, January 1992–April 1997

Series	Dickey-Fuller Unit-Root Test (t)
Labour vote intentions[a]	−1.96*
ΔLabour vote intentions	−13.29
Best prime minister[a]	−2.43*
ΔBest prime minister	−10.60
Labour party identification[a]	−2.82*
ΔLabour party identification	−12.20
Economic Evaluations	
Personal financial expectations	−2.39*
ΔPersonal financial expectations	−8.78
Personal financial retrospections	−0.87*
ΔPersonal financial retrospections	−10.02
National economic retrospections	−1.47*
ΔNational economic retrospections	−7.42
National economic expectations	−2.62*
ΔNational economic expectations	−8.09
Most Important Problem	
Inflation[a]	−1.89*
ΔInflation	−10.94
Unemployment[a]	−0.58*
ΔUnemployment	−12.21
Education	−1.85*
ΔEducation	−10.78
Health	−1.32*
ΔHealth	−9.07
Interest rates	−2.96**
ΔInterest rates	−5.51
N for Dickey-Fuller test	63
N for augmented Dickey-Fuller test	62

*p = .05; fails to reject null hypothesis of nonstationarity, critical value = −2.91. **p = .01; fails to reject null hypothesis of nonstationarity, critical values = −3.54.
[a]Augmented Dickey-Fuller test, lag = 1.

[9] The three-step estimates are asymptotically equivalent to full-information maximum likelihood (Engle and Yoo 1991, 257). See also Harris (1995, 56).

[10] Cointegration tests involving the three Labour series and various economic series (considered one at a time) demonstrate that none of the latter is involved in a cointegrating relationship with the former.

TABLE 2. Cointegrating Regression: Labour Vote Intentions by Labour Leader as Best Prime Minister and Labour Party Identification, January 1992–April 1997

Regressor Variables	β	s.e.
Labour leader	.635	.083***
	(.737)	(.269)**
Labour party identification	.585	.078***
	(.488)	(.254)*
R^2 = .709		
N = 64		

Note: Engle and Yoo (1991) three-step estimates in parentheses. *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$; one-tailed tests. Unit-root test for residuals = −3.59, rejects null hypothesis of nonstationarity at .05 level (critical value = −3.44).

Hypotheses concerning the effects of these variables are straightforward: As interest rates and public concern about inflation and unemployment increase, support for the opposition Labour Party grows; when interest rates fall and public concern about prices and joblessness subsides, Labour support does so as well.

The vote intention models also include two other "most important problem" series, *Education* and *Health Care*, to tap levels of public concern with two issues emphasized by Tony Blair after he became Labour leader. Both issues grew in prominence as the 1997 election drew closer, and both were stressed by Labour in the election campaign. It is hypothesized that Labour vote intentions will vary positively with the percentages mentioning education and health as most important problems. Finally, we include several dummy variables that index highly salient events hypothesized to have affected Labour support since 1992. Events expected to exert a positive influence include the Black Wednesday *Currency Crisis* in September 1992,[11] the Conservatives' 1993 *Intraparty Divisions over the Maastricht Treaty*,[12] the *1994 European Elections/ Death of John Smith*,[13] the spring 1996 revelations of a possible link between bovine spongiform encephalopathy and Creutzfeld-Jakob disease in humans (the *"Mad Cow" Crisis*),[14] and the widely publicized *Conservative "Sleaze" Scandals* occurring at the beginning of the general election campaign in March 1997.[15] Events expected to have a negative effect include the *July 1995 Reselection of John Major as Conservative Leader*,[16] the February 1996 disclosure that Labour's shadow cabinet education spokesperson, Harriet Harman, had enrolled her child in a private school (*Harman Affair*),[17] and the Conservatives' anti-Labour media blitz and the intraparty bickering that surrounded Labour's 1996 shadow cabinet election and subsequent October 1996 annual conference (*Labour Conference*).[18] All of these events, except the currency crisis and the annual conference, are modeled as short-term effects.[19]

In equation form, the Labour vote intention models are:

$$\Delta LAB_t = \beta_0 + \beta_1\Delta LBPM_{t-i} + \beta_2\Delta LPID_{t-i} + \beta_3 ECM_{t-1} + \beta_4\Delta ECEV_{t-i} + \beta_5\Delta INTR_{t-i} + \beta_6\Delta INIP_{t-i} + \beta_7\Delta UMIP_{t-i} + \beta_8\Delta EDIP_{t-i} + \beta_9\Delta HEIP_{t-i} + \Sigma\beta_{10-k}EVENTS_{t-i} + \varepsilon_t, \quad (1)$$

where LAB = Labour vote intentions; LBPM = Labour leader as best prime minister; LPID = Labour party identification; ECM = error-correction mechanism; ECEV = one of four subjective economic evaluations; INTR = interest rates; INIP, UMIP, EDIP, HEIP = perceptions of inflation, unemployment, education, and health as most important problems;

[11] In response to intense pressure on sterling, and after vigorous denials that it would do so, the Conservative government abruptly left the European Union's Exchange Rate Mechanism (ERM) and devalued the pound on September 16, 1992. The crisis sharply eroded public confidence in Prime Minister Major and his government, while emboldening "Eurosceptics" in the Conservative caucus (Denver 1998, 17–20; see also Sanders 1995).

[12] Conservative divisions over Europe were widely publicized by the government's protracted difficulties in passing a bill implementing the Maastricht Treaty. After a year-long struggle, the bill received Commons approval on May 20, 1993 (Norton 1998, 82–4).

[13] John Smith, who had replaced Neil Kinnock as Labour leader after the 1992 general election, died suddenly in late May 1994, thus focusing public attention on Labour and the possibility that the party would select a popular figure such as Tony Blair as its new leader. Soon afterward, the June 1994 elections for the European Parliament provided further positive publicity, as Labour trounced the Tories by a 44% to 27% margin (Denver 1998, 37–8).

[14] The government's attempts to deal with the widely publicized problem, including initial denials of a health threat to humans and hollow threats to other EU countries for boycotting British beef, benefited Labour by making Prime Minister Major and his cabinet colleagues appear weak and indecisive (Denver 1998, 43–4).

[15] Immediately after the campaign began, the Conservatives were buffeted by a series of sex and bribery scandals. Two MPs resigned, but two others refused to do so (Whiteley 1997, 550).

[16] Under heavy fire from Eurosceptic opponents in his party, Prime Minister Major attempted to bolster his support by resigning as party leader in June 1995 and standing for reelection. When he defeated his Eurosceptic opponent, John Redwood, internecine squabbling in the party continued, but calls for his resignation ceased (Denver 1998, 24–6).

[17] The disclosure received wide publicity and earned Harman the appellation "Harriet Hypocrite" because she had contradicted her professed emphasis on the desirability of public education. Critics within the Labour Party used the episode to fuel their arguments that a New Labour government would abandon the party's historic egalitarian ideological goals.

[18] The Conservative campaign featured so-called Demon Eyes billboards, which displayed huge pictures of Tony Blair with glowing red eyes and a demonic grin. The picture was accompanied by the proclamation, "New Labour—New Danger." Subsequent advertisements had slogans such as "New Labour—New Taxes" and "New Labour—Euro Danger." At the same time, intraparty disputes surrounding the shadow cabinet election exposed policy/ideological fissures and personality conflicts in the Labour Party, and prompted speculation that these would be exacerbated should Labour form the next government. Similar conflicts emerged at the party's annual conference in October.

[19] Short-term effect dummies are coded +1 in the months when the event occurs, −1 in the next month, and 0 otherwise. Long-term effect dummies are coded +1 in the month when the event occurs, 0 otherwise. Given that the dependent variable (vote intentions) is first-differenced, long-term effects would be permanent except for the presence of the error-correction mechanism, which erodes these effects at a rate estimated by the coefficient for the error-correction mechanism term in the vote intention model. Since the dependent variables in the best prime minister and party identification models are differenced, and these models do not contain an error-correction mechanism, events coded 0, 1, 0 will exert permanent effects.

TABLE 3. Error-Correction Models of Labour Vote Intentions, January 1992–April 1997

	Models							
	Personal Prospective		Personal Retrospective		National Retrospective		National Prospective	
	β	s.e.	β	s.e.	β	s.e.	β	s.e.
Predictor Variables								
Constant	0.47	0.24*	0.47	0.25*	0.47	0.25*	0.45	0.25*
Best prime minister (*t*)	0.27	0.08***	0.29	0.08***	0.28	0.08***	0.29	0.08***
Labour party identification (*t*-2)	0.19	0.09*	0.16	0.09*	0.16	0.09*	0.16	0.09*
Error-correction mechanism (*t*-1)	−0.15	0.07*	−0.14	0.07*	−0.14	0.07*	−0.14	0.07*
Economic Evaluations								
Personal expectations (*t*)	−0.08	0.04*	X		X		X	
Personal retrospections (*t*)	X		−0.04	0.07	X		X	
National retrospections (*t*)	X		X		−0.02	0.03	X	
National expectations (*t*)	X		X		X		−0.02	0.02
Interest rates (*t*)	1.40	0.86	1.43	0.89	1.35	0.89	1.31	0.89
Most Important Problem								
Inflation (*t*-1)	0.55	0.14***	0.51	0.15***	0.50	0.15***	0.51	0.15***
Unemployment (*t*-1)	0.15	0.05**	0.15	0.05**	0.15	0.05**	0.15	0.05**
Maastricht (*t*)	2.58	1.35*	2.80	1.39*	2.75	1.39*	2.81	1.38*
Euro elections/Smith (*t*)	4.73	1.33***	4.61	1.37***	4.56	1.37***	4.54	1.37***
Major reselection (*t*)	−2.73	1.33*	−2.55	1.38*	−2.56	1.37*	−2.52	1.36*
Harman affair (*t*)	−3.13	1.31**	−2.99	1.37*	−2.95	1.35*	−2.97	1.35*
Mad cow (*t*)	1.63	1.33	1.26	1.35	1.31	1.35	1.26	1.35
Demon eyes/Shadow cabinet (*t*)	−4.44	1.38**	−4.19	1.42**	−4.20	1.41**	−4.21	1.41**
1996 Labour conference (*t*)	−3.93	1.84*	−3.92	1.91*	−3.99	1.90*	−4.00	1.90*
Conservative campaign scandals (*t*)	5.20	1.31***	5.21	1.35***	5.19	1.35***	5.32	1.35***
Model Diagnostics								
R^2	.75		.74		.74		.74	
Adjusted R^2	.67		.65		.65		.65	
Standard error of estimate	1.74		1.79		1.80		1.79	
Durbin-Watson	2.14		2.29		2.26		2.25	
Serial correlation (12 lags)	16.10		17.10		18.28		18.06	
Functional form	2.02		0.81		0.91		0.90	
Normality	4.65		3.24		3.39		3.72	
General heteroskedasticity	2.14		2.65		2.85		2.71	
ARCH (1)	0.07		0.22		0.29		0.19	
N	61		61		61		61	

X = variable not included in model.
Note: Dependent variable, Labour vote intentions, is first-differenced. *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$; one-tailed tests.

EVENTS = salient pro/anti-Labour events; ε_t = stochastic error process ($\sim N(0,\sigma^2)$); Δ = difference operator; β_0 = constant; and $\beta_{1\text{-}k}$ = regression coefficients. The error-correction mechanism (ECM) is measured as the residuals from the cointegrating regression, that is, $\text{ECM} = \text{LAB}_t - \alpha_1\text{LBPM}_t - \alpha_2\text{LPID}_t$. Although theory does not delineate the exact time lags at which various continuous variables influence Labour vote intentions, it is plausible that any delayed effects will occur at short lags. Thus, alternative preliminary specifications are assessed using contemporaneous (time *t*) effects and those operating at lags of 1 through 3 periods. Model parameters are estimated using OLS regression.

Preliminary analyses indicate that one of the shock variables, the currency crisis, did not have a significant effect on Labour vote intentions. Accordingly, this variable was omitted, and the four variants of the model were reestimated. The results (Table 3) show that, as hypothesized, both Labour leader as best prime minister and Labour party identification have significant short-term effects on Labour vote intentions. Considering the personal expectations version of the model, parameter estimates indicate that a 1% increase in the percentage of persons believing Tony Blair or his predecessors would make the best prime minister raises the party's vote intention share by .27%, whereas a 1% increase in the cohort of Labour party identifiers boosts it by .19%. When interpreting the substantive importance of these statistics, recall that both the percentage of persons believing that the Labour leader would make the best prime minister and the percentage of Labour identifiers rose substantially (by 31 and 17 points, respectively) during the period of interest. Ceteris paribus, the former increase is sufficient to boost the Labour vote intention share by 8.4% (.27 × 31), whereas the latter is enough to raise it by 3.2% (.19 × 17).

Regarding other predictors, economic variables in the personal expectations version of the model operate as anticipated, with Labour voting support varying positively with the percentages of persons mentioning inflation or unemployment as most important national problems, and inversely with the balance of positive

versus negative personal economic expectations. But, contrary to expectations, Labour electoral support is not associated with the salience of education and health care as national problems. As for various events, Major's reelection as party leader in July 1995 and the Harman affair in February 1996 each temporarily lowered Labour support by approximately 3%. Labour's internecine bickering during its shadow cabinet election and 1996 annual conference had slightly larger negative effects, with the latter persisting through the end of the period. Not all shocks were negative: Labour support was raised temporarily by more than 4%, as a result of the 1994 local elections/death of John Smith; by nearly 2% due to the 1996 Mad Cow scare, and by more than 5% owing to sleaze allegations against Tory MPs.

The error-correction mechanism also is statistically significant, and consistent with the idea that it is a reequilibrating or negative feedback device, its coefficient is negative. Its modest size (e.g., −.15 in the personal expectations model) indicates that factors affecting Labour voting support could have a relatively long "half-life." Ceteris paribus, their influences were eroded by the long-term cointegrating relationship among voting support, best prime minister, and party identification at a rate of 15% in each subsequent month after they occurred. For example, consider the 3.93-point negative shock to Labour's vote intention share that occurred in October 1996 (refer to Table 3). That shock cost Labour 3.34 points the next month (3.93 − (3.93 × .15)) and 2.84 points two months later (3.34 − (3.34 × .15)).[20] The slow decline continued, such that six months after the shock occurred, on the eve of the May 1997 general election, its effects were still operative, reducing the Labour vote by 1.48 points. If the initial effect had been larger, the delayed effects would have been as well. For instance, a ten-point negative shock to Labour support in October 1996 would have cost the party 4.4 points the following April.

Viewed more generally, the personal expectations model of Labour vote intentions performs quite well. As Table 3 shows, the model has a reasonably good fit (adjusted $R^2 = .67$, *see* = 1.74), and tests for residual autocorrelation, heteroskedasticity, functional form, and normality have acceptable, that is, statistically insignificant ($p > .05$), values. Additional diagnostics focusing on the estimates of the long-run effects of the best prime minister and party identification variables[21] and a change in survey methods in January 1997[22] bolster confidence in the model.

The attractiveness of the personal expectations model is further enhanced by the finding that none of the other three types of subjective economic evaluations has a statistically significant effect (refer to Table 3, personal retrospections, national retrospections, and national expectations models). Moreover, although these rival specifications do well in terms of the behavior of most other individual predictors, overall fit, and residual diagnostics, the superiority of the personal expectations variant is suggested by its performance on a battery of encompassing tests (Hendry 1995, chapter 14; see also Charemza and Deadman 1997, chapter 8). The intuition motivating these tests is that if a model is superior to (nonnested) alternatives, it should explain all the features of the data that the alternatives explain and additional ones as well. If a model is able to accomplish this task, then it is said to encompass its rivals. Here, encompassing tests[23] indicate that the personal retrospections, national retrospections, and national expectations models are encompassed by the personal expectations model, but do not encompass it (at the .01 level; data not shown). Overall, these results, unlike those reported in recent studies of Conservative support (Clarke and Stewart 1995; Clarke, Stewart, and Whiteley 1997), are consonant with the Sanders (1991, 1993) thesis that personal economic expectations deserve "pride of place" in vote intention models.

BEST PRIME MINISTER AND PARTY IDENTIFICATION

To untangle the skein of forces affecting the short- and long-term dynamics of Labour vote intentions, we next consider the determinants of perceptions of the Labour leader as making the best prime minister and Labour party identification. We again investigate the effects of different economic evaluations in four alternative model specifications. Other predictor variables in the best prime minister models include party identification, interest rates, perceptions of inflation, unemployment, education and health care as most important national problems, and various salient events. Since Labour has had three leaders during the period under consideration, two of these latter variables are designed to capture public reactions to the two leadership changes (from *Kinnock to Smith* in July 1992 and from *Smith to Blair* in July 1994). Other event variables in the best

[20] These are direct, not total, effects; the October 1996 shock also had an indirect influence on Labour vote intentions via its influence on perceptions that Tony Blair would make the best prime minister and Labour party identification.

[21] When all parameters in the error-correction models were estimated in a single step (see Banerjee et al. 1993, chapter 7; Beck 1993), the results are very similar to those in tables 2 and 3. For example, in the personal expectations version, the one-step coefficients for the long-run effects of the best prime minister and party identification variables are .773 and .533, respectively, and the adjustment coefficient for the error-correction mechanism is −.148. One cannot reject the null hypothesis that these coefficients are equal to those estimated by the two-step method ($\chi^2 = 3.51$, $df = 3$, $p = .320$). Also, one cannot reject the more general null hypothesis that all coefficients estimated by the one-step procedure are equal to their two-step counterparts ($\chi^2 = 4.00$, $df = 17$, $p = .999$).

[22] Gallup switched from in-person interviews with quota samples to telephone interviews with respondents selected by a random-digit dialing technique in January 1997. To ascertain whether the change was consequential, the four vote-intention models were reanalyzed, including a dummy variable scored 1 for the January–April 1997 period, 0 otherwise. In all cases, the coefficient for this variable was small (<.50) and insignificant, and all other coefficients and standard errors remained very similar to those presented in Table 3. The models also were reestimated terminating the time series in December 1996; again, changes in the estimates were trivial.

[23] For details regarding these tests, see Pesaran and Pesaran (1997, 355–7).

TABLE 4. Models of Labour Leader as Best Prime Minister, May 1992–April 1997

	Models							
	Personal Prospective		Personal Retrospective		National Retrospective		National Prospective	
	β	s.e.	β	s.e.	β	s.e.	β	s.e.
Predictor Variables								
Constant	−0.03	0.29	0.12	0.31	0.15	0.29	0.00	0.31
Labour party identification (*t*-2)	0.25	0.11**	0.20	0.11*	0.19	0.10*	0.18	0.11
Economic Evaluations								
Personal expectations (*t*)	−0.18	0.06**	X		X		X	
Personal retrospections (*t*)	X		−0.21	0.09*	X		X	
National retrospections (*t*)	X		X		−0.13	0.04**	X	
National expectations (*t*)	X		X		X		−0.06	0.03*
Most Important Problem								
Inflation (*t*-1)	0.34	0.16*	0.30	0.17*	0.25	0.16	0.28	0.17
Unemployment (*t*-1)	0.08	0.06	0.08	0.06	0.09	0.06	0.08	0.07
Education (*t*-3)	0.20	0.14	0.18	0.15	0.16	0.14	0.14	0.15
Health (*t*-2)	0.28	0.13*	0.12	0.14	0.18	0.13	0.25	0.14*
Postelection antihoneymoon (*t*)	−5.02	1.72**	−5.73	1.79**	−3.97	1.80*	−5.12	1.86**
Currency crisis (*t*)	5.49	2.22**	5.35	2.37*	4.54	2.27*	6.13	2.35**
Maastricht (*t*)	2.60	1.61	2.70	1.70	2.28	1.62	3.10	1.70*
Leader Change								
Kinnock to Smith (*t*)	3.79	2.16*	2.18	2.50	2.76	2.21	3.78	2.33
Smith to Blair (*t*)	14.85	2.22***	13.93	2.29***	14.45	2.18***	13.81	2.33***
Major reselection (*t*)	−5.83	2.20**	−5.44	2.34**	−5.55	2.20**	−6.09	2.35**
Harman affair (*t*)	−4.80	1.52**	−5.11	1.61**	−4.71	1.51**	−4.81	1.62**
Mad Cow (*t*)	3.24	1.64*	3.32	1.74*	3.04	1.63*	2.70	1.75
Demon eyes/Shadow cabinet (*t*)	−7.72	1.61***	−6.82	1.70***	−6.81	1.60***	−7.45	1.71***
1996 Labour conference (*t*)	−4.83	2.17*	−4.61	2.30*	−4.46	2.17*	−5.15	2.30*
Model Diagnostics								
R^2	.77		.74		.77		.74	
Adjusted R^2	.68		.65		.69		.64	
Standard error of estimate	2.12		2.22		2.11		2.26	
Durbin-Watson	2.45		2.54		2.36		2.41	
Serial correlation (12 lags)	20.04		23.87		18.95		22.68	
Functional form	0.06		0.25		0.52		0.02	
Normality	1.13		0.77		0.24		0.04	
General heteroskedasticity	1.98		2.60		2.33		2.09	
ARCH (1)	0.28		0.53		0.01		0.32	
N	60		60		60		60	

X = variable not included in model.
Note: Dependent variable, Labour leader as best prime minister, is first-differenced. *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$; one-tailed tests.

prime minister models include the currency crisis (permanent effect), John Major's reselection as Tory leader (permanent effect), a postelection "*Antihoneymoon*" for Neil Kinnock, who led the party to defeat in 1992 (temporary effect), the Harriet Hypocrite and Mad Cow affairs (temporary effects), the shadow cabinet and party conference bickering (temporary and permanent effects, respectively), and the Conservative sleaze revelations (temporary effect). Since the Labour leader as best prime minister and Labour party identification series are judged to be integrated (I[1]), they are analyzed in first-differenced form.

Preliminary analyses indicate that interest rates and Tory sleaze do not have significant effects in the best prime minister models. Models omitting these variables perform well—they explain substantial amounts of the variance in the dependent variable (adjusted R^2 ranges from .65 to .69), and they pass the extensive battery of residual diagnostic tests (Table 4). Several predictor variables perform as advertised. The leadership change from Smith to Blair was particularly profound: Net of all other considerations, the selection of Tony Blair prompted a permanent 13–15% upward shift in the percentage of persons believing the Labour leader would make the best prime minister. The currency and Maastricht crises and the Mad Cow scare had smaller positive effects (2–6% in each case). Several negative shocks also are significant: The party's 1992 election defeat, the Harriet Hypocrite affair, and the shadow cabinet squabbling all prompted one-month decreases ranging in size from 4 to 8%. Major's reelection as Tory leader and negative publicity at the time of the 1996 party conference both exerted permanent negative effects of 4 to 5%.

Subjective economic evaluations also work as anticipated in the best prime minister models; as these evaluations become more sanguine, the percentage of voters selecting the Labour leader as best prime minister decline. Unlike the Labour vote intention analyses, all four types of economic evaluations have signif-

TABLE 5. Models of Labour Party Identification, April 1992–April 1997

	Models							
	Personal Prospective		Personal Retrospective		National Retrospective		National Prospective	
	β	s.e.	β	s.e.	β	s.e.	β	s.e.
Predictor Variables								
Constant	0.15	0.29	0.18	0.30	0.18	0.30	0.16	0.30
Economic Evaluations								
Personal expectations (*t*)	−0.11	0.05*	X		X		X	
Personal retrospections (*t*)	X		−0.09	0.08	X		X	
National retrospections (*t*)	X		X		−0.02	0.04	X	
National expectations (*t*)	X		X		X		−0.00	0.03
Interest rates (*t*-2)	1.62	1.02	1.64	1.06	1.55	1.07	1.50	1.07
Most Important Problem								
Unemployment (*t*-1)	0.17	0.05***	0.17	0.06**	0.17	0.06**	0.16	0.06**
Education (*t*)	0.29	0.15*	0.24	0.15	0.26	0.16	0.24	0.16
Health (*t*-1)	0.24	0.12*	0.30	0.12**	0.28	0.12*	0.28	0.12*
Currency crisis								
(*t*)	8.03	2.14***	8.42	2.21***	8.40	2.27***	8.67	2.24***
(*t*-3)	4.33	2.39*	3.59	2.43	3.40	2.49	3.14	2.48
Maastricht (*t*-1)	2.81	1.50*	2.74	1.55*	2.81	1.57*	2.77	1.59*
Major reselection (*t*-1)	−4.05	2.14*	−4.19	2.22*	−4.47	2.24*	−4.64	2.24*
Euro elections/Smith (*t*)	3.44	1.54*	3.18	1.58*	3.02	1.60*	2.99	1.60*
Harman affair (*t*)	−5.06	1.61***	−4.76	1.66**	−4.64	1.68**	−4.57	1.68**
Mad Cow (*t*)	3.59	1.53**	3.23	1.58*	3.26	1.60*	3.23	1.60*
Demon eyes/Shadow cabinet (*t*)	−2.68	1.52*	−2.49	1.57	−2.46	1.60	−2.56	1.60
1996 Labour conference (*t*)	−4.85	2.14*	−4.87	2.24*	−5.14	2.26*	−5.34	2.25**
Model Diagnostics								
R^2	.62		.59		.58		.58	
Adjusted R^2	.50		.46		.45		.45	
Standard error of estimate	2.09		2.16		2.19		2.20	
Durbin-Watson	2.56		2.61		2.61		2.60	
Serial correlation (12 lags)	25.04		27.00		28.25		28.07	
Functional form	0.15		0.71		0.33		0.49	
Normality	0.47		2.58		3.62		1.43	
General heteroskedasticity	3.05		2.93		3.42		3.53	
ARCH (1)	0.02		0.51		0.55		0.34	
N	61		61		61		61	

X = variable not included in model.
Note: Dependent variable, Labour party identification, is first-differenced. *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$; one-tailed tests.

icant effects on best prime minister perceptions (Table 4). Encompassing tests reveal that the personal expectations model encompasses ($p < .05$) the personal retrospections and national expectations alternatives (data not shown). The personal expectations model does not fare as well against the national retrospections model, however, with most encompassing tests indicating that neither of these models is superior to its competitor. Thus, the evidence for personal expectations versus other types of economic evaluations in the best prime minister analyses is weaker than in the vote intention ones.

Variables in the party identification models are similar to those used in the vote intention and best prime minister ones, and once more we employ the four types of economic evaluations in separate analyses. Preliminary soundings indicate that perceptions of inflation as the most important problem and some events do not have significant effects. Reestimating the models without these variables reveals that the only significant subjective economic evaluation is personal expectations. As anticipated, its effect on Labour partisanship is negative (−.11) (Table 5, personal expectations model). In contrast, interest rates and perceptions of unemployment, education, and health care as most urgent problems have the expected positive influences on the Labour identifier share; as rates increase and worries about joblessness, education, and health care grow, so does the size of the Labour partisan cohort. Perhaps most noteworthy is the large influence of the currency crisis, the immediate and lagged effects (see Sanders 1995) of which prompted an 11–12% increase in Labour party identification. Labour also benefited from the Tories' divisions over Europe (by 2–3%), the 1994 European elections coupled with the untimely death of John Smith (by 3+%), and the Mad Cow incident (by 3–4%). Negative influences on Labour partisanship include the Harman affair (a temporary decrease of 4–5%), the shadow cabinet squabbling (a temporary decrease of 2–3%), and the 1996 annual conference (a permanent decrease of 4–5%). Major's reelection as Tory leader was influential as well—occasioning a permanent 4–5 point decrease in the percentage of Labour identifiers.

Overall, the party identification models confirm the story conveyed visually by Figure 3: The upward movements in Labour partisanship after the 1992 election were both sizable and occasionally "lumpy," with the largest increases coming in the wake of highly publicized events (especially the currency crisis) that damaged the credibility of the governing Conservatives in the public mind. The party identification models also strengthen the case for personal economic expectations. These are the only type of economic evaluations to achieve statistical significance in these models, and encompassing tests indicate that the personal expectations variant encompasses its three rivals, but they do not encompass it (data not shown). The personal economic expectations coefficient (−.11) is not large in comparison with those for events such as the currency crisis, but when one considers the extent of variability in such expectations, the effect appears more impressive. Between January 1992 and April 1997, personal economic expectations varied by 44 points—ceteris paribus, this is sufficient to shift the Labour party identifier share by nearly 5%. Movements in Labour partisanship thus were produced by a combination of major events and less spectacular, but influential, ongoing changes (both positive and negative) in personal economic expectations.

Collectively, the models presented above tell a plausible story about the dynamics of Labour support after 1992. The plot is simple and straightforward, focusing on party identification and party leader evaluations as major players in the unfolding drama of Labour's impending electoral resurgence. But is it not possible that there are simultaneous causal linkages among these variables that would render model estimates suspect (biased and inconsistent)? Stated technically, can we demonstrate that Labour party identifications and judgments that the Labour leader would make the best prime minister are weakly exogenous to Labour vote intentions? Such a demonstration provides warrant for our causal inferences. Recall that a variable X_t is weakly exogenous to another variable Y_t if the latter affects the former only with a lag of at least one period (e.g., Charemza and Deadman 1997, chapter 7; see also Ericsson and Irons 1994). If Y_t does not have a contemporaneous or lagged effect on X_t, that is, if there is both weak exogeneity and Granger noncausality, then one can say that X_t is strongly exogenous to Y_t. Strong exogeneity is not required for inferences about the effect of X_t on Y_t.

As Charemza and Deadman (1997, 228–30) observe, the test for weak exogeneity in an error-correction model specification has two steps. In the present context, one must first demonstrate that the error-correction mechanism is statistically insignificant in the models for best prime minister and Labour party identification. Then, one must show that the residuals for the latter models (from analyses that omit the error-correction mechanism) are insignificant in the model for Labour vote intentions. Empirically, the models pass both tests. Testing at the .05 level of probability, we find that the t-ratios for the error-correction mechanism in the personal expectations variant of the best prime minister and Labour party identification models are −0.23, and 1.27, respectively. When the residuals from the best prime minister and Labour party identification models (absent the error-correction mechanism) are included as additional predictors in the Labour vote intention specification, they have t-ratios of .90 and 1.20, respectively. Statistically insignificant results also obtain in tests for the models using the other three types of subjective economic expectations. We therefore conclude that the best prime minister and party identification variables are weakly exogenous to vote intentions, and the inferences drawn from our Labour support analyses are warranted.[24]

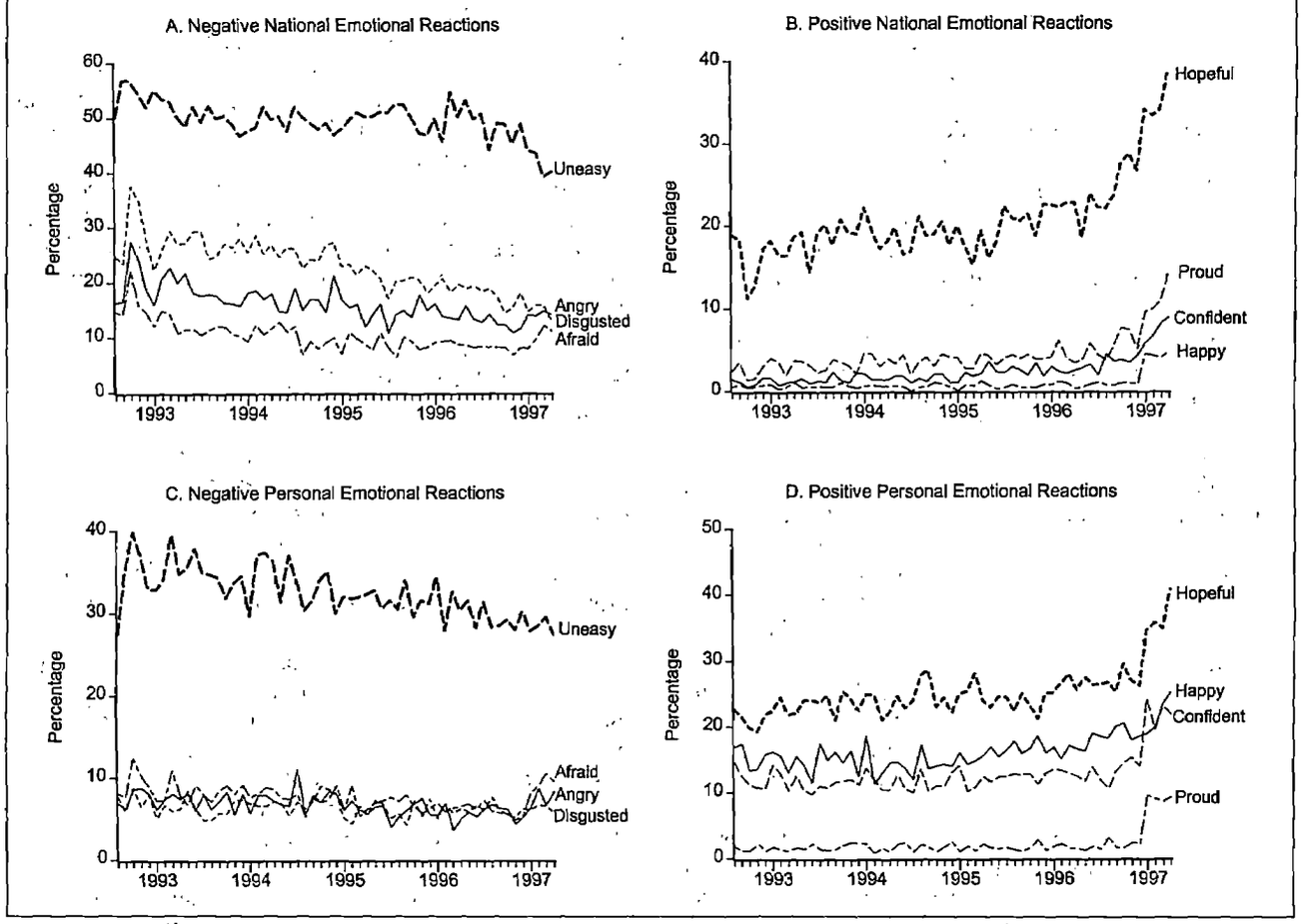
EMOTIONAL REACTIONS TO ECONOMIC CONDITIONS

As observed earlier, objective economic indicators, notably inflation and unemployment rates, traditionally have been accorded the status of prime movers in party support models. In recent years these variables have had to share space on the right-hand side of these models with measures of subjective economic evaluations. Although the latter have helped to articulate aggregate party support models with the microanalytical literature on factors affecting voting behavior and election outcomes, affective or emotional reactions to personal and national economic circumstances remain missing ingredients of model specification. Data on these reactions are now available, albeit for a slightly shorter (August 1992–April 1997) time frame than that considered above.

The monthly data show that British voters' emotional reactions to the national economy more often have been negative than positive since August 1992. The dominant negative emotion expressed is *Uneasiness*, which overshadows feelings of *Anger*, *Disgust*, and *Fear* in every survey (Figure 5A). For example, at the time of the currency crisis in September 1992, 57% reported feeling uneasy about the country's general economic situation. In the most recent (April 1997) survey, 44% said that they felt uneasy, whereas 14%, 13%, and 11% said that they were angry, disgusted, and afraid, respectively. With regard to positive emotional reactions to the national economy, respondents typically report feelings of *Hope* rather than *Confidence*, *Happiness*, or *Pride*. Moreover, as Figure 5B shows, hopeful feelings nearly doubled over the several months preceding the general election, climbing from 22% in August 1996 to 39% in April 1997. Other positive emotions about the national economy also

[24] Recent debates concerning the exogenous status of economic expectations in models of presidential approval (MacKuen, Erikson, and Stimson, 1996; Norpoth 1996a, 1996b) prompted us to conduct additional exogeneity tests for personal economic expectations in the Labour vote intention, Labour leader as best prime minister, and Labour party identification models. Exogeneity tests also were conducted for national economic retrospections in the best prime minister model. These tests required the development of models for these two types of economic evaluations. In all cases, exogeneity test statistics are insignificant ($p > .05$).

FIGURE 5. Emotional Reactions to National and Personal Economic Conditions, August 1992–April 1997

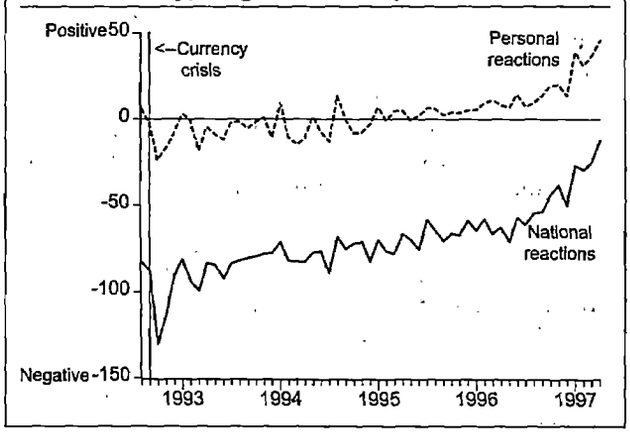


became more common during this period but remained at much lower levels. In April 1997, 15%, 9%, and 5%, respectively, claimed to be confident, happy, or proud about Britain's economic performance.

The finding that sociotropic evaluations tend to be less sanguine than egocentric ones (Clarke and Stewart 1995) has its counterpart in emotional reactions. The mean percentage of persons reporting uneasiness about the country's economic condition is 50% (range: 39–57%), whereas the mean percentage expressing unease about personal economic circumstances is 32% (range: 27–40%) (Figure 5C and 5D). Positive emotions display the opposite pattern; for example, the mean percentage stating they were hopeful about the national economy is 20% (range: 11–39%), with the comparable percentage for one's personal economic situation being 25% (range: 19–41%). Figure 6 summarizes the *Balance of Positive and Negative Emotional Reactions to National and Personal Economic Conditions* over the August 1992–April 1997 period. As illustrated, personal reactions were relatively more positive than national ones, with the overall balance of positive to negative emotions about the national economy consistently being in the negative zone (i.e., less than zero). There is no strong tendency, however, for the aggregate balance of emotional reactions to personal economic conditions to be positive; between August 1992 and April 1997 it hovered near the neutral point (zero) in most months and was in the negative zone in 24 of 57 instances.

Finally, note the sharp downward movements in the two summary variables at the time of the currency crisis

FIGURE 6. Balance of Positive and Negative National and Personal Emotional Reactions to the Economy, August 1992–April 1997



and their upward surges in the months preceding the 1997 election. These dynamics accord well with the argument that emotional reactions to national and personal economic conditions are potentially quite mutable (Marcus and MacKuen 1993, 675, 681; see also Sniderman, Brody, and Tetlock 1991, 7). Rather than being wholly governed by stable personality traits generating generalized positive or negative feelings, such reactions reflect dramatic, quickly unfolding, events (e.g., the currency crisis) as well as more slowly paced changes in the objective economy (e.g., the upturn in the British economy occurring in the year before the 1997 election).[25]

Do emotional reactions to the economy provide leverage for explaining the dynamics of party support beyond that supplied by the several variables included in the models considered above? To answer this question, we incorporate the two summary indices of the balance of emotional reactions to national and personal economic conditions in models of Labour vote intentions, Labour leader as best prime minister, and Labour party identification. Given the relatively strong performance of personal economic expectations in the earlier analyses, we confine attention to models that include this variable. Since the emotional reactions series begin in August 1992, we consider the 57-month period from that date to April 1997, with the regression analyses covering slightly shorter time spans because of the use of differenced and lagged variables. Consistent with the idea that psychological variables should act either contemporaneously or with short lags, alternative specifications are entertained for emotional reactions operating contemporaneously and with lags of one to three periods.

The results for the Labour vote intention models are disappointing; neither national nor personal emotional reaction to economic conditions achieves statistical significance (data not shown). The situation is different, however, for judgments that a Labour leader would make the best prime minister. Preliminary analyses indicate that perceptions of unemployment and inflation as most urgent national problems do not have significant effects in models in which the emotional reactions variables are present. Estimates for a best prime minister model that omits these variables are presented in Table 6. As shown, the balance of emotional reactions to national economic conditions has a properly signed (negative) and statistically significant ($p < .05$) effect, indicating that increasingly negative emotional reactions to the national economy are associated with a growing tendency to judge that the leader of the (opposition) Labour Party will make the best prime minister. In addition, the variables which were significant in the specification that did not incorporate national emotional reactions remain so, with most parameter estimates being similar to those reported above (refer to Table 4). Confidence in the national emotional reactions model is enhanced by the observations that model fit is quite good (adjusted $R^2 = .69$, *see* = 2.05) and all residual diagnostic test statistics have satisfactory, that is, statistically insignificant, values. The analysis also shows that emotional reactions to personal economic conditions do not fare as well. Although personal emotional reactions have a significant effect ($-.05$, $p \leq .05$) when national emotional reactions are omitted from the best prime minister model (data not shown), personal reactions are not significant when national reactions are included (Table 6).

Analyses of Labour party identification also reveal that emotional reactions to economic conditions are influential (Table 7). The coefficient measuring the effect of emotional responses to the national economy is slightly larger than the one for personal financial circumstances ($-.06$, $p \leq .05$ versus $-.05$, $p \leq .10$), and encompassing tests indicate the superiority of the model that includes the former variable. Moreover, similar to the best prime minister analyses, inspection of these party identification models shows that virtually all variables which exert significant and properly signed effects in the analyses that do not include the emotional reactions variables continue to do so when these variables are added (compare Table 5, personal expectations model, and Table 7). With the sole exception of one general test for heteroskedasticity ($p \leq .05$),[26] all fit statistics and model diagnostics have satisfactory values.

In sum, these analyses reveal that emotional reactions to economic circumstances influence aggregate

[25] Modeling emotional reactions to national economic conditions as a function of personal economic expectations, interest rates, unemployment rates, the currency crisis, and a first-order moving average error process (as suggested by preliminary diagnostics) yields the following results:

$$\Delta BALNAT_t = .02 + .73\Delta FINANFT_t - 7.76\Delta INTR_{t-2} - 13.89\Delta UNEMP_{t-1} - 27.00BLACK_t + \varepsilon_t - .60\varepsilon_{t-1} \quad \bar{R}^2 = .56$$
$$(.79)(.18) \qquad (2.70) \qquad (7.54) \qquad (7.51) \qquad (.12)$$

where BALNAT = balance of positive and negative emotional reactions to national economic conditions, FINANFT = personal economic expectations, INTR = interest rates, UNEMP = unemployment rate, BLACK = currency crisis dummy, Δ = difference operator, and ε = error term ($\sim N(0,\sigma^2)$). These estimates (standard errors in parentheses) show that the currency crisis, interest rates, unemployment rates, and the currency crisis shock all have hypothesized statistically significant, negative effects on emotional reactions to the national economy. Personal economic expectations have the expected, significant, positive effect. A comparable model of emotional reactions to personal economic conditions shows that, although the currency crisis does not have a significant influence net of other factors, interest rates and unemployment rates have the anticipated negative effects, and personal economic expectations have the expected positive effect.

$$\Delta BALPER_t = -.67 + .58\Delta FINANFT_t - 3.92\Delta INTR_{t-2} - 15.68\Delta UNEMP_{t-1} - 6.16\,BLACK_t + \varepsilon_t - .83\varepsilon_{t-1} \quad \bar{R}^2 = .48$$
$$(.48)(.15) \qquad (1.67) \qquad (4.98) \qquad (7.21) \qquad (.09)$$

where BALPER = balance of positive and negative emotional reactions to personal economic conditions, and other variables are as described above.

[26] The general heteroskedasticity test reported is that suggested by Breusch and Pagan (1979). An alternative test (White 1980) yields insignificant results for this analysis and all others reported in the article.

TABLE 6. Model of Labour Leader as Best Prime Minister with National and Personal Emotional Reactions, October 1992–April 1997

	β	s.e.
Predictor Variables		
Constant	0.01	0.29
Labour party identification (*t*-2)	0.20	0.10*
Economic Evaluations		
Personal expectations (*t*)	−0.12	0.05*
Most Important Problem		
Education (*t*-3)	0.22	0.14
Health (*t*-2)	0.29	0.13*
Currency crisis (*t*)	6.03	2.15**
Maastricht (*t*)	2.60	1.57*
Leader Change		
Smith to Blair (*t*)	13.53	2.18***
Major reselection (*t*)	−5.77	2.13**
Harman affair (*t*)	−5.13	1.48***
Mad Cow (*t*)	2.86	1.61*
Demon eyes/Shadow Cabinet (*t*)	−7.79	1.53***
1996 Labour conference (*t*)	−5.29	2.09**
Emotional Reactions		
National (*t*-1)	−0.08	0.04*
Personal (*t*-1)	0.01	0.04
Model Diagnostics		
R^2	.77	
Adjusted R^2	.69	
Standard error of estimate	2.05	
Durbin-Watson	2.36	
Serial correlation (20 lags)	21.99	
Functional form	0.31	
Normality	0.10	
General heteroskedasticity	1.07	
ARCH (1)	0.01	
N	55	

Note: Dependent variable, Labour leader as best prime minister, is first-differenced. *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$; one-tailed tests.

TABLE 7. Model of Labour Party Identification with National and Personal Emotional Reactions, November 1992–April 1997

	β	s.e.
Predictor Variables		
Constant	0.30	0.31
Economic Evaluations		
Personal expectations (*t*)	−0.17	0.06**
Interest rates (*t*-2)	2.51	1.09*
Most Important Problem		
Unemployment (*t*-1)	0.18	0.06**
Education (*t*)	0.24	0.15
Health (*t*-1)	0.23	0.12*
Currency crisis (*t*-3)	7.07	2.64**
Maastricht (*t*-1)	2.75	1.51*
Major reselection (*t*-1)	−3.93	2.13*
Euro elections/Smith (*t*)	4.12	1.58**
Harman affair (*t*)	−4.78	1.61**
Mad Cow (*t*)	3.73	1.52**
1996 Labour conference (*t*)	−4.24	2.15*
Emotional reactions		
National (*t*-2)	−0.06	0.03*
Personal (*t*-1)	−0.05	0.03
Model Diagnostics		
R^2	.64	
Adjusted R^2	.50	
Standard error of estimate	2.08	
Durbin-Watson	2.46	
Serial correlation (20 lags)	23.47†	
Functional form	0.00	
Normality	0.78	
General heteroskedasticity	5.06	
ARCH (1)	0.17	
N	55	

Note: Dependent variable, Labour party identification, is first-differenced. *$p \leq .05$; **$p \leq .01$; ***$p \leq .001$; one-tailed tests. †$p \leq .05$; two-tailed test.

movements in perceptions of the Labour leader and Labour partisanship net of several other variables that have played prominent roles in previously articulated models of party support. Since analyses presented above demonstrate that judgments about Labour leaders' prime ministerial qualifications and Labour partisanship have important short- and long-term influences on Labour vote intentions, one may conclude that emotional responses to the economy exert indirect effects on the party's electoral support.

NEW MODELS FOR NEW LABOUR (AND OTHER PARTIES)

This article has considered four longstanding, unresolved, debates in the literature on the political economy of party support. One of these controversies concerns the nature and dynamics of party identification in contemporary mature democracies. The finding of sizable aggregate-level movements in party identification in Britain over the 1992–97 period accords well with a conceptualization of party identification as a summary of party performance evaluations that is updated over time as new information becomes available. Since recent evaluations may strongly contradict earlier ones, partisan attachments are potentially mutable factors in the set of forces that determine parties' electoral fortunes. The factors that cause change in party identification need not be cataclysmic; party identification in present-day Britain is influenced by both quotidian and extraordinary events and conditions.

A second debate concerns the influence of party leader image on electoral choice. Conventional wisdom concerning the unimportance of British party leaders notwithstanding, leader effects are important—there is a cointegrating relationship among perceptions of Labour leaders' prime ministerial qualifications, Labour party identification, and Labour vote intentions, and the former two variables exert significant long- and short-run effects on the latter. These findings testify to the key role that leader image and party identification play in the matrix of forces affecting parties' electoral support, and they suggest that aggregate-level vote intention models which omit leader image and party identification variables run serious risks of misspecification.

The third and fourth debates focus on how the

economy affects party and party leader support. Consonant with the tenets of the influential Essex model, personal economic expectations exercise significant influences on Labour vote intentions and Labour party identification, but other types of economic evaluations do not affect either of these variables. However, the case for personal economic expectations is not as clear-cut in analyses of which party leader would make the best prime minister. In these analyses retrospective judgments about the national economy perform as well as personal expectations. That the party leader analyses do not make a compelling case for personal expectations indicates that future studies should distinguish carefully between party and party leader support when testing hypotheses about the importance of various kinds of economic evaluations.

Economic evaluations are not all that matter, however. Contrary to the image of voters in the rational choice literature as coldly calculating utility maximizers, Labour's share of party identifiers and judgments that the party's leader would make the best prime minister reflect emotional reactions to national economic conditions. These reactions thus indirectly affected Labour vote intentions during the 1992–97 period preceding the party's dramatic return to power. Gauging the effect of emotional reactions to economic conditions in other political contexts is an important topic for future inquiry.

In conclusion, this article has two general messages concerning future research on the dynamics and determinants of public support for political parties. First, basing model specification on what data exist rather than what data are needed precludes testing rival models that address unresolved theoretical controversies over the forces shaping party support. Testing such models will require substantial and careful data-gathering efforts. These efforts can be expensive, and they require patience, but the alternative is the continued absence of time-series data on key variables. Second, more attention should be paid to important advances that have been made in the methods for analyzing time-series data. Particularly promising are procedures for investigating integrated and long-memoried processes. These procedures, in conjunction with others such as encompassing and exogeneity tests, provide significant leverage for delineating the short- and long-run determinants of party support. Used together, new data and new methods will do much to advance understanding of the processes that govern electoral choice and party fortunes in contemporary democracies.

APPENDIX: SURVEY QUESTIONS AND VARIABLES

Question batteries were included on monthly Gallup surveys conducted over the January 1992–April 1997 period using research funds provided by National Science Foundation (grants SES-9309018 and SES-9600018). The fieldwork was conducted under the direction of Robert Wybrow, Rory Fitzgerald, and Sheri Weber. Variables based on the survey questions include the following.

Labour Vote Intentions

(a) "If there were a General Election tomorrow, which party would you support?" (b) [If "don't know"] "Which party would you be most inclined to vote for?" Labour voting support in the electorate is calculated as the sum of the percentages of persons responding "Labour" to (a) or (b), with refusals and declared nonvoters excluded.

Labour Leader as Best Prime Minister

This variable is the percentage mentioning a Labour leader in response to the question: "Who would make the best Prime Minister, Mr. Major, Mr. Kinnock/Smith/Blair, or Mr. Ashdown?"

Labour Party Identification

This variable is measured as the percentage responding "Labour" to the question: "Generally speaking, do you think of yourself as Conservative, Labour, Liberal Democrat, or what?" The SNP and Plaid Cymru also are mentioned in Scotland and Wales, respectively.

Subjective Economic Evaluations

The variables are constructed by subtracting the percentage offering negative responses from the percentage offering positive ones to the following questions. (a) Personal expectations: "How do you think the financial situation of your household will change over the next 12 months?" (b) Retrospective evaluations of personal conditions: "How does the financial situation of your household now compare with what it was 12 months ago?" (c) National expectations: "How do you think the general economic situation in this country will develop over the next 12 months?" (d) Retrospective evaluations of national conditions: "How do you think the general economic situation in this country has changed over the last 12 months?" Response categories for these questions are: "get/got a lot better," "get/got a little better," "stay/stayed the same," "get/got a little worse," "get/got a lot worse."

Most Important Problems

Levels of public concern about inflation, unemployment, education, and health care are the percentages mentioning these issues in response to the question: "What would you say is the most urgent problem facing the country at the present time?"

Emotional Reactions to National and Personal Economic Conditions

National conditions: (a) "Please look at this card and tell me if any of the words on it [uneasy, hopeful, disgusted, angry, afraid, confident, happy, proud] describes your feelings about the country's general economic situation. [If yes] Which ones?" *Personal conditions*: (b) "Please look at this card again and tell me if any of the words on it describes your feelings about the financial condition of your household. [If yes] Which ones?" These questions were asked in January 1992 and in each of the August 1992–April 1997 surveys.

Mentions of "anger," "disgust," "fear," and "uneasy" are classified as indicating negative emotional reactions, and mentions of "confidence," "happiness," "pride," and "hopeful" as indicating positive emotional reactions. This positive-

negative classification is similar to the factor structures reported by Conover and Feldman (1986) and Marcus (1988). See also Marcus and MacKuen (1993). The aggregate summary measures of emotional reactions to national and personal economic conditions are constructed by summing the percentages of positive and negative reactions, and subtracting the latter from the former.

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How Does Cabinet Instability Affect Political Performance? Portfolio Volatility and Health Care Cost Containment in Parliamentary Democracies

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This article explores the relationship between cabinet instability and political performance in parliamentary democracies. I develop two theoretical arguments about how cabinet instability should affect government effectiveness, and I use these to define several measures of instability. The first argument suggests that instability in the partisan composition of cabinets should make it difficult for governments to adopt and implement new policy programs. The second argument suggests that instability in the partisan control of portfolios within the government (portfolio volatility) should make it difficult for cabinet ministers to obtain relevant information during policy formulation and implementation. I test both arguments by examining the short- and long-run effect of the instability variables on success at health care cost containment. The analysis indicates that short-run increases in portfolio volatility present problems for government decision makers, but in the long run, unstable systems are able to address the problems that instability poses.

Does cabinet instability influence political performance in parliamentary democracies? If so, how does this occur? Previous research does not answer these questions because existing studies do not systematically examine instability as an *explanatory* variable. Instead, scholars have focused principally on instability as the variable to be explained (e.g., Laver and Schofield 1990; Powell 1982; Warwick 1979, 1994).

Studies of cabinet instability generally proceed by identifying the class of events that can be coded as government terminations, measuring government duration as the time between such events, and identifying factors that explain variation in government duration. Although this literature clearly identifies factors affecting government stability, it suffers from the fact that the first step in the process—identification of government terminations—typically proceeds with little or no theoretical foundation. Consequently, the events that are coded as government terminations are very diverse. The following cases are examples: (1) a single-party majority government replaces its prime minister; (2) a single-party majority government is replaced by a different majority party; (3) a coalition government of three parties resigns, and two of the parties reenter the government along with a new third party; (4) a coalition government loses a vote of confidence, but the same parties form a new government after reshuffling portfolios; (5) a "surplus" party leaves an oversized coalition; and (6) a pivotal party leaves a majority government, resulting in a minority government.

Insofar as political scientists do not view cabinet instability as intrinsically important and care only about its effects on political behavior or government performance, treating these diverse events in the same way seems inappropriate. In fact, given the diversity of these events, even if one could establish an empirical link between instability (so measured) and political performance, it would be impossible to identify the precise mechanism by which such instability actually affects performance. It is not surprising, therefore, that confusion and disagreement exists about how to interpret the substantive importance of research on cabinet instability. Some scholars view the level of instability as a measure of poor political performance, and others consider instability irrelevant to understanding government performance.[1]

This article seeks to increase our understanding of the relationship between cabinet instability and political performance. The analysis offers two theoretical arguments about how instability affects the capacity of governments to achieve their goals and uses these arguments to define appropriate measures of instability. The first argument is that cabinet instability makes it difficult for governments to develop the cohesion necessary for adopting and implementing new policy programs. This idea leads to a definition of cabinet instability that focuses exclusively on changes in the *partisan composition of the governing coalition*. The second argument is that instability makes it difficult for cabinet ministers to control civil servants during policy formulation and implementation. This idea leads to a

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The first draft of this article was prepared while I was a Robert Wood Johnson Scholar in Health Policy Research. I received helpful comments from several anonymous reviewers, Chris Achen, Larry Bartels, Nancy Burns, Mike Chernew, Jamie Druckman, Simon Hug, Catherine McLaughlin, Joseph Newhouse, Roy Pierce, and seminar participants at Columbia University, Harvard University, University of Iowa, University of Michigan, Michigan State University, and Washington University. Rob Franzese is due particular thanks for his patient advice and guidance on methodological issues. I received excellent research assistance from James Gutman, Mark Gromala, and Ryan Hudson. I am also grateful to the National Science Foundation for financial support.

[1] Among researchers who treat cabinet stability as a measure of political performance are Linz (1978), Powell (1982), and Strom (1990). Scholars who argue that there is no explicit link between cabinet instability (broadly defined) and political performance include Lijphart (1984a), Sartori (1994), Siegfried (1956), and Taylor and Herman (1971).

definition of instability, called portfolio volatility, that focuses on changes in the *control of policy portfolios within the government*. Measuring changes in the distribution of power within governing coalitions represents the most significant departure in this article from previous perspectives on cabinet instability.

I test the two arguments by examining the effect of the cabinet instability variables on government success at health care cost containment in 18 advanced industrial democracies. These tests, which are described and defended below, indicate that instability in the partisan distribution of portfolios—not the partisan composition of the government—has a substantial influence on political performance. The relationship between portfolio volatility and health care expenditure levels, however, exists only in the short run. The analysis suggests, therefore, that short-run increases in instability present problems for government decision makers, but in the long run, countries with high levels of instability are able to address the commitment and informational problems that it can pose. This finding has implications for future research on how policymakers craft institutional arrangements to address the problems created by instability.

PARTISAN CABINET STABILITY AND GOVERNMENT EFFECTIVENESS

The comparative literature has argued that cabinet instability can hinder government performance in two ways: by making it difficult for parties in government to develop the cohesion necessary to adopt new policies and by denying party leaders in the cabinet the power to follow through on implementing these policies. This argument is developed informally, is rarely made explicit, and takes different forms. All versions, however, focus on the importance of stability in the partisan composition of the cabinet.

The "Italian" version, for instance, argues that there are coalition formulae that last for a long time, providing a core of party leaders who can resolve disputes and push necessary legislation through parliament, regardless of the number of cabinet resignations that may occur over the life of a particular formulae (Dogan 1989; LaPalombara 1987; Spotts and Wieser 1986; Siegfried 1956 develops a similar argument about the French Fourth Republic). Suleiman (1974), to take another example, argues that continuity in the partisan composition of the government in France is important because it gives individual ministers the backing they need to deal with civil servants during policy formulation and implementation. Such collective cabinet support was essential, for example, to Edgar Pisani's successful reform of the agriculture bureaucracy in the 1960s, a success that had eluded the cabinet during the Fourth Republic (see Suleiman 1974, chapter 7). Lijphart (1984a) states that party stability in the government is a sign of executive strength vis-à-vis the parliament. Without such stability, he argues, it is more difficult for the government to adopt desired policies.[2]

These arguments about the link between government effectiveness and stability in the party composition of the cabinet lead to a concept of cabinet instability that differs from typical definitions. The latter include all government resignations (or formal defeats), regardless of their effect on government composition. The research discussed above suggests that such resignations or defeats will have a significant influence on government performance only when they affect the party composition of the cabinet. I provide an operational definition of this partisan cabinet instability below.

PORTFOLIO VOLATILITY AND GOVERNMENT EFFECTIVENESS

A second line of argument uses intensive analyses of unstable systems (in particular, the French Third and Fourth Republic) to conclude that instability in certain portfolios makes it difficult for those ministers to control senior civil servants (Diamant 1959; Leuthy 1954; Scheinman 1965, 211; Suleiman 1974, chapter 7; Williams 1958, 405; Williams 1964, chapter 24; and Williams and Harrison 1960, 67). Such control is important because bureaucrats and cabinet ministers often have competing objectives (Aberbach, Putnam, and Rockman 1981; see also Scheinman 1965; Suleiman 1974, chapter 9; and Williams 1964). Portfolio volatility can add to these differences by giving bureaucrats substantial autonomy in how they organize their units, leading to the capture of agencies by narrow or special interests (see, in particular, two case studies in Suleiman 1974, chapter 7; see also Williams 1964, 378).

Why does portfolio volatility make it difficult for ministers to work with bureaucrats? Scholars have often focused on the role of information. Suleiman is most explicit:

> One of the most important factors that determines the nature of the minister's relationship to his higher civil servants is the possession of information, and this factor is intimately linked to, and affected by, the relative stability or instability of the minister's tenure. Information is generally the monopoly of higher civil servants in the ministry, which can be eroded only if the minister *and* his cabinet remain in office for a sufficiently long time (Suleiman 1974, 165; see also 168–9).

The informational advantage that bureaucrats have over individual ministers in periods of instability enables bureaucrats to block ministerial policy initiatives by refusing to make timely reports (e.g., Suleiman 1974, 167–9; Williams 1964). It also makes it more difficult for ministers to monitor on-going civil service activities.

The argument about portfolio volatility diverges

[2] Simmons (1994) also argues that instability affects the ability of cabinets to act collectively, although her study of monetary policy focuses on presidential and authoritarian regimes, in addition to parliamentary systems.

significantly from other studies of cabinet government. The literature described in the previous section, for example, focuses on the dynamics of "who gets in" government, whereas the portfolio volatility argument focuses on the dynamics of "who gets what" within government. The portfolio volatility argument also differs from formal theories of portfolio distribution in cabinets, which assume that cabinet ministers can costlessly implement their most preferred policies (Austen-Smith and Banks 1990; Laver and Shepsle 1990, 1996). Changes in the distribution of cabinet portfolios, according to these models, do not create informational problems; rather, the redistributions should be viewed as rational responses to changes in the political environment (Laver and Shepsle 1996).

Given that the French studies were not developed with an eye toward offering general theoretical arguments, and given that they lead to conclusions that differ from much of the previous literature, there are reasons to be hesitant about cross-national application of the portfolio volatility argument. It is possible, however, to increase our confidence in the accounts from the French politics literature, and to sharpen our understanding of the dynamics that these accounts describe, by viewing them through the lens of signaling models of information transmission.

These signaling models have sought to explain the circumstances under which a "speaker" with private information (e.g., a civil servant) will reveal this information to a "receiver" (e.g., a cabinet minister) who must take actions that affect the utility of both the speaker and the receiver. The models provide a rich theory about factors affecting information transmission. In their seminal work, for example, Crawford and Sobel (1982) show that the receiver can typically learn from the speaker when the two individuals know that they share a common objective (in such cases, the speaker has no incentive to withhold information or to mislead). Uncertainty by the receiver about the preferences of the speaker has been shown to diminish the extent to which influential information transmission can occur (because the receiver can be less confident that s/he shares a common interest with the speaker) (Austen-Smith 1995; Sobel 1985). Similarly, repeated interactions improve the quality of information transmission (if a speaker expects future interaction with the receiver to be valuable, then the speaker will have obvious incentives to be honest in order to build trust for the future) (Sobel 1985). Finally, opportunities for low-cost ex post monitoring by a receiver make possible random audits of the truth of the sender's information, creating incentives for honesty (Banks 1989).

These abstract models make explicit the logic from the French politics literature. Obtaining reliable information should be easiest when a cabinet minister knows that s/he shares a common objective with civil servants, expects to be in a long-run relationship with them, and bears low costs of oversight. If a cabinet minister is new to a particular post, then s/he will often be uncertain about the preferences of civil servants. This uncertainty should decline as the minister's tenure increases, enabling the minister to learn on whom to rely and creating opportunities for the minister to mold the civil service personnel (for example, by promoting individuals with similar preferences). Consistent with these arguments, Ramseyer and Rosenbluth (1993) argue that during much of the postwar period in Japan, the dominant Liberal Democratic Party was very successful in its use of hiring and promotion strategies in the civil service to promote its interests. In the French Fourth Republic, this was attempted, but instability made it very difficult to do (Suleiman 1974; Williams 1964).

Monitoring costs should increase with ministerial instability for two reasons. First, developing and evaluating new policy proposals takes time, and if a cabinet minister literally is not around to evaluate bureaucratic behavior, then monitoring costs will be prohibitive. Second, monitoring costs are affected by interest groups, which sound "fire alarms" that provide valuable information to legislators (or cabinet ministers) (Banks and Weingast 1992; McCubbins and Schwartz 1984). In order for such alarms to be effective, however, a legislator must be able to rely on the information provided by interest groups, and the informational problems faced by "unstable" ministers in their relationships with bureaucrats should also be present in relationships with interest groups. Not surprisingly, then, Suleiman (1974) and Williams (1964) report that interest groups in the Fourth Republic generally turned to civil servants rather than to cabinet ministers as their primary avenue for influencing policy outcomes.

Finally, an individual minister has to convince the other members of the cabinet that s/he has formulated a good policy and has effectively implemented existing policies. In this context, the cabinet minister becomes the speaker and the collective cabinet the receiver. Cabinet ministers will be most successful in conveying information to colleagues if they all know one another's preferences, expect to be in a relatively long-run relationship with one another, and know that the minister making a proposal has been able to obtain reliable information from civil servants.

In sum, the portfolio volatility argument suggests that the operational definition of instability should focus on changes in the identity of the ministers controlling specific cabinet portfolios. A precise definition of portfolio volatility is provided below.

SHORT- OR LONG-RUN EFFECTS?

The arguments outlined above indicate how partisan instability and portfolio volatility could hinder government effectiveness. These theoretical arguments are unclear, however, about whether the level of instability (or volatility) in a country, or sudden changes in this level, should affect performance. Scholars often focus on the level of instability. That is, they discuss instability as if it were a problem for the political system, the concern being that countries with relatively high levels of instability will have relatively low levels of government effectiveness. If this is true, then instability is said

to be in a long-run equilibrium relationship with performance.

As noted above, however, scholars of unstable systems often contend that instability does not create significant problems. If that view is correct, then the average level of instability in a country should not affect political outcomes. This does not imply, however, that the theoretical arguments are wrong. Instead, it may be that the average levels of instability do not affect government performance, but sudden changes in these levels do. Policymakers in a country, for example, may accommodate quite nicely a certain number of portfolio transfers each year but not a sudden increase in this number. If government effectiveness is influenced by changes in the level of instability, then the effect of instability on political performance is said to exist only in the short run.

The theoretical arguments are not sufficiently specific to sort out the short- versus long-run question. It is important to note, however, that if some countries are plagued by chronic instability, then one might expect policymakers in these countries to adopt institutional structures in cabinets and bureaucracies that respond to the problems instability can create. They may, for example, address the collective cabinet decision-making problems by creating policy institutions that take particular types of decisions out of politicians' hands. Or they may address informational problems by adopting institutional arrangements for gathering information that bypass civil servants. Such solutions, to which I return in the conclusion, should be imperfect and far from universally applicable across the wide range of issues that governments must address. But if government policymakers choose institutional arrangements that are sensible responses to the types of problems revealed by the two arguments discussed above, then short-run rather than long-run relationships between instability and performance should be most plausible.

MEASURING CABINET INSTABILITY AND PORTFOLIO VOLATILITY

We need appropriate measures of partisan cabinet instability and portfolio volatility in order to test the arguments described above. To test the partisan cabinet instability argument, I use a definition of cabinet instability that Lijphart (1984b) advocates. According to this definition, a government termination occurs when there is a change in the partisan composition of the governing coalition, which occurs if either (1) there is a change in the party composition of the government, or (2) there is no such change, but the government loses its majority (requiring the government to rely on external support parties). The more often such terminations occur, the more difficult it should be for governments to achieve their goals. In the analysis below, a country-year will be the unit of analysis, and the variable *Partisan Cabinet Instability* measures the number of times that the partisan composition of the governing coalition changes each year.

Before offering an operational definition of portfolio volatility, it is important to address two conceptual issues. The first concerns the identity of the agent controlling cabinet portfolios: Is volatility in the names of individuals controlling portfolios or in the names of parties controlling portfolios more likely to have an adverse effect on policymaking? If one assumes that individuals cannot easily share information regardless of their party affiliation, then the appropriate unit of analysis is the individual. This should seldom be a reasonable assumption, however. Suleiman (1974), for example, argues that political parties (primarily through their role in staffing ministerial cabinets) help individuals within the same party overcome the problems associated with individual ministerial volatility (see also Ollé-Laprune 1962, 258). And the formal models suggest that even when different individuals from the same party do not wish to cooperate, an individual minister succeeding someone from his own party will benefit from the personnel decisions that the previous minister was able to make in the civil service (assuming the two ministers share similar policy preferences). When an individual replaces a minister from a different party, however, the previous minister should have fewer incentives to help the new one, and the new minister should be less likely to trust information provided by her predecessor. Thus, *Party Portfolio Volatility* should have a larger effect on governmental policymaking than individual volatility if one assumes relatively homogeneous preferences and fluid flows of information within parties.

A second conceptual issue regards the level of aggregation across different cabinet portfolios. Should we expect portfolio volatility to be a problem only if it occurs within the portfolios of cabinet ministers who have jurisdiction in the particular policy area, or should aggregate portfolio volatility also pose a significant problem? Since cabinet ministers typically have responsibility for policy formulation and oversight in particular policy areas, we should expect portfolio volatility to hinder the government when it occurs in the ministry responsible for the policy area under examination. But aggregate portfolio volatility should also affect policymaking. First, particular policies are seldom without jurisdictional overlap. Virtually all major policy decisions will involve the prime minister, the finance minister, and the minister with specific jurisdiction (such as justice or health). And policy decisions often involve more than one jurisdiction (such as the minister of education and the minister of health). Second, focusing on portfolio-specific volatility ignores bargaining problems in the cabinet. Even when an individual minister lasts a long time in her post (giving her advantages in relationships with civil servants), it should be more difficult for this minister to convince colleagues of which policies to adopt when these other ministers are new to their post. Third, a key factor affecting the ability of specific ministers to gain information and monitor effectively is the expectations of civil servants about the duration of a minister's tenure. We have little knowledge about how such expectations arise, but it seems quite plausible that the expectations of civil servants and interest groups about

TABLE 1. Correlations of Various Stability and Volatility Variables

	Total Portfolio Volatility	Within-Party Reshuffles	Party Portfolio Volatility	Ideological Portfolio Volatility	Partisan Cabinet Instability	General Cabinet Instability
Total Portfolio Volatility	1.0					
Within-Party Reshuffles	.54	1.0				
Party Portfolio Volatility	.89	.11	1.0			
Ideological Portfolio Volatility	.64	−.05	.78	1.0		
Partisan Cabinet Instability	.74	.13	.81	.69	1.0	
General Cabinet Instability	.83	.42	.75	.57	.75	1.0

Note: The entries in each cell are Pearson correlation coefficients. Data are from 18 countries, 1972–89. $N = 324$.

portfolio-specific volatility may emanate from more general expectations about the probability of any portfolio changing hands.

This article therefore examines several different measures of portfolio volatility, each of which can be operationalized at the aggregate and portfolio-specific level. For each measure, a country-year is the unit of analysis, and the portfolio volatility variables measure the number of changes in the control of portfolios. But the types of changes that count for each measure are different.

First, consider the various aggregate measures. *Total Portfolio Volatility* is the sum during one year of (1) the number of changes in the individuals controlling portfolios, (2) the number of portfolios added, and (3) the number of portfolios eliminated. Total portfolio volatility includes changes in the size of the cabinet, because such changes often influence relationships between cabinet ministers and civil servants. For example, the addition of new ministers creates relationships where none previously existed, and the elimination of cabinet posts leads to jurisdictional changes for the remaining ministers (and thus to new relationships between them and civil servants).

It is desirable to break down total portfolio volatility into its party and individual components. Party portfolio volatility has the same definition as total portfolio volatility, but it measures only changes that occur across parties (so that a change from Minister Smith to Minister Jones adds to volatility only if Smith and Jones are from different political parties). *Within-Party Reshuffles* refers to changes that occur across individuals within the same party. It is measured by subtracting party portfolio volatility from total portfolio volatility.

Ideological Portfolio Volatility takes account of the ideological distance that portfolios travel when they switch parties (a ministerial shift from, say, a Social Democrat to a Communist may create fewer problems than a shift from a Communist to a Conservative). This variable uses a ten-point Left-Right scale to measure the sum of the (absolute) changes in the ideology of each portfolio that is transferred across parties (assuming each member of the party is at the same Left-Right position), divided by the total number of portfolios transferred across parties in a year. For example, if a single party located at 3 on a ten-point scale holds all ten portfolios in a cabinet at time $t - 1$, whereas two parties, one located at 3 and one located at 5, each hold five portfolios at time t, then ideological portfolio volatility takes the value 1 at time t (((5 portfolios*0 ideological change)+(5 portfolios*2 points of ideological change))/10 portfolios present in both governments).[3]

Each of the aggregate measures also can be adapted at the portfolio-specific level. *Party volatility of the prime minister*, for example, would measure the number of times that the office of prime minister changes party in a given year. *Individual volatility of the prime minister* would measure the number of times that the individual controlling the prime minister's office changes in a given year.

Table 1 provides the correlations of the five aggregate variables described above, as well as a variable called *General Cabinet Instability* (which measures the total number of terminations in a country-year in the Woldendorp, Keman, and Budge [1993] data; see the Appendix for details about the data). As one would expect, the various definitions of portfolio volatility and cabinet instability are clearly related, with all of the correlations except one in the expected direction, and most being fairly strong. Within-party reshuffles is the only variable that is quite weakly related to the others. This weak relationship is due to the fact that a large number of reshuffles occur in single-party governments during years when no other types of instability or volatility are occurring. Most of the total portfolio volatility therefore, is, related to party changes rather than to individual reshuffles. Finally, one should note that since the correlations are far from perfect, these variables are clearly measuring different things.

CABINET INSTABILITY, PORTFOLIO VOLATILITY, AND HEALTH CARE COST CONTAINMENT

The theoretical arguments suggest that two specific types of instability should affect the ability of govern-

[3] I measure the location of parties using data from Castles and Mair (1984), Huber and Inglehart (1995), and Morgan (1976). When data for a party are available from more than one source, I use the data source that is closest in time to the cabinet being coded.

ments to achieve their goals. The next step is to test these arguments empirically. There are, however, two general problems associated with using instability as an independent variable to explain government effectiveness. First, measuring a government's success at achieving its goals requires information about problems requiring government attention, policymaker preferences, and policy outcomes, all of which can be very difficult to obtain. Second, it is tricky to sort out the direction of causation: To operationalize any kind of instability as an exogenous variable affecting performance may be wrong-headed if in fact poor performance is the variable that explains instability.

The empirical test used here addresses these problems. The test involves estimating the effect of portfolio volatility and cabinet instability on health expenditures (as a percentage of GDP) in 18 advanced parliamentary democracies from 1975 to 1989. The general argument behind this test is that in the countries and time periods analyzed, all cabinets, regardless of their ideological dispositions, face strong incentives to contain health care costs. Cost containment requires cabinets to be proactive collectively, and it requires a variety of individual cabinet ministers to deal effectively with civil servants to solve informational problems inherent to health policymaking and implementation. Cabinet instability and portfolio volatility should make cost containment more difficult. Thus, controlling for the other factors that affect health expenditures, increases in cabinet instability and portfolio volatility should be associated with increases in health expenditures.

Health care cost containment is obviously a policy issue with its own idiosyncrasies. It is therefore important to discuss these and to acknowledge that the empirical analysis here, like that on any policy issue, should not be viewed as decisive in evaluating the partisan cabinet instability or portfolio volatility arguments. I will return to this issue in the conclusion. The remainder of this section concentrates on justifying the test, and on making transparent health care's idiosyncrasies.

The hypothesis about instability and health care cost containment follows from several observations about health care provision in the countries analyzed. First, to facilitate inferences about preferences, the data contain only countries and time periods in which health care is not a controversial public policy issue (because the principle of universal access to high-quality health care is universally accepted, see OECD 1992, 1994; Raffel 1985).[4] Second, governments in these countries play the central role in the finance, delivery, and regulation of medical care (see, for example, OECD 1987, 1992, 1994; Raffel 1985). Third, although health care is extremely important for all governments (on average, 7.3% of GDP goes toward health care in the countries and time periods used in the analysis), cabinets rarely fall on issues of health policy. In fact, in the data used, governments fell in only one country, Ireland, because of disputes about health care. And in each of the Irish cases, the government had already been teetering on the edge of defeat, with health care playing only a part in the government's ultimate fall.[5] Thus, it is quite reasonable to assume that if there is a relationship between health spending and portfolio volatility or cabinet stability, it is because volatility or stability affects health spending, and not the other way around.

Finally, it is possible to make inferences about the preferences of government decision makers because all governments have strong incentives to contain health care costs in these countries and time periods. In making this argument, I am not suggesting that all governments want to spend the same amount—some certainly want to spend more than others. Instead, I am arguing that in all these countries, regardless of the level of health expenditures that any particular government desires, all governments face strong incentives to perform some sort of rationing or cost containment (see OECD 1987, 1992, 1994).

This fact is well recognized by health care scholars. Part of the problem is moral hazard on the part of consumers and providers (e.g., Feldstein 1973; Mooney and Ryan 1993; Pauly 1968, 1986; and Reinhardt 1993). Since patients almost invariably receive access to health care under some type of insurance scheme, they never face the full marginal cost of the care they consume (and in most countries with universal coverage, they face virtually no cost at all). Given that medical care is basically "free" at the point of service, patients have incentives to use services which they would decline if they had to pay the full cost (Manning et al. 1987). The value of these extra services to individuals consequently falls short of the social cost of producing them, even when the costs are limited by such cost-sharing mechanisms as co-insurance. Similarly, physicians and other providers have private information about a patient's condition and about the most appropriate treatment. Since neither patients nor payers are in a strong position to challenge a provider's professional judgment about the necessity of a pre-

[4] The countries are Australia (7.6; 3.9), Austria (8.0; 14.7), Belgium (7.1; 28.7), Canada (8.0; 24.4), Denmark (6.5; .2), Finland (6.9; 14.9), France (7.9; 24.8), Germany (8.4; 2.1), Iceland (7.0; 37.9), Ireland (8.1; −14.8), Italy (6.7; 25.7), Japan (6.4; 20.0), Luxembourg (6.7; 21.8), the Netherlands (8.0; 6.6), New Zealand (6.8; 6.2), Norway (7.0; 11.1), Sweden (9.0; 8.2), and the United Kingdom (5.8; 10.5). The first number in the parentheses is the average spending on health (as a percentage of GDP) for the period 1975–89. The second number is the percentage change in health spending when spending in 1989 is compared with spending in 1975.

[5] In 1982, following the death of one Fianna Fail MP and the illness of another, the Fianna Fail minority government fell by two votes when three members of the Workers Party refused to support the government's economic plan, which included cuts to health services. In 1987, four members of the minority Labour government resigned to protest the fact that the poorest members of society would have to bear the brunt of the government's health and social welfare cuts. But these resignations were, in fact, part of a broader dispute within the Labour Party about government participation. And in 1989, the sixth in a string of defeats for the minority Labour government concerned the perceived overcompensation of hemophiliacs who had contracted AIDS. The paralyzed government resigned and called for new elections. In the empirical tests presented below, I run the analysis with and without Ireland.

scribed treatment, if providers are paid for the specific services they provide, they will have financial incentives to prescribe unnecessary services. Even in the absence of financial incentives, providers may feel a professional responsibility to pursue all possible avenues of treatment, regardless of their cost-effectiveness.[6] Since it is impossible for the payers of health care (e.g., insurance companies, sickness funds, or government officials) to obtain the specific information necessary for eliminating unnecessary care, some expenditures on such care is inevitable.[7]

The adverse supply-and-demand incentives inherent to health care provision create the potential for excess spending. But this potential has existed ever since health care has been provided by third-party payers, and during much of this history, health care costs have not been viewed as a large burden on government budgets. This began to change in the early 1970s, and the reason most widely cited by health policy researchers is the rapid technological change that has occurred in the last 25 years. More often than not, technological innovations make it possible either to treat previously untreatable health problems or to lower the personal costs to individuals of treating problems that previously were very difficult to treat (e.g., Newhouse 1992, 1993; Rettig 1994; Schwarts 1987; Weisbrod 1991).[8] In either case, technological innovation creates new opportunities for "excess" supply and demand. Given the important role that technological innovation plays in shaping health care costs, the data used in the test are for the period 1975–89. Many countries had universal coverage before this, but health care costs were not (relatively) large or rapidly increasing prior to the mid-1970s, providing little reason to expect a relationship between the various instability variables and health expenditures during earlier periods.[9]

Portfolio volatility and cabinet stability should be related to cost containment success for several reasons. First, bureaucrats should have little natural incentive to contain costs. Members of the civil service are likely to be sympathetic to the groups affected by the programs they administer, making it difficult to make hard decisions like cutting benefits, closing hospitals, or otherwise limiting access to care. Bureaucrats may also be natural budget maximizers, or they may simply be resistant to change. Cabinet ministers must therefore be able to control effectively the numerous agencies involved in health care finance and delivery and to supervise the implementation of specific reforms passed by parliaments. Second, cabinet instability and portfolio volatility can limit cost-containment success through their effect on the ability of the central government to adopt effective cost-saving legislation. In order to contain health care costs, countries must be proactive, and the OECD has chronicled many of the efforts that have been made since the mid-1970s. These include changes in the systems of finance, in how providers are paid, and in how cost overruns are addressed. Governments also have made hard decisions about rationing care by requiring general practitioners to act as gatekeepers, by limiting the availability of capital equipment, and by placing limits on the types of procedures that are covered. In some countries, proactive legislative efforts assume rather extraordinary detail, with legislation specifying which hospitals (or hospital beds) must close, which pharmaceuticals will be covered by social insurance, or the price paid to providers for individual services (or a given pharmaceutical).

The great variety of cost-containment strategies available to politicians is important for justifying the use of health spending to test the portfolio volatility arguments. One may be tempted to claim that informational arguments are irrelevant to the health care spending issue if indeed all parties share the same cost-containment objective. But such an argument would be wrong. On the one hand, competing parties may have few incentives to help each other regardless of their preferences (making it more difficult for cabinets to control civil servants if civil servants do not have cost-containment objectives). On the other hand, parties will likely have quite different views on how to contain costs (with some, for example, preferring to focus on providers, others on payers, and still others on consumers). An illustrative example comes from the Netherlands. In 1986, the Center-Right government formed the Dekker Committee on health care reform, which made a report that led to major structural changes in 1988. But when a Center-Left government came to power in 1989, it stopped implementation of the reforms for six months, not because the Left government opposed cost containment, but because it wished to conduct its own review of how to do it. The new government ultimately decided to continue implementing the main cost-containment initiatives proposed by the Dekker Committee, but it also decided to slow the pace of reform, and to modify changes proposed by the Dekker Committee on a variety of policy issues, including the content of the basic insurance package, compulsory payment of premiums, and the regulation of competition (see OECD 1992, chapter 7).

In sum, since the early 1970s, all governments have faced severe cost-containment problems in the area of health care. Addressing these problems requires the oversight of bureaucrats, as well as the proposal and adoption of new legislation. Cabinet instability and portfolio volatility should negatively affect a government's ability to monitor bureaucrats, obtain information for reform, and act collectively to make changes in the health care system. Thus, when controlling for

[6] On physician-induced demand, see Evans 1974; Hay and Leahy 1982; and Rice and Labelle 1989.

[7] In the United States, the rapid movement toward managed care in the 1990s has been a (partial) response to precisely this problem. In other countries, analogous direct control by payers over providers is rare in medical decision making and was virtually nonexistent during the period I analyze in this article.

[8] It is clearly not the case that all technologies increase the costs of medical care, with the polio vaccine being an excellent and frequently cited example of a technology that reduces costs. But there are numerous examples of technologies—"non-invasive imaging, invasive cardiology, transplantation, monoclonal antibodies, and renal dialysis" (Newhouse 1993, 162)—that increase costs.

[9] The ending date of my data set is 1989 because that is the last year for which I have portfolio data.

other factors influencing health care expenditures, increases in portfolio volatility or cabinet instability should be associated with increases in health spending.

THE EMPIRICAL MODEL

The goal of the statistical tests is to estimate the effect of the various cabinet instability variables on health care cost control. To estimate this effect, it is crucial to control for the other factors that affect health care spending. When such controls are included, the coefficients and standard errors for the cabinet instability variables provide the relevant information about the cost of instability to government effectiveness at containing costs.

The statistical models include eight control variables taken from the literature on health care spending.[10] *Population>65* measures the percentage of all individuals who are 65 years of age or older. Since the elderly suffer frequent, chronic, and serious health problems, they are expensive to treat. We should therefore expect this variable to have a positive effect on expenditures. *Medical CPI* measures the medical price index. Numerous individuals point out that technological advance has led medical-specific inflation to outpace overall inflation since the early 1970s. This variable should therefore have a positive coefficient. *Doctors per Capita* is the number of doctors per 1,000 citizens. Given the monetary and nonmonetary incentives that physicians have to oversupply medical care, this variable should have a positive coefficient. *Fee for Service* is a dummy variable that takes the value 1 if physicians are paid according to the number of services they provide and 0 otherwise (when physicians are paid by capitation or salary). The variable should have a positive coefficient because fee-based payment systems encourage oversupply of care by doctors. *Global Budgets* is a dummy variable that takes the value 1 if hospitals are paid by global budgets and 0 otherwise. Scholars have argued that capping spending should be an effective way of curtailing spending in hospitals on unnecessary care. This variable should therefore have a negative coefficient. *Public Finance* is the percentage of total health expenditures that are public expenditures. Scholars argue that this is an important variable, but they differ on its expected effect. Some argue that centralization of finance in the hands of one (or a small number of payers) creates greater opportunities to shape individual incentives to limit demand (such as co-insurance) (see Cuyler 1989; see also Gerdtham et al. 1992). Viewed this way, the public finance variable should have a negative coefficient. Leu (1986), on the other hand, argues that the coefficient should be positive because he believes the concentration of decision-making power in the hands of government bureaucrats leads to inefficiencies. Finally, *Log(GDP/capita)* is the log of GDP per capita. This variable has been widely found to be linked to health expenditure levels, with richer countries spending more on health care.

A final control variable, which I omit from the results reported here (see below), measures the Left-Right ideology of parties in government. Justification for including this variable would be a belief that Left governments should want to spend more on health care than Right governments (as is often posited, for example, in studies of the welfare state; see Hicks and Swank 1992 or Pampel and Williamson 1989). It is important to note, however, that the Left governments argument is difficult to defend in the health expenditures context. As noted above, the data are carefully chosen to include only countries and time periods in which the battle over universal coverage had already been won, with the principal of universal coverage widely accepted by all parties and seemingly irrevocable. In addition, the direction of a Left-Right argument is unclear in an environment of cost containment—money spent on health care often goes to groups that are likely to support the Right, such as pharmaceutical companies, physicians, or religious hospitals.

In empirical tests, when variables measuring the Left-Right position of the government are included in the analysis, none has an effect that is remotely close to statistically significant. Moreover, including government location variables never affects the results for the other variables in the analysis. Given the shaky theoretical foundation for the government ideology argument and the lack of empirical support for including it, I omit the government ideology variables from the analysis below.[11]

To estimate models that use time series data with cross sections, ordinary least squares (OLS) is inappropriate because such data have error processes that violate standard OLS assumptions. In particular, models for such data should allow for temporally correlated errors (errors in country j at time t are correlated with errors in country j at time $t - 1$), spatially correlated errors (errors in country j at time t are correlated with errors in country k at time t), and panel heteroskedasticity (error processes differ across countries, for example, because of the size of the countries). I therefore use an error correction model to estimate the relationship between the instability variables and health expenditures. Error-correction models are perhaps best known for use in situations where the dependent variable is nonstationary and cointegration is present (i.e., two or more variables are in an equilibrium relationship and thus tend to vary in the same way). In my data, the dependent variable is stationary, so I am not using error-correction to address cointe-

[10] See, for example, Chu 1992; Gerdtham, Søgaard, Andersson, and Jönsson 1992; Glaser 1991; Hitiris and Posnett 1992; Jellinek 1982; Mendelson and Schwartz 1993; Newhouse 1977; Pampel and Williamson 1989; Parkin, McGuire, and Yule 1987; and Wolfe and Moran 1993.

[11] I obtain measures of the Left-Right locations of parties using data from Castles and Mair 1984; Huber and Inglehart (1996); and Morgan 1976. When more than one data source is available for a party, I use the data most proximate to the country-year being measured. I weight the Left-Right position of each party in government by the number of seats that it controls in parliament, and for each country-year, I weight each government by how many days it was in power. Government location variables were estimated using the current year, as well as averages for the past 2, 3, and 4 years.

gration but rather because it has nice properties for investigating the theoretical questions posed above.[12]

The error-correction model is given by

$$\Delta y_{i,t} = \alpha + \Delta x_{i,t}\beta + \phi(y_{i,t-1} - x_{i,t-1}\gamma) + \varepsilon_{i,t}, \quad (1)$$

where $y_{i,t}$ is health expenditures in country i during year t, and x is a vector of independent variables. The dependent variable, then, is the change in health expenditures, and the independent variables include the lagged level of health expenditures, the lagged values of each independent variable, and changes in the independent variables. The model is based on the idea that the dependent and independent variables are in a long-run equilibrium relationship,[13] and also that there exist short-run (temporary) effects of the independent variables on the dependent variable. To see this, note that the model one actually estimates is

$$\Delta y_{i,t} = \alpha + y_{i,t-1}\beta_0 + \Delta x_{i,t}\beta_k + x_{i,t-1}\beta_j + \varepsilon_{i,t}.^{14} \quad (2)$$

The short-run effects are given by β_k, which provides estimates of the effect of changes in the independent variables on changes in health expenditures. The long-run effects measure the long-run equilibrium relationship between the independent variables and the levels of health expenditures.[15] The model therefore will allow us to explore whether the theoretical arguments about instability and volatility are about long-run equilibrium relationships and/or short-run impediments to government performance.

I estimate the model using OLS, which produces consistent parameter estimates under standard assumptions. The lagged dependent variable addresses the problem of serial correlation. I report panel-corrected standard errors to address problems of panel heteroskedasticity and contemporaneous correlation of errors across countries (Beck and Katz 1995).[16]

The political instability variables are measured based on average levels of instability or aggregate volatility in the three years prior to the current year of health spending.[17] The tests of the portfolio volatility arguments focus on aggregate variables because health expenditures turns out to be a very difficult issue on which to test portfolio-specific arguments. One problem with a portfolio-specific approach is that ministers other than the minister of health make important decisions that affect health expenditures. Two very important actors are the finance minister and prime minister, but the social security minister, labor minister, education minister, trade minister, minister for intergovernmental affairs, and even environmental minister (who makes decisions about the disposal of the radioactive waste produced by hospitals) also play an important role in health policymaking. A second problem is that across countries, health ministers do quite different things. Sometimes they have large responsibilities for making decisions that affect spending, while in other countries their role is much more limited.[18] Thus, testing the effect of health ministry volatility on health spending would be inappropriate, and in this analysis I focus exclusively on aggregate portfolio measures.

EMPIRICAL RESULTS

Table 2 presents the results.[19] The first five columns include the control variables and one of the instability variables. The first five rows give the estimates for the long-run (permanent) effects of the instability variables on health expenditures (β_j in equation 2). The next five rows (6–10) give the estimates for the short-run (temporary) effects. The controls are given at the bottom of the table, as is the lagged health expenditures variable.[20]

First consider the long-run effects of instability (the level variables). Since rows 1–5 indicate that none of the political variables is even close to statistically significant, these regressions reveal the absence of any long-run equilibrium relationship between the level of

[12] A Dickey-Fuller test strongly rejects the possibility that a unit root exists. My discussion of the error correction model draws heavily on Beck 1991 and Beck and Katz 1996.

[13] The "error correction mechanism" (the term in parentheses in equation 1) measures how far out of equilibrium this relationship becomes following short-run changes, and the coefficient ϕ measures the speed with which the relationship returns to equilibrium.

[14] Equations 1 and 2 are the same: β_0 in equation 2 is ϕ in equation 1, β_j is $\phi\gamma$, and β_k is β.

[15] These long-run effects are given by $-\beta_j/\beta_0$. Note that we can rewrite equation 2 as an ordinary lagged dependent variable model, $y_{i,t} = (1 + \beta_0)y_{i,t-1} + \Delta x_{i,t}\beta_k + x_{i,t-1}\beta_j + \varepsilon_{i,t}$. The calculation of long-term effects is therefore the same in this context as the calculation of long-term effects in standard lagged dependent variable models. See, for example, Green 1997, chapter 13.

[16] Beck and Katz (1996) conduct a variety of Monte Carlo simulations which support the use of the lagged dependent variable with panel-corrected standard errors in this context. For each estimation presented, I conducted the Lagrange multiplier test described in Beck and Katz 1996. All these tests indicate the presence of no serial correlation in the models that include the lagged level of health expenditures.

[17] A lag is used because it takes time for government decisions to affect actual spending outcomes. In addition, excluding the current year addresses the endogeneity problem—it would be very difficult to argue, for example, that health spending in 1985 caused cabinet instability in 1984 (especially controlling for health spending in 1984, as done here). Averages address the problem that the January 1 cutoff points result in arbitrary categorizations of volatility and instability that occur near the beginning or end of a calendar year. The three-year averages provide a reasonable way to capture the time it takes for decisions to affect outcomes, to maintain proximity to the actual spending, and to smooth volatility across the (arbitrary) calendar years.

[18] Countries vary considerably, for example, on whether health ministers are responsible for hospitals, insurance companies, the overall performance of the health care system, finance, budgets across different aspects of health care, public health, physicians, reimbursements, medical education, pharmaceuticals, and so forth. As noted above, often these various tasks are shared across a range of ministers.

[19] In addition to the results presented here, I also estimated the model with the general cabinet instability variable described in Table 1. The coefficients for the change and level versions of this variable were not statistically significant, which is not surprising given the atheoretical nature of the general instability variable. As noted above, even had the variable been statistically significant, it would be difficult to know how to interpret the effects, since the variable captures such a diversity of events.

[20] All results were generated using the *xtgls* command (with the *pcse* option) in Stata 5.0.

TABLE 2. Effects of Portfolio Volatility and Partisan Cabinet Instability on Health Expenditures

Dependent Variable: Change in Health Expenditures/GDP	(1)	(2)	(3)	(4)	(5)	(6)
Total Portfolio Volatility	−.0009	—	—	—	—	—
	(.003)					
Within-party Reshuffles	—	−.008	—	—	—	−.008
		(.005)				(.005)
Party Portfolio Volatility	—	—	.002	—	—	.005
			(.003)			(.005)
Ideological Portfolio Volatility	—	—	—	−.02	—	—
				(.03)		
Partisan Cabinet Instability	—	—	—	—	−.01	−.06
					(.05)	(.09)
Δ Total Portfolio Volatility	.006*	—	—	—	—	—
	(.003)					
Δ Within-Party Reshuffles	—	−.005	—	—	—	−.005
		(.007)				(.007)
Δ Party Portfolio Volatility	—	—	.009**	—	—	.016*
			(.004)			(.007)
Δ Ideological Portfolio Volatility	—	—	—	.04	—	—
				(.03)		
Δ Partisan Cabinet Instability	—	—	—	—	.11	−.13
					(.07)	(.13)
Fee for Service	.11**	.11**	.10*	.10*	.10*	.09*
	(.04)	(.04)	(.04)	(.04)	(.04)	(.05)
Global Budgets	−.07	−.07	−.08*	−.07*	−.07	−.08
	(.04)	(.04)	(.04)	(.04)	(.04)	(.04)
Population > 65	.022**	.022**	.022**	.022**	.023**	.021*
	(.009)	(.009)	(.009)	(.009)	(.009)	(.009)
Doctors per Capita	.071	.061	.072	.079	.071	.060
	(.049)	(.050)	(.049)	(.050)	(.049)	(.049)
Public Finance	−.006*	−.007**	−.006*	−.006*	−.006*	−.007**
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Medical CPI	−.003**	−.002*	−.002*	−.003**	−.003**	−.002*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Log(GDP/capita)	−.17*	−.17*	−.18*	−.18*	−.18*	−.17*
	(.08)	(.08)	(.08)	(.08)	(.08)	(.08)
Δ Population > 65	.16	.20*	.17	.17	.17	.19
	(.10)	(.10)	(.10)	(.10)	(.10)	(.10)
Δ Doctors per Capita	.31	.24	.26	.30	.32	.24
	(.37)	(.37)	(.37)	(.37)	(.38)	(.37)
Δ Public Finance	−.013	−.016*	−.013	−.014	−.013	−.015
	(.008)	(.008)	(.008)	(.008)	(.008)	(.008)
Δ Medical CPI	.018***	.018***	.018***	.019***	.018***	.017***
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Δ Log(GDP/capita)	−3.7**	−3.6***	−3.6***	−3.6***	−3.7***	−3.4***
	(.70)	(.69)	(.70)	(.70)	(.69)	(.70)
Lag of health expenditures	−.11***	−.12***	−.11***	−.11***	−.11***	−.12***
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Constant	1.71***	1.85***	1.67***	1.70***	1.70***	1.86***
	(.30)	(.31)	(.29)	(.30)	(.30)	(.31)
Adjusted R^2	.27	.26	.27	.26	.26	.27
Root Mean Square Error	.27	.27	.27	.27	.27	.27

Note: OLS is used to estimate the parameters, and panel-corrected standard errors are given in parentheses. $N = 270$ (15 years and 18 countries). $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

cabinet instability or portfolio volatility and the level of health expenditures. Put differently, all else equal, countries with higher levels of portfolio volatility or cabinet instability do not control health expenditures less effectively than do countries with lower levels. These results, then, do not support the theoretical arguments, applied at the level of the national political system.

The results for the short-run effects of cabinet instability (the change variables, which are denoted by the symbol Δ) are given in rows 6–10. All of the coefficients except within-party reshuffles have the expected positive sign. And two of the variables, Δ *Total Portfolio Volatility* and Δ *Party Portfolio Volatility* are precisely estimated (with the p-value for the coefficients on party volatility being less than .01). The short-run effects for within-party reshuffles and ideological portfolio volatility are measured imprecisely and do not support the

theoretical arguments. Given these results, it seems clear that the component of total volatility that is most important for explaining health expenditures is changes in the partisan control of portfolios. The coefficient for partisan cabinet instability is more precisely estimated than those for within-party reshuffles or ideological portfolio volatility, but this coefficient is not statistically significant at conventional levels (p = .12).

Before discussing these results in greater detail, it is useful to test the two theoretical arguments in the same statistical model. The results are given in column 6 of Table 2, where the model includes within-party reshuffles, party portfolio volatility, and partisan cabinet instability (as well as the other control variables). These results, of course, should be interpreted with some caution because of the high correlation between party portfolio volatility and partisan cabinet instability. Nevertheless, it is important to note that the result for the short-run effect of party portfolio volatility is consistent with that found in column 3. The coefficients for both the short-run and long-run cabinet instability variables continue to be measured with considerable error, as do the coefficients for within-party changes in portfolio control. The results from column 6, then, provide support for the informational argument applied to party volatility, but they fail to support the application of informational arguments to reshuffles of portfolios within political parties. The analysis also fails to support the argument about the partisan composition of the cabinet.

The relationship between changes in party portfolio volatility and health expenditures seems very robust. The results, for example, are not sensitive to the precise specification of the control variables. As noted above, none of the results is affected by the inclusion of a variable measuring the Left-Right ideology of the government (which is of doubtful theoretical relevance). And although Table 2 includes the control variables that I feel are most prominent in the health spending literature (see cites in footnote 10), the inclusion of each of these variables is not uncontroversial. Newhouse (1992), for example, argues that medical CPI is a noisy variable, and the long-term effect of this variable has the wrong sign in Table 2. And Glaser (1987) argues that global budgets may not always contain costs. I therefore estimated models without each of these variables. In addition, it is not clear what the functional form for the GDP variable should be; so I also estimated the model using GDP/capita, rather than the log of this variable. Finally, although the theoretical argument for doctors per capita is well developed, this variable is not statistically significant in Table 2. I therefore estimated the model without it. Notably, none of these permutations in the specification of the control variables affects the substantive findings regarding party portfolio volatility.

In addition, the findings are not sensitive to the inclusion of particular countries. One can remove any country from the model in column 3 or column 6 and the coefficient for the party portfolio volatility variable remains precisely estimated and stable in magnitude.

Similar "robustness" tests also show some fragile support for the arguments about partisan cabinet instability. In particular, when any of five countries is removed from the data (Austria [.09], Finland [.06], Ireland [.09], Italy [.09], or Japan [.07]), the coefficient for the short-run cabinet composition variable is statistically significant in the expected direction and is measured reasonably accurately (with p-values ranging from .06 to .09).[21]

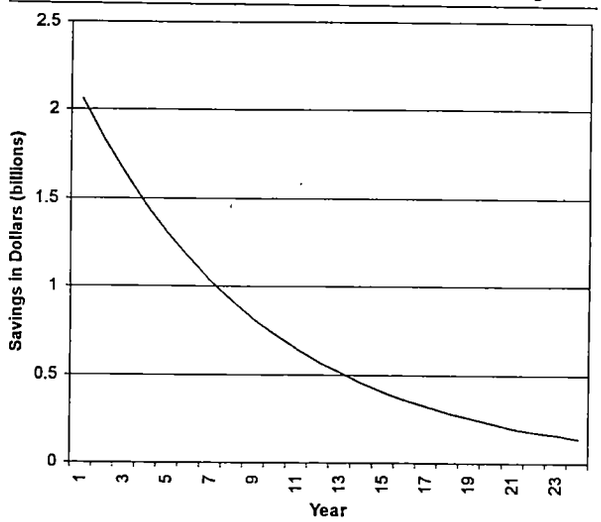
Party portfolio volatility has a robust short-run effect on health expenditures, but is the magnitude of this effect substantial? In error-correction models, the magnitude of a short-run effect depends on the size of the effect *and* on how long it persists. How long an effect persists depends on the relationship between the dependent variable and its lag (because, for example, if a change in variable x leads to a change in expenditures at time t, this effect will continue into time $t + 1$ if expenditures at time t affect expenditures at time $t + 1$). In general, if β_0 is equal to the coefficient on the lagged dependent variable, then to find out how many time units, t, it takes for the short-run effect to reach p % of its original value, one must calculate $(1 + \beta_0)^t = p$. The effect after t years is therefore $t = ln\ p/ln\ (1 + \beta_0)$.

If we use the coefficients in column 6, then a one standard deviation increase in Δ party portfolio volatility (10.7 portfolios) in year t would be associated with an increase in health spending of 2/10 of 1% of GDP in year $t + 1$. The results for the lag of the health expenditures variable (see the bottom of Table 2) indicate a strong effect of health spending at time $t - 1$ on health spending at time t. The effect of this change would therefore disappear quite slowly, remaining at 1/10 of 1% of GDP 5.3 years later.

Figure 1 shows another way to view the magnitude of the effect of the change variable for party portfolio volatility. The figure provides an answer to the following hypothetical question: If France reduced its level of party portfolio volatility to the level found in Germany and sustained this reduction, how much money would France save on health expenditures? In my data, France's average level of party portfolio volatility is approximately 14 per year, and Germany's is approximately 3 per year, so France would have to reduce its portfolio changes across parties by about 11 per year to achieve Germany's level of party portfolio volatility (just over 1 standard deviation in the data set). Using the .016 coefficient for Δ party portfolio volatility (from column 6 of Table 2), the reduction by 11 in party portfolio volatility would decrease spending by .176% of GDP. France's GDP was 1.173 trillion dollars in 1995. Thus, this one-time decrease in the level of party portfolio volatility would save France just over two billion dollars in the first year. But as noted above, since there is a strong effect of health spending at time $t - 1$ on health spending at time t, the savings in the

[21] In addition, when the medical CPI variable is excluded, the short-run effect of party cabinet instability is statistically significant, (p = .04), although this effect disappears when any of three countries is omitted from the data set.

FIGURE 1. Potential Health Care Savings in France Following a Reduction in Party Portfolio Volatility to the Level in Germany



Note: Figure assumes that (1) there is a (one-time) reduction in portfolio volatility of 11 portfolios (see text), (2) the effect of party portfolio volatility is equivalent to that given in column 6 of Table 2, and (3) the GDP in France of 1.173 trillion dollars.

first year would have a ripple effect beyond the first year. As Figure 1 shows, assuming no further change in party portfolio volatility, the effect of this one-time reduction would still be about one billion dollars in savings eight years later, and would be about 225 million twenty years later. The coefficient for this variable, then, is substantively large, and although the effect is temporary, it disappears quite slowly.

CONCLUSION

The empirical tests produce two significant findings. The first is the absence of a long-run equilibrium relationship between any of the instability or volatility variables and health expenditures. Although this finding obviously concerns only one policy area, it underlines the need for careful thought about whether the average level of government instability in a country affects political performance there. Indeed, although scholars frequently talk of instability as if it is a problem that plagues particular countries or regimes, specialists of the most unstable systems (Italy and Fourth Republic France) have often questioned the extent to which instability actually creates policymaking problems (e.g., Dogan 1989; Sartori 1994; Siegfried 1956). Consistent with the doubts raised by these specialists, the results presented here indicate that on average, countries with higher levels of partisan instability in governing coalitions or partisan portfolio volatility find ways to solve problems that instability and volatility can create.

But the second empirical finding suggests that although the theoretical arguments do not explain differences in performance at the system level, they do explain problems that instability can create in the short run. In particular, the analysis provides robust support for the informational argument that an increase in the number of portfolio changes across parties limits government effectiveness. Empirical support is much weaker, however, for the argument about the effect of changes in the party composition of the governing coalition on political performance. The analysis therefore underlines the importance, in operationalizing definitions of governmental stability, of measuring not simply the occurrence of particular events (i.e., government terminations) or the changes that occur in government composition, but also of measuring the distribution of power across parties within governing coalitions over time.

Together, these two findings suggest several items for future research. First, the presence of short-run effects and the absence of long-run effects for party portfolio volatility points to the need for further analysis of how institutional arrangements shape relations between bureaucrats and cabinet ministers. Scholars have offered interesting arguments that particular institutional arrangements can help elected politicians overcome the informational advantages often enjoyed by bureaucrats (e.g., Bawn 1995; Kiewiet and McCubbins 1991; Ramseyer and Rosenbluth 1993). But this research does not explore how (exogenously caused) changes in the policy objectives of elected officials (i.e., by changes in party control of portfolios) affects the ability of these officials to create appropriate incentives in bureaucracies. As noted above, if portfolio volatility is a regular feature of political life, and if portfolio volatility poses informational costs to ministers in their relationships with civil servants, then we should expect ministers in such systems to adopt organizational structures that limit these costs.

The analysis presented here has not focused on what such structures might look like, although certain possibilities suggest themselves. For example, if volatility diminishes the ability of cabinet ministers to rely on information from civil servants, then we may expect ministers to look to other sources for information, at least during policy formulation. These sources may include interest groups (though previous comparative studies suggest that volatility encourages interest groups to create alliances with bureaucrats), special task forces that are created to study specific problems, extensive ministerial cabinets (personal staffs of ministers who are policy experts), or extensive party-funded research staffs. Similarly, politicians in volatile systems may create more fluid rules for hiring and promoting senior civil servants, so that new ministers can quickly shape the personnel in the relevant agencies. They also may grant less autonomy to senior civil servants in establishing annual agency budgets.

Second, the finding of a short-run effect of portfolio volatility on political performance suggests the need for additional research on the distribution of ministerial portfolios. As noted above, Laver and Shepsle (1990, 1996; see also Austen-Smith and Banks 1990) have made headway on this question, but the analysis presented here suggests the presence of an important informational problem that their approach neglects. In

particular, the assumption in existing models that cabinet ministers can costlessly implement their most preferred policies should be adjusted to accommodate more explicitly both the *distributive* and the *informational* cost and benefits associated with changes in the distributions of portfolios. Such an approach may yield new insights into why in some countries political parties often seem to "own" particular portfolios (see, e.g., Budge and Keman 1990).

Third, additional research needs to focus on in-depth analyses of the precise ways in which portfolio volatility affects policymaking. Drawing on previous studies of cabinet government in unstable systems, and on existing game theoretic models, I have argued that portfolio volatility affects political performance through its effect on information transmission. Consistent with the theoretical argument, the empirical analysis provides direct evidence that portfolio volatility is a problem for governments. Although I am unaware of other theoretical arguments that may lead to the same operationalization of the portfolio volatility variable, I also recognize that the empirical analysis provides only indirect evidence of informational problems across ministers, civil servants, and interest groups. Of course, the same will be true of any analysis that uses instability variables as proxies to explore the effects of particular types of political decision-making problems on political performance. Future research should therefore explore more directly the ways in which cabinet ministers address the types of informational problems (or cabinet decision-making problems) discussed in this article.

Finally, the book should hardly be closed on further analysis of how changes in the party composition of the governing coalition affect political behavior. On one hand, there is weak empirical evidence supporting the argument. On the other hand, the empirical tests used here may have stacked the deck in favor of informational arguments. As noted, health care raises a great deal of informational difficulties for policymaking, and many health expenditure decisions are the result of interactions between civil servants and ministers. In this respect, it is not surprising that the results are stronger for the portfolio variables than for the cabinet stability variables. On other issues, especially ones where informational asymmetries are less important, or where policy differences across political parties are more stark, it is more likely that frequent or unexpected changes in the partisan composition of the cabinet will create difficulties for party leaders.

This observation further underscores the importance of developing careful theoretical arguments about the types of instability that are important in specific decision-making contexts. Without such arguments, it is impossible to know how to operationalize instability as an explanatory variable, and it is also impossible to know how to interpret the significance of research focusing on the sources of instability itself. With such theoretical arguments, it is possible to investigate a wide range of issues that are central to achieving a better understanding of political decision-making in parliamentary democracies.

APPENDIX: Summary Statistics of Variables

Variable	Source	Mean	SD
Health Spending	OECD 1993	7.2	1.0
Population >65	OECD 1993, Table A1.1.5	12.7	2.3
Doctors per Capita	OECD 1993, Table 5.1.1 and Table A1.1.1	1.9	0.5
Public Finance	OECD 1993, Table 4.1.2 and Table 4.1.1	81.0	8.5
Global Budgets	Gerdtham et al. 1992, OECD 1992, 1994, and Wolfe 1993		
Fee for Service	Gerdtham et al. 1992		
Medical CPI	OECD 1993, Table 4.4.1	79.2	32.3
Log(GDP/capital)	OECD 1993 software package using purchasing power parity dollars	2.2	0.4
Total Portfolio Volatility	Woldendorp et al. 1993 and *Keesing's Record of World Events*[a]	11.3	12.5
Party Portfolio Volatility	Same as Total Portfolio Volatility	6.9	10.6
Within-Party Reshuffles	Same as Total Portfolio Volatility	4.6	5.7
Ideological Portfolio Volatility	Same as Total Portfolio Volatility[b]	.6	1.1
Partisan Cabinet Instability	Same as Total Portfolio Volatility	.3	.6

[a]Woldendorp et al. (1993) was used to identify changes that occurred across successive governments. *Keesing's Contemporary Archives* was used to identify each portfolio change that occurred between the formation of successive governments.
[b]In addition, three expert surveys were used to place each government party on a 10-point Left-Right scale (Castles and Mair 1984, Huber and Inglehart 1995, and Morgan 1976). (Data from Castles and Mair and Morgan were converted to the 10-point scale.) Where multiple data sources existed for particular parties, the expert survey most proximate to the time period of analysis was used.

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Comparative Dynamics of Parliamentary Governments

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This article presents a dynamic theory of parliamentary governments that incorporates attributes of the institutional system in a country, exogenous events that shape parliamentary and electoral opportunities, and the strategies of the government and the opposition as structured by institutions and preferences. The dynamics are investigated in an infinitely repeated game in which events in the form of shocks to income or government resources occur and the government responds with a legislative proposal that is subject to a confidence or censure procedure and may lead to government continuation, reorganization, or dissolution. With a majority confidence procedure, governments are stable, and if parties are politically patient, voting cohesion within the government is high. A censure motion initiated by the opposition can result in voluntary dissolution of government, and the approach of required elections increases the likelihood of dissolution. If the events represent fluctuations in aggregate income, government dissolution occurs in good times for the government leader and bad times for the other parties.

Formal theories of parliamentary governments typically focus on the formation and duration of governments. This focus reflects the fundamental characteristic of such systems that the government is installed by and continues under the confidence of the parliament. Under this confidence relationship the government exercises effective control of the agenda-setting process in the legislature, and the legislation enacted by the parliament then follows from the program of the government. Changing circumstances and exogenous events, however, can disrupt this relationship and result in government termination, which requires either the formation of a new government or an unscheduled election. This article presents a dynamic theory of parliamentary government with an emphasis on government formation, legislation, and termination under alternative institutional confidence relationships.

The empirical literature has taken three basic approaches to the study of the duration and termination of parliamentary governments (Browne, Frendreis, and Gleiber 1988; Strom 1988). One is based on attributes of the country and its political system, including voter preferences and societal cleavages, party polarization and fragmentation, and electoral and parliamentary institutions (Dodd 1976; Taylor and Herman 1971; Warwick 1979, 1992). A second approach focuses on critical events, such as economic and political crises, that can lead to government termination, and it often involves estimation of a hazard rate (Browne, Frendreis, and Gleiber 1984, 1986; Merlo 1997). These two streams have been combined by King, Alt, Burns, and Laver (1990), who consider a model that includes both a hazard rate and attributes.

The third approach focuses on the strategies governments and their opposition adopt in response to events that affect preferences and opportunities. These strategies determine whether, for example, a government reorganizes or terminates, and hence they identify which events are critical. Strom (1990) and Grofman and Van Roozendaal (1994) take this perspective in empirical studies, and Lupia and Strom (1995) and Diermeier and Feddersen (1996) present theories of government termination based on this approach. The strategic perspective identifies the importance of parliamentary activity in government termination and requires that legislation be taken into account. This paper was motivated by the work of Lupia and Strom and Diermeier and Feddersen.

The model presented here includes the principal features of these three approaches: attributes of the institutional system in a country, exogenous events that represent changing circumstances and shape parliamentary and electoral opportunities, and the strategies of the government and the opposition as structured by institutions and preferences. The focus is on the conjunction of legislative activity and parliamentary procedures for terminating a government, and the principal institutional features considered are confidence and censure procedures.[1] The dynamics are investigated in an infinitely repeated game of distributive politics. In each period, an event in the form of a shock to income or government resources occurs, and the government responds with a legislative proposal that is subject to a confidence or censure procedure and may lead to government continuation, reorganization, or dissolution. This provides a comparative theory of parliamentary governments in which the comparison is across procedures for government termination. It also provides an approach to the study of the dynamics of parliamentary governments that can inform empirical studies.

The theory seeks explanations based on the institutional characteristics of the parliament and not on preference-based alignments of interests that can pro-

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This research was supported by NSF Grant No. SBR-9409559. Daniel Diermeier, John Huber, three referees, and the participants in the Marschak Colloquim at UCLA, the University of Houston, University of Minnesota, Rice University, Stanford University, and the European Science Foundation conference on the Political Economy of Economic Policy provided helpful comments on an earlier version of this paper.

[1] See Laver and Schofield (1990), chapter 4, for an analysis of the role of these procedures in parliamentary democracies.

vide stability to governments. The model thus is formulated to make government stability difficult to achieve and sustain. The model has five features. First, the interests of parties are diametrically opposed, and hence the stability of a government does not directly follow from preferences. Second, legislative proposals are unrestricted, so the government can seek the support of the opposition on legislation. Third, the parties or members of parliament are unable to commit to actions, such as always voting for the proposals of their coalition partners or against the proposals of the opposition. Fourth, the events are independently distributed, so there is no serial correlation to favor stability. Fifth, the strategies of parties are not conditioned on the past, so there is nothing in the equilibrium that explains government stability or instability in terms of past coalitions. More specifically, strategies are not based on a history that can provide an affinity for a particular government, nor are government formation, legislation, and termination based on punishments for past behavior. Instead, government formation and stability are based on opportunity, and since parties are assumed to be unable to commit to how they will act in the future, they act based on current opportunities and their expectations about future opportunities. Despite these features, governments can be stable. The principal factors contributing to this stability are the control of the agenda-setting process and the opportunity to respond to events through legislation and possibly the direct reorganization of government, if permitted by the confidence procedure (Strom 1990, 110–1).

Parliamentary institutions include a variety of both constitutional and less formal procedures for terminating a government prior to a required election. Huber (1996) reviews government-initiated confidence procedures in eighteen parliamentary democracies, and Laver and Schofield (1990, 62–6) provide data on confidence and dissolution procedures in twenty countries. Using the distinction made by Huber, two procedures are considered. The first is a majority confidence procedure initiated by the prime minister or government under which the government terminates if a legislative proposal it introduces fails to receive a majority vote. Huber (1996) refers to this as a "confidence vote procedure," and with the exception of how abstentions and absences are counted, it is the procedure in all but one of the eighteen countries identified in his Table 1. The second is a censure or no-confidence procedure under which the government terminates as the result of an opposition proposal to which the procedure is attached.[2]

A majority confidence procedure is shown to yield government stability, but exogenous events may provide incentives for a government party to seek the support of the out party on legislation when it is opportune to do so. An important parameter of the model is the political discount factor, which reflects the political impatience of party leaders to obtain benefits for their constituents. This impatience may result from electoral considerations or from the personal preferences of party leaders to be in government. When parties are patient, the political discount factor is high, and the resulting government cohesion is high. When parties are impatient, cohesion is lower. If the events represent fluctuations in aggregate income, then government dissolution can occur in good economic times for the constituents of the government leader and bad economic times for the constituents of the other parties. This corresponds to the opportunistic calling of an early election by the government leader.

A censure procedure can result in voluntary dissolution of the government, and this occurs when events are favorable for the government leader. This is consistent with the perspective that changes of government are caused by events and pressure from the opposition. The approach of required elections increases the likelihood that there will be a voluntary dissolution, since the incentive to maintain control of the agenda-setting process is reduced. The hazard rate thus is increasing, but this is caused not by the time in office but by the time to the next required election. The theory also explains which events are critical for government cohesion and stability and which institutional factors affect the set of critical events.

In addition, the model predicts that the governments formed are minimal winning coalitions, since the party designated the formateur has majoritarian incentives to redistribute income among as few parties as possible (Riker 1962). Moreover, governments always include the smallest of three parties and hence are never formed by the largest parties. Alternative models are considered to identify conditions under which a minority government will form. Surplus majorities do not form.

PARLIAMENTARY GOVERNMENTS

Most parliamentary systems have a set of distinguishing institutional features. First, parliament selects or installs the government—the prime minister and the cabinet—so the executive and a majority in parliament are aligned. Second, the government controls the legislative agenda-setting process, over which the opposition has little direct influence.[3] Third, parliamentary systems have confidence and censure procedures that allow the government or the opposition to condition the continuation of the government on the vote on a legislative proposal. These procedures can lead to a reorganization of the government or an unscheduled election. Once a government has formed, however, it presumably will remain in office unless changing circumstances or events provide incentives to restructure

[2] Majority confidence procedures and censure or no-confidence procedures are both present in most parliamentary systems. The analysis will begin with the first type in isolation, and then both types will be considered together.

[3] Huber (1996), for example, models the legislative process as involving proposals only from the majority that supports the government. Silk and Walters (1995, 109) note that in the United Kingdom almost all government bills are passed, and in 1992–93 all were passed.

or terminate it. Yet, the government members can respond by changing their policies to preserve the government, so the events may or may not be critical. Budge and Keman (1990, 160–1) present data for nineteen parliamentary democracies from 1950–83 indicating that 46.4% of the governments were terminated for political reasons, such as dissension within the government, and 12.6% were terminated to hold an early election. Among the minority governments studied by Strom (1990), 72% were terminated as the result of a crisis or a defeat on a confidence vote, censure motion, or legislative policy.

The formal theory of parliamentary governments initially focused on coalition formation implemented by the allocation of benefits, such as portfolios (Riker 1962). Axelrod (1970) extended this perspective to a spatial model and examined whether coalitions are connected or disjoint. Austen-Smith and Banks (1990) and Laver and Shepsle (1990, 1996) examined coalition formation in a model of ministerial delegation in which the policies of government are determined by the allocation of portfolios and the subsequent delegated policy choices of ministers. Another line of research views government formation as a bargaining game in which parties are sequentially extended the opportunity to form a government. Baron (1991, 1993) presents a sequential model of government formation in which policy-oriented parties bargain over policies and coalitions. Austen-Smith and Banks (1988) present a unidimensional model of a parliamentary system with a proportional representation electoral system and a sequential government formation process. None of these frameworks, however, incorporates the possibility of dissolution of the government once formed.

Several recent models have explored the possibility of government dissolution. Lupia and Strom (1995) present a one-period model incorporating attributes, an exogenous event that reflects the election prospects of parties, and strategic responses by the coalition government. Crombez (1996) presents a model in which, as a function of seat shares and policy preferences, surplus and minority governments can form in addition to minimal winning coalitions. Huber (1996) presents a model in which a prime minister can use a confidence procedure to bring members of the majority party back into line. Huber (1997) extends this to incorporate incomplete information about the preferences of pivotal members of the government coalition, and his tests found that the nature of cabinet decision making and the extent of the opposition's influence affect whether government termination results from defeat in parliament or from resignation. Smith (1996) presents a model of a majority government in which voters have incomplete information about the competency of the government. He found that early elections are called when times (e.g., the economy) are good, and more competent governments wait longer than less competent governments before calling an election. In the complete information model considered here, governments are also more likely to terminate in good times than in bad for the government leader (agenda setter), and the more impatient the parties and the closer a required election, the more likely is the government to terminate. The model follows most closely that of Diermeier and Feddersen (1996), who compare a parliamentary system with a majority confidence procedure to a U.S.–style system, in which once formed the government cannot fall.[4]

THE MODEL

Using the approach introduced by Diermeier and Feddersen, the framework presented here includes periods of two types. The first is a government organization period in which a party is selected to form a government. If one is not formed, then the next period is another organization period. If a government formation proposal receives a majority vote, then the government is installed, and the second type of period commences. In this legislative period, a member of the governing coalition is selected as leader and makes a proposal to redistribute income. That proposal is voted against an exogenous reservation value, and the parties vote taking into account the consequences for the continuation of the government.[5] Attached to the proposal is a confidence or censure procedure that can result in the termination of the government, followed by an organization period.[6] An organization period can involve the reorganization of the government or an election, followed by a government formation process.

In every period, the economy is assumed to generate one unit of income or government revenue that may be redistributed by the parliament under majority rule. The interests of parties are assumed to be to redistribute income to their constituents, whether a geographic constituency or, in a proportional representation system, such interests as business, labor, consumers, environmental groups, or agriculture. Redistribution can take place only after a government has been formed in an organization period, which may be interpreted as an election and the formation process that follows. Electoral competition is not modeled, and in order to focus on coalition governments, the election outcome is assumed to be such that no party has a majority. In order to focus on the process of governing, the time until the next required election is initially assumed to be infinite, and then a finite constitutional interelection period is considered. Both government organization and legislation are viewed as bargaining games, as in Baron and Ferejohn (1989).

The objective is to study the effect of confidence and censure proposals on government formation, stability, and cohesion, so the model incorporates events that may put the government at risk or provide temptation for a government member to defect. The model does so by allowing the current income of a member's constituents to change exogenously. A government thus may

[4] Persson, Roland, and Tabellini (1996) also provide a comparison between presidential and parliamentary systems with a focus on the separation of powers.

[5] Strom (1990, 111–3) discusses some of the preference-related and institutional factors that can affect this legislative bargaining.

[6] Laver and Schofield (1990, 144–63) discuss this bargaining environment and relate it to the empirical literature.

change in one of two ways. First, a successful censure motion or the defeat of a legislative proposal to which a confidence motion is attached leads to an organization period. Second, a member of the government can withdraw from the coalition, resulting in the dissolution of the government. This may be in anticipation of a successful censure motion, but as Budge and Keman (1990, 59) argue, termination also may occur when the government parties are successful and united. For example, the government may anticipate electoral success, in which case termination is voluntary and based on opportunity.

The incentive to be in government results from its control of the agenda-setting process, which allows it to propose a redistribution of income.[7] This may be viewed as corresponding to the selection of a minister to make a proposal in his or her policy jurisdiction; to simplify the model, policy jurisdictions will not be differentiated.[8] Proposals by the opposition are considered in the context of censure motions.

The government members cannot commit to how they will redistribute income or to how they will vote on a proposal. That is, all actions must be rational at the time the member has an opportunity to act. Subgame perfect Nash equilibria will thus be characterized, and the focus is on stationary equilibria in which the same strategies are played in structurally equivalent subgames. Two subgames are structurally equivalent if and only if the sequence of moves is the same, the action sets are the same at each corresponding node, and the preferences of the players are the same in each period. The strategies in structurally equivalent subgames are assumed to depend only on a state that includes the payoff-relevant information in the history of play. A stationary equilibrium in this model is a stationary Markov perfect equilibrium, where the state σ_t at the beginning of a legislative period (t) is the government coalition, and the state at the beginning of an organization period is null.

To simplify the notation and facilitate the exposition, a basic model is presented first, and then extensions are considered. The parliament is assumed to have three parties, $i = 1, 2, 3$, no one of which holds a majority. As in Diermeier and Feddersen (1996), a purely distributive event occurs, which will be represented as a draw $x = (x_1, x_2, x_3) \in X \equiv \{x | x_i \geq 0, i = 1, 2, 3, \Sigma_{i=1}^3 x_i = 1\}$, where x is uniformly distributed over X, and the draws are independent over time. The events, which may be interpreted as fluctuations in the distribution of current income caused by the economy, represent the changing circumstances confronting the government.[9] Initially, aggregate current income is assumed to be a constant, which focuses the comparisons on the consequences of alternative institutional procedures.[10] Lupia and Strom (1995) incorporate a random event and interpret it as future electoral prospects that provide a temptation to terminate the government and hold an early election. In their examples, those prospects also sum to a constant.

A government consists of a coalition $C \in \mathbf{C}$, where $\mathbf{C}$ is the set of all coalitions, including those with only one party. In each legislative period, the government controls the agenda and can respond to the event x with a party $l \in C$ selected to set the agenda by making a proposal $b^l \in X$ for the redistribution of income.[11] Initially, the party will be selected at random, and then the probability of selection will be allowed to differ among the members of the government. If a majority of parliament votes for b^l against the alternative x, then the income is divided according to b^l.[12] Otherwise the income is distributed according to x.[13] A vote in a legislative period is an action from the set $Y = \{0, 1\}$, where 1 denotes a vote for b^l and 0 a vote for x. Voting is simultaneous, and the votes $y_i \in Y$, $i = 1, 2, 3$, result in enactment of b^l if and only if $\Sigma_{i=1}^3 y_i \geq 2$. Weakly dominated voting strategies conditional on the state are assumed to be eliminated.

Associated with the proposal b^l is a confidence or censure procedure as formalized below.[14] If b^l is enacted, then government C continues in office in the next legislative period. If b^l is defeated, and hence a vote of confidence fails or a censure motion succeeds, then the next period is an organization period. The agenda setter is also assumed to be able to withdraw from the government, resulting in its dissolution.[15] The process of responding to the event may be interpreted as intracoalitional bargaining with the opportunity to seek support from the out party. The sequence in a legislative period is illustrated in Figure 1, which does not depict the individual votes separately.[16] If b_l is

[7] To simplify the notation and make the analysis more transparent, organization and legislative periods are assumed to be of equal length. If an organization period encompasses an election and the subsequent government formation process, then it could be lengthy. Merlo (1997) reports that in postwar Italy, for example, the government formation process had a mean duration of four weeks and a maximum of thirty-nine weeks.

[8] Silk and Walters (1995) note that in the United Kingdom proposals are generally introduced by the minister in whose jurisdiction the bill is located.

[9] The economy may also generate other income, so x is to be interpreted as the income available for redistribution.

[10] This assumption is relaxed below.

[11] The model shares three features with the Lupia and Strom (1995) and Diermeier and Feddersen (1996) models. First, governing involves organization and legislative periods. Second, the government through its control of the agenda can respond strategically to events. Third, the events are exogenous.

[12] In the context of government expenditure programs, the proposal b^l can be interpreted as the distribution of spending, where x is the current-period reversion spending in the absence of new legislation.

[13] Amendments or counterproposals by the out party are considered below.

[14] For example, a confidence procedure may be invoked after the defeat of a government legislative proposal and hence is effectively attached to that proposal. Confidence and censure procedures are typically invoked only for major legislation, so the proposals considered here are intended to correspond to major legislation. Laver and Schofield (1990, 65–6), however, provide an example of a vote of confidence on the closing of a hospital.

[15] Laver and Schofield (1990, 64, Table 4.1) summarize dissolution authority for twenty countries.

[16] The model differs from that of Diermeier and Feddersen (1996) in three principal aspects. First, income is generated in both organization and legislative periods, which simplifies the notation and the analysis. Diermeier and Feddersen assume that there is income only

FIGURE 1. Legislative Period

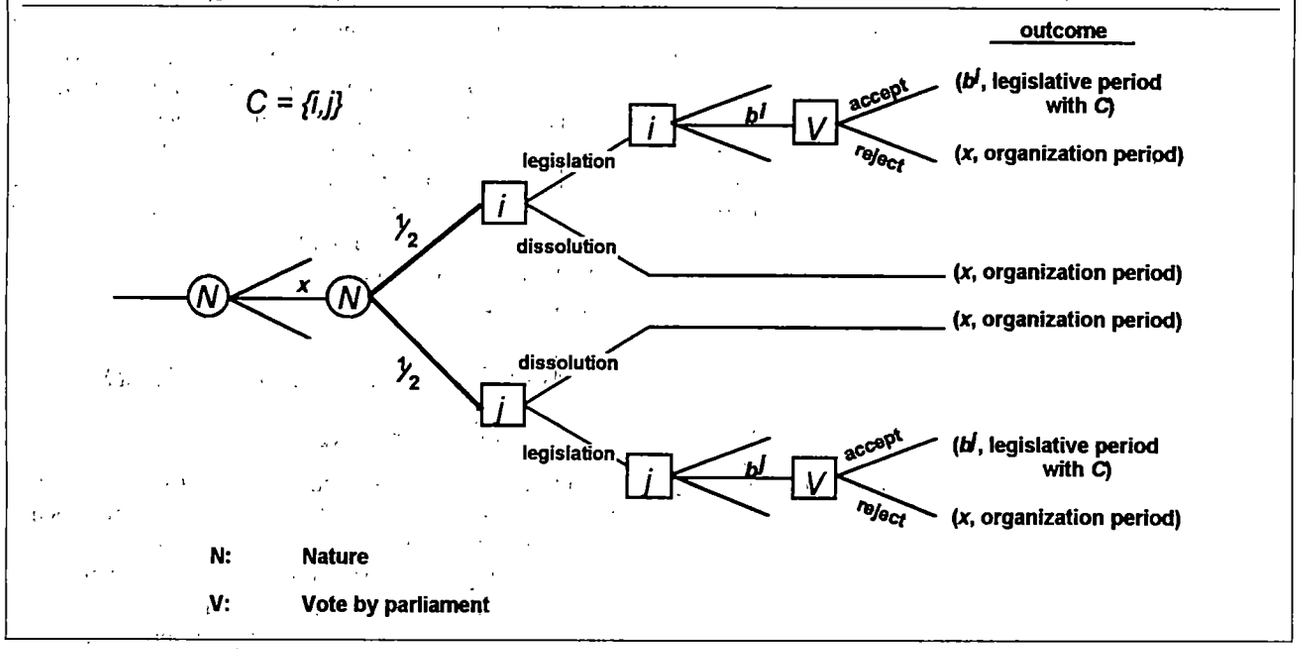
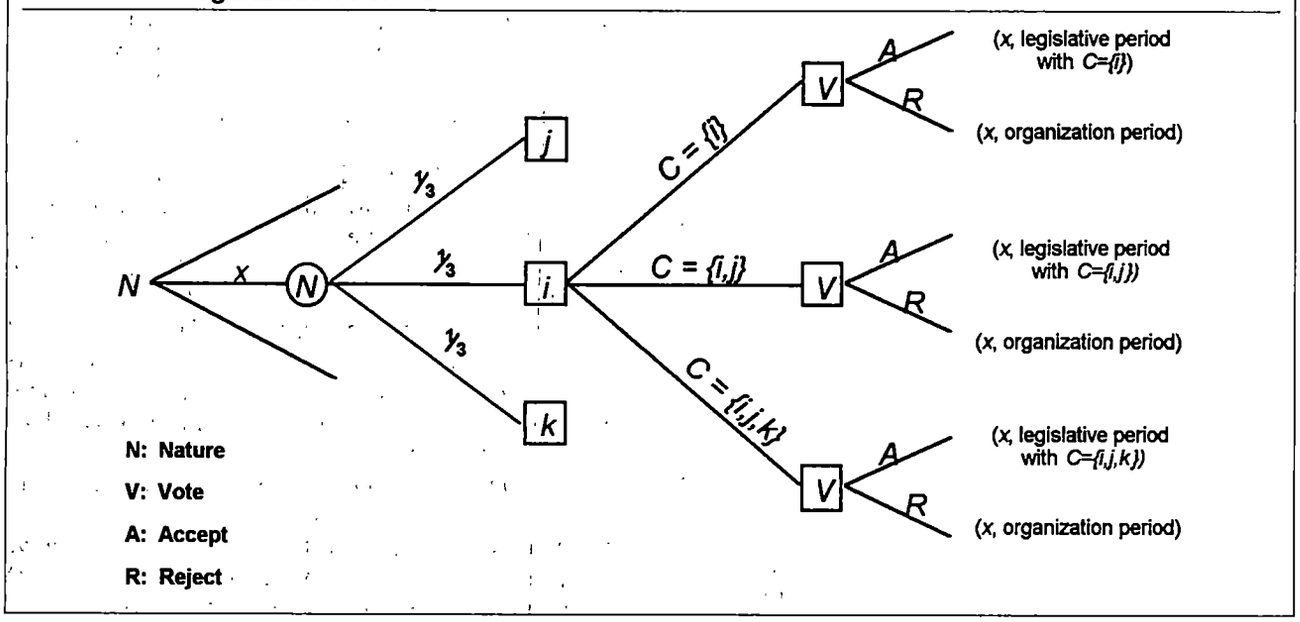


FIGURE 2. Organization Period



enacted in period t, then the state variable ($\sigma_{t+1} \in \mathbf{C} \cup \phi$) in the next period is $\sigma_{t+1} = \sigma_t = C$, and if b_l is defeated or a member of the government withdraws resulting in government dissolution, then $\sigma_{t+1} = \phi$, where ϕ denotes an organization period.

In an organization period, a caretaker government is assumed to be in place, and that government cannot redistribute income. As illustrated in Figure 2, nature first selects a distribution of income ($x \in X$) and then selects at random a party ($l \in \{1, 2, 3\}$) to act as a formateur to propose a government coalition ($C \in \mathbf{C}$). Since any two of the three parties is sufficient to form a government, the representation in parliament does not affect which government a formateur chooses. To preserve symmetry, the parties are assumed to have the same probability of being selected, so representation

in a legislative period, an assumption that works in favor of government stability and cohesion. Their assumption is considered below in the context of minority governments. Second, the model has an infinite number of periods, whereas Diermeier and Feddersen consider a finite horizon. That horizon allows them to use backward induction, whereas here a stationary equilibrium is characterized. Third, the model incorporates political impatience, whereas Diermeier and Feddersen assume that the discount factor is one, although the finite horizon has an effect similar to a lower discount factor. A high discount factor favors government stability and cohesion, as will be demonstrated below.

may be thought of as the same for each party. This assumption will be relaxed below. In an empirical study of government formation processes in twelve countries during the postwar period, Diermeier and Merlo (1998) found strong support for selection of the formateur in proportion to representation, with a sizable premium for incumbency. They found no support for selection of the largest party as formateur. If the C proposed by the formateur is supported by a majority, then that coalition is installed as the government, and the game enters a legislative period with $\sigma_{t+1} = C$. If the coalition proposal is defeated, then another government organization period ensues with $\sigma_{t+1} = \phi$. The model begins with a government organization period, so $\sigma_0 = \phi$.

The interests of a party (l) are assumed to be derived from officeholding preferences, which depend on serving constituents. Preferences are assumed to be time separable and risk neutral and are represented by a utility function, u_l:

$$u_l(z(\cdot)) = \sum_{t=1}^{\infty} \delta^t z_l(t),$$

where ($\delta \in [0, 1)$) is a common political discount factor, and income $z_l(t) = x_l(t)$ if $\sigma_t = \phi$; if $\sigma_t \in \mathbf{C}$, then

$$z_l(t) = \begin{cases} b_l(t) & \text{if } \sum_{i=1}^{3} y_i \geq 2, \text{ and } t \text{ is a legislative period,} \\ x_l(t) & \text{if } \sum_{i=1}^{3} y_i < 2, \text{ or } t \text{ is an organization period,} \end{cases}$$

where $b_l(t)$ and $x_l(t)$ are, respectively, the proposal and current income in period t. Preferences of the parties are thus diametrically opposed or polarized, so there is nothing in the configuration of preferences that contributes to government stability.

The political discount factor is intended to reflect the impatience of party leaders to distribute income to constituents. Such preferences may be derived from officeholding preferences of party leaders and the willingness of constituents to shift allegiance from one party to another in response to the income allocated to them. Party leaders thus prefer to deliver income sooner rather than later. Alternatively, constituents may support parties retrospectively, based on what was done for them in the last legislative period. Similarly, the impatience of party leaders may result from intraparty politics that give them a relatively small window of opportunity to lead, and delivering income to constituents is crucial to continuing their leadership. In this context, δ may be interpreted as the probability that a party leader is replaced by another party member.

Performance of a parliamentary government will be assessed in terms of its stability, the cohesion of legislative voting by its members, and the resulting distribution of income. Stability (s) will be measured by the probability that the members of the government in the current period constitute the government in the next period, so if a government member switches coalition partners each period, stability is one-half. The hazard rate (h) is defined as the probability that the government terminates in a period conditional on the time in office. Cohesion (c) is the probability that the members of a government vote together on legislation proposed by the government. The income distribution is measured by the expected income of the constituents of each party.

A MAJORITY CONFIDENCE PROCEDURE

Majority Governments

Under a majority confidence procedure, the government declares it will resign if its legislative proposal is defeated, and the political system then enters an organization period.[17] In the model presented here, the majority vote need not come solely from the governing coalition; that is, opposition party members can support the government on a legislative proposal.

A government coalition (C) is identified as two parties ($C = \{i, j\}$), and the opposition or out party will be identified by k. The equilibrium considered first has, on the equilibrium path of play, a government formed in the first period, and in each subsequent legislative period the government obtains majority support for its proposal, so the government remains in office. The equilibrium is driven by two majoritarian incentives. The first is to redistribute income among as few parties as possible. Since the government has agenda-setting powers, the incentive to preserve the government results from this majoritarian incentive. The second majoritarian incentive is for a government party to redistribute more income to its constituents than to those of other government parties. This majoritarian incentive provides temptation that can lead to a break in government cohesion.

If income is redistributed only to government members, then the continuation value (v_l^M, $l \in C$) for a stationary equilibrium of the subsequent legislative subgame, in which in each period the majority (^{M}) coalition constitutes the government, is by symmetry

[17] As in Huber (1996), this case corresponds to one in which no censure motion by the opposition can be expected. Huber considers a model in which members of the governing coalition make proposals, and then the prime minister invokes a confidence procedure; s/he chooses a proposal that all members of the coalition prefer to the alternative of terminating the government and conducting an election. Huber's model requires the support of all members of a government, which in the model considered here would result in both government stability and voting cohesion. This is the result of a dilemma faced by government partners who are compelled to support unfavorable legislation so as to maintain control of the agenda-setting process. This result is consistent with Huber's discussion of Prime Minister John Major's use of a confidence procedure to bring party members into line on the ratification of the Maastricht Treaty.

$$v_l^M = v^M \equiv \frac{1}{2(1-\delta)}, \; l = i, j. \quad (1)$$

In an organization period there is one unit of income, and since the parties are symmetric, the expected discounted future income in the subsequent legislative periods, once a government is formed, is $1/(3(1-\delta))$. Consequently, the expected income of a party (l) entering an organization period is $E(x_l) + \delta/(3(1-\delta)) = \frac{1}{3} + \delta/(3(1-\delta)) = 1/(3(1-\delta))$, which is the continuation value v_l^O; that is,

$$v_l^O = v^O \equiv \frac{1}{3(1-\delta)}, \; l = i, j, k. \quad (2)$$

The incentive for a member of government to remain in office is represented by the difference between v^M and v^O, or $v^M - v^O = 1/(6(1-\delta))$.

In making a proposal (b^i) in a legislative period, the agenda setter (i) can attract the vote of either the other government member (j) or the out party (k), since either vote preserves the government under a majority confidence procedure. Consider, first, a proposal to j. Assume that party k has a dominant strategy of voting against b^i, and i has a dominant strategy of voting for it. If j votes for $b^i = (b_i^i, b_j^i, b_k^i)$, then the government is preserved, and the continuation value is v^M in equation 1. Party j's expected income is then $b_j^i + \delta v^M = b_j^i + \delta/(2(1-\delta))$. If j votes against b^i, j's expected income is $x_j + \delta v^O = x_j + \delta/(3(1-\delta))$, since the government falls, and the political system enters an organization period. Party j thus votes for b^i if and only if

$$b_j^i \geq x_j - \frac{\delta}{6(1-\delta)}. \quad (3)$$

The agenda setter (i) has an incentive to offer as little as possible to j, or

$$b_j^{i*} = \max\left\{0, x_j - \frac{\delta}{6(1-\delta)}\right\}. \quad (4)$$

Then, i and j will vote for $b^{i*} = (1 - b_j^{i*}, b_j^{i*}, 0)$ against x. As identified above, the term $\delta/(6(1-\delta)) = \delta(v^M - v^O)$ in equations 3 and 4 is j's incentive to preserve the government for the future rather than enter an organization period. Because j has an incentive to remain in government, the income the agenda setter must provide to obtain j's vote is lower than it otherwise would be. The agenda setter partially captures this incentive when $x_j < \delta/(6(1-\delta))$ and fully captures it when $x_j \geq \delta/(6(1-\delta))$, as indicated in equation 4.

If j's current income (x_j) is high, then i must in equation 4 allocate substantial income to obtain its vote. The agenda setter could instead obtain k's vote, and the minimum income required would be

$$b_k^{i+} = x_k + \frac{\delta}{3(1-\delta)}, \quad (5)$$

since k has an incentive, represented by the term $\delta/(3(1-\delta))$, to vote for x and enter an organization period. The proposal is then $b^{i+} = (1 - b_k^{i+}, 0, b_k^{i+})$, where party j is allocated zero, and i retains $b_i^{i+} = 1 - b_k^{i+}$.

The agenda setter always prefers to preserve the government because control of the agenda-setting process is valuable, and both the proposals satisfying equations 4 and 5 preserve control. If $b_j^{i*} = 0$ in equation 4, then i prefers to obtain j's vote rather than k's vote, since $b_k^{i+} > 0$ with probability one. Next, consider the case in which $b_j^{i*} > 0$. Letting w^{il} denote i's income when l's vote is obtained, i prefers to obtain j's vote rather than k's vote if and only if

$$w^{ij} = 1 - b_j^{i*} + \delta v^M \geq w^{ik} = 1 - b_k^{i+} + \delta v^M,$$

or

$$\frac{\delta}{2(1-\delta)} \geq x_j - x_k. \quad (6)$$

This condition is satisfied for all x (e.g., $x_j = 1$ and $x_k = 0$) if and only if

$$\delta \geq \delta^* = \tfrac{2}{3},$$

in which case the voting cohesion of the government equals one. Note that the bound δ^* depends on the support of x and not on the distribution function.

The bound δ^* on the political discount factor reflects the coalition partner's interest in preserving the government, and that interest results from the government's control of the agenda-setting process. For $\delta \geq \delta^*$, the vote of the coalition partner is less costly than the vote of the out party, which has an incentive to vote against a proposal so as to enter an organization period in which it may have the opportunity to participate in the next government. Because of the partner's interest in preserving the current government, its support is less expensive than support from the out party when $\delta \geq \frac{2}{3}$.

The agenda setter also has the alternative of terminating the government and entering an organization period. As the following lemma indicates, the government will not terminate when the political discount factor is high.

LEMMA 1: *For $\delta \geq \frac{2}{3}$, under a majority confidence procedure the agenda setter prefers to obtain the vote of its coalition partner rather than terminate the government.*

Proof: The agenda setter (i) prefers to obtain j's vote rather than enter an organization period if and only if

$$w^{ij} = 1 - b_j^{i*} + \delta v^M = 1 - x_j + \frac{2\delta}{3(1-\delta)} \geq x_i + \delta v^O$$

$$= x_i + \frac{\delta}{3(1-\delta)}. \quad (7)$$

Since $x_i \leq 1 - x_j$, this condition is satisfied for all $x \in X$ and δ. *Q.E.D.*

Consequently, if $\delta \geq \frac{2}{3}$, in every legislative period the agenda setter offers

$$b^{i*} = (1 - b_j^{i*}, b_j^{i*}, 0); \tag{8}$$

coalition members i and j vote for it, and the out party (k) votes against it. The proof of lemma 1 also implies that the approach of required elections will not result in government termination under a majority confidence procedure when the political discount factor is high. For example, suppose that an election were required in the next period, in which case v^M would be replaced by v^O in equation 7. Then, since $x_i \leq 1 - x_j$, the agenda setter still prefers to obtain the coalition partner's vote rather than terminate the government.

When the future is very important relative to the present ($\delta \geq \frac{2}{3}$), the government remains in office in every period, so stability is $s = 1$, and the cohesion of the parliamentary government with a majority confidence procedure is $c = 1$. Stability is maximal because, when δ is high, the incentive to maintain control of the agenda-setting process by preserving the government is greater than the temptation to terminate the government, take the current income x_i, and enter an organization period. Cohesion is maximal because the agenda setter finds coalition member j's vote less expensive than the vote of the out party. This, however, is not a direct effect, since the incentive rests with j, who must choose between preserving the government and receiving $b_j^{i*} + \delta v^M$ or forcing a reorganization and receiving $x_j + \delta v^O$. By putting j in this position, i is able to capture some or all of j's incentive to preserve the government and its associated control of the agenda-setting process. This induces i to obtain the vote of j rather than that of k, even when j's current income (x_j) is high and k's is low.

When the future is less important relative to the present ($\delta < \frac{2}{3}$), however, the agenda setter has an incentive to seek the vote of the out party when coalition member j's reservation income (x_j) is high and x_k is low. In this event, k acts as a supporting party for the government.[18] Strom (1990, 110–1) refers to this as an ad hoc majority and relates its likelihood to institutional features, such as investiture requirements and legislative committee structure. If on the equilibrium path for some $x \in X$ the agenda setter obtains k's vote and the government is preserved, then k's continuation value (v_k) is positive. The continuation value of a member of the government thus is reduced to $1/2(1 - \delta) - v_k/2$. When the agenda setter solicits the vote of k with positive probability, k's vote is less costly. Also, j's vote is more costly because preserving the government is less valuable when an agenda setter solicits the support of the out party with positive probability. Party k will vote for an offer ($\hat{b}_k^i$) when

$$\hat{b}_k^i = x_k + \frac{\delta}{3(1-\delta)} - \delta v_k, \tag{9}$$

where the right-hand side is positive, since $v_k \leq 1/(3(1 - \delta))$. Proceeding as above, the condition corresponding to equation 6 is

$$\frac{\delta}{2(1-\delta)} - \frac{3\delta}{2} v_k \geq x_j - x_k, \tag{10}$$

where

$$\hat{b}_j^i = \max\left\{0, x_j - \frac{\delta}{6(1-\delta)} + \frac{\delta}{2} v_k\right\}. \tag{11}$$

The agenda setter obtains k's vote for all x such that equation 10 is not satisfied.

On the equilibrium path, i prefers to obtain j's or k's vote rather than terminate the government, as established in the following lemma.

LEMMA 2: *For no $x \in X$ and $\delta \in [0, 1)$ does the government terminate.*

Proof: Lemma 1 establishes this for $\delta \geq \frac{2}{3}$. Consider $\delta < \frac{2}{3}$, and suppose i obtains j's vote. The government will not terminate if w^{ij} is at least i's expected income ($x_i + \delta/(3(1 - \delta))$) from dissolution of the government. This requires

$$w^{ij} = 1 - \hat{b}_j^i + \left(\delta v^M - \frac{\delta}{2} v_k\right)$$

$$= 1 - x_j + \frac{2\delta}{3(1-\delta)} - \delta v_k \geq x_i + \frac{\delta}{3(1-\delta)},$$

or

$$1 - x_j - x_i \geq -\delta\left(\frac{1}{3(1-\delta)} - v_k\right).$$

Since agenda control is valuable, $v_k \leq \delta/(3(1 - \delta))$, and the inequality is satisfied for all $x \in X$. Thus, i prefers to obtain j's vote rather than to terminate the government. *Q.E.D.*

The out party (k) receives income for $x \in X$, such that equation 10 is not satisfied, and k's continuation value ($\hat{v}_k$) satisfies the recursion relation

$$\hat{v}_k = E_x \hat{b}_k^i + \delta \hat{v}_k, \tag{12}$$

where $E_x \hat{b}_k^i \equiv E_{x|x_j - x_k \leq 3\delta/(2(1-\delta)) - \delta/2\hat{v}_k} \hat{b}_k^i$ is the expected income of k in i's proposal.

Letting $\hat{v}_k$ be the value satisfying equation 12, if $\delta < \frac{2}{3}$ and if equation 10 is not satisfied, then in equilibrium the agenda setter offers

$$\hat{b}_k^i = x_k + \frac{\delta}{3(1-\delta)} - \delta \hat{v}_k \tag{9a}$$

[18] See Laver and Schofield (1990, 67) for a discussion of "support parties."

to k and otherwise offers $\hat{b}_j^i$ in equation 11 to j. In either event the government remains in office. The continuation values ($\hat{v}_l^M$) for the members of government satisfy the recursion relation

$$\hat{v}_l^M = \tfrac{1}{2}(1 - E_x\hat{b}_k^i) + \delta\hat{v}_l^M,\ l = i, j. \qquad (13)$$

For example, if $\delta = 0$, the agenda setter offers $\hat{b}_k^i$ in equation 9 to k with probability one-half, and $\hat{v}_k = \frac{1}{12}$. For $\delta = 0.5$, $\hat{v}_k = 0.1312$, and $\hat{v}_i^M = \hat{v}_j^M = 0.4344$. As δ increases, the probability that the agenda setter obtains the vote of the coalition partner increases and equals one for $\delta \in [\frac{2}{3}, 1)$.

The equilibrium is summarized in the following proposition.

PROPOSITION 1: *With a majority confidence procedure, if $\delta \geq \frac{2}{3}$, then the agenda setter (i) in government $C = \{i, j\}$ proposes b_j^{i*} in equation 4 for all $x \in X$, and parties i and j vote for the proposal, and party k votes against it. If $\delta < \frac{2}{3}$, then i offers $\hat{b}_j^i$ in equation 11 to j if equation 10 is satisfied and offers $\hat{b}_k^i$ in equation 9 to k if equation 10 is not satisfied. In the former case, i and j vote for the proposal, and k votes against it; in the latter case, i and k vote for the proposal, and j votes against it.*

The voting cohesion of the government can be determined with the aid of Figure 3. Voting is perfectly cohesive for $x \in \bar{X}^c(\delta) = \{x \in X | x_j - x_k \leq \delta/(2(1 - \delta)) - (3\delta/2)\,\hat{v}_k\}$, and it is less cohesive if $x \in \bar{X}^n(\delta) = X - \bar{X}^c(\delta)$. Cohesion ($c(\delta)$) is given by

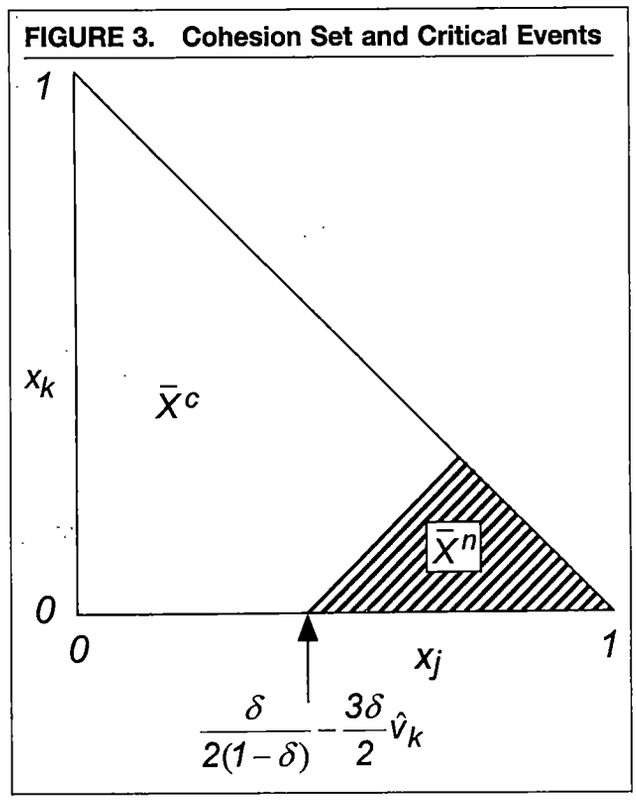
$$c(\delta) = Pr(x \in \bar{X}^c(\delta)) \qquad (14)$$
$$= 1 - 2\left(\frac{1}{2} - \frac{\delta}{4(1-\delta)} + \frac{3\delta}{4}\hat{v}_k\right)^2.$$

For $\delta = 0$, $c(0) = \frac{1}{2}$, since as noted above the agenda setter obtains the vote of k whenever $x_k < x_j$. For $\delta \in (0, \frac{2}{3})$, $c(\delta) \in (\frac{1}{2}, 1)$ and is increasing in δ. Even though cohesion is less than one, the government is completely stable ($s = 1$) under a majority confidence procedure. The hazard rate is thus zero.

This characterization of cohesiveness can also be used to define the set of critical events in which a government member votes against a government proposal and the out party preserves the government by voting for the proposal. As indicated in Figure 3, a critical event is one in which x_j is high and x_k is low, so the government partner's vote is expensive, and the out party's vote is inexpensive. The set $\bar{X}^n(\delta)$ of events critical for cohesion is thus composed of extreme events, and these are more extreme the higher is δ. Moreover, if x_j is high, then x_i for the agenda setter is low. This may be interpreted as conflict within the government. Cohesion thus is lower when there is conflict within the government and the out party's support can be obtained at a low cost.

The equilibrium distribution of income depends on δ. If $\delta \geq \frac{2}{3}$, then income is distributed only to members of the coalition, so their ex ante expected income

FIGURE 3. Cohesion Set and Critical Events



is v^M in equation 1. Conditional on party i being the agenda setter, party j receives b_j^{i*} in equation 4, which has an expected value

$$E_x b_j^{i*} = \int_{\delta/(6(1-\delta))}^{1} \left(x_j - \frac{\delta}{6(1-\delta)}\right) 2(1 - x_j)\, dx_j =$$
$$\frac{1}{3}\left(1 - \frac{\delta}{2(1-\delta)} + \frac{\delta^2}{12(1-\delta)^2} - \frac{\delta^3}{216(1-\delta)^2}\right), \qquad (15)$$

where $f(x_j) = 2(1 - x_j)$ is the density function of x_j. For example, if $\delta = \frac{2}{3}$, then $E_x b_j^{i*} = \frac{8}{81}$. The agenda setter receives $1 - E_x b_j^{i*}$, and party k receives zero. Income is redistributed in favor of the government members and particularly in favor of the agenda setter. If the political discount factor is less than $\frac{2}{3}$, k has a positive continuation value ($\hat{v}_k$) given in equation 12 and a positive expected payoff in the current period. As δ increases, k's income share increases as the cohesion of the government decreases. The expected incomes and continuation values ($\hat{v}^M$) in equation 13 of the constituents of the government members are thus lower when voting is less cohesive. That is, cohesion and the incomes of the government members' constituents are positively correlated.

The properties of the equilibrium are summarized in the following proposition.

PROPOSITION 2: *For all $\delta \in [0, 1)$, the government is perfectly stable ($s = 1$), and the hazard rate (h) is zero. For $\delta \geq \frac{2}{3}$, income is redistributed only to members of the government, and the expected current*

period incomes of i and j are $E_x b_j^{i*}$ in equation 15 and $1 - E_x b_j^{i*}$, respectively. For $\delta \geq \frac{2}{3}$, $c(\delta) = 1$. For $\delta < \frac{2}{3}$, $c(\delta) \in [\frac{1}{2}, 1)$ is given in equation 13 and is increasing in δ, and the expected current period income of k is positive.

Although the model has been formulated to make government stability difficult, a majority confidence procedure initiated by the government results in government stability. This is due to two institutional features of the model. First, the parliamentary system enables the government to control the legislative agenda, which creates an incentive for the government parties to preserve their control. If the out party can challenge that control by making a counterproposal, however, then the government can terminate, as will be demonstrated below. Second, under this confidence procedure the government is preserved by a majority vote. Without a confidence procedure, the government would change with the majority that supports a proposal; that is, the agenda setter would include in the next government the party that supported the current proposal, so cohesion and stability would be one-half.

Minority Governments

The preceding analysis pertains to a majority government, but in an organization period the formateur may form a minority government.[19] A minority government makes a proposal in every legislative period in which it remains in office, and to remain in office under the majority confidence procedure, party i must obtain in each period the vote of one other party. A party l, $l \neq i$, has a reservation value, $x_l + \delta/(3(1 - \delta))$, since rejection leads to an organization period. The agenda setter will make an offer (b_l^i) to the party with the lower reservation value, so

$$b_l^i = \min\{x_j, x_k\} + \frac{\delta}{3(1-\delta)} - \delta\bar{v}^o,$$

where $l \in \{j, k\}$ denotes the party with $x_l = \min\{x_j, x_k\}$, and $\bar{v}^o$ is the continuation value of a party that is out of government. The income b_l^i is nonnegative if $\bar{v}^o \leq 1/3(1 - \delta)$, which will be assumed and then verified below.

The minority government (i) has the choice in each legislative period of proposing $b^i = (1 - b_l^i, b_l^i, 0)$ or terminating the government, taking x_i, and entering an organization period for which the continuation value is $v^O = 1/3(1 - \delta)$. Party i thus will propose b^i if and only if

$$1 - b_l^i + \delta v^m \geq x_i + \frac{\delta}{3(1-\delta)},$$

or

$$1 - \min\{x_j, x_k\} - \frac{\delta}{3(1-\delta)} + \delta\bar{v}^o + \delta v^m \geq x_i + \frac{\delta}{3(1-\delta)},$$

where v^m is the continuation value for a minority government. This condition is satisfied for all x, since $1 - \min\{x_j, x_k\} \geq x_i$, and $\delta\bar{v}^o + \delta v^m = (\delta/2)(v^m + 1/(1 - \delta)) \geq 2\delta/(3(1 - \delta))$ when $v^m \geq 1/(3(1 - \delta))$.[20] Consequently, a minority government can make an acceptable proposal and hence stay in office with support from one of the other two parties.

The continuation value satisfies the recursion relation

$$v^m = 1 - E_x b_l^i + \delta v^m,$$

which has the solution

$$v^m = \frac{5 - 4\delta}{3(1-\delta)(2-\delta)}. \tag{16}$$

The continuation value of the other parties is thus

$$\bar{v}^o = \frac{1 + \delta}{6(1-\delta)(2-\delta)}, \tag{17}$$

and the expected income of the party that votes for the minority government's proposal is

$$E_x b_l^i = \frac{1 + \delta}{3(2 - \delta)}.$$

The continuation value (v^m) for a minority government is greater than the continuation value ($v^M = 1/(2(1 - \delta))$) for a majority government if and only if $\delta < \frac{4}{5}$.[21] Consequently, in an organization period, the formateur prefers a majority government if the future is very important ($\delta \geq \frac{4}{5}$) and a minority government if the future is less important. A minority government thus is both sustainable and preferred by the formateur in an organization period when $\delta < \frac{4}{5}$. Whether a minority government will be formed and installed in an organization period is considered next.

Government Organization

The formateur in an organization period will never propose a surplus or consensus government, $C = \{i, j, k\}$, since the value of that government is $1/(3(1 - \delta))$, whereas the value of a majority or a minority government is greater. That is, the formateur chooses not to share agenda control in a grand coalition.

As indicated above, the formateur prefers a majority government only if $\delta \geq \frac{4}{5}$. In that case, a majority

[19] For 1945–87, Strom identified 341 minority governments in fifteen countries with at least two such governments during that period. Forty-eight percent ended in a crisis or defeat on a confidence or censure procedure, and the others terminated as a result of an early election.

[20] The equality follows, since $v^m + 2\bar{v}^o = 1/(1 - \delta)$, and $v^m \geq 1/(3(1 - \delta))$ is implied by $\bar{v}^o \leq 1/(3(1 - \delta))$.

[21] The continuation value $\hat{v}^m$ is also greater than the continuation value $\hat{v}^M = v^M - \frac{1}{2}\hat{v}_k$ for $\delta < \frac{2}{3}$.

government will be installed, since a party invited to join the coalition has a continuation value of $v^M = 1/(2(1-\delta))$, whereas rejecting the offer results in a continuation value of $v^O = 1/(3(1-\delta))$.

If $\delta < \frac{4}{5}$, the formateur prefers a minority government. With the organizational process in Figure 2, however, a proposal to form a minority government will be rejected. To see this, suppose that i proposes a minority government. Party l can support that government and have a continuation value of $\bar{v}^o$ (equation 17) or reject the proposal and enter another organization period with a continuation value of $1/(3(1-\delta))$. Comparing these two indicates that

$$\bar{v}^o = \frac{1+\delta}{6(1-\delta)(2-\delta)} \leq \frac{1}{3(1-\delta)},$$

which is satisfied for all $\delta \in [0, 1)$.[22] A minority government will thus not receive the support needed for installation. This establishes the following proposition.

PROPOSITION 3: *With a majority confidence procedure, a minimal winning government forms.*

Consequently, only minimal winning coalitions result with a majority confidence procedure. This is also the case for the other confidence and censure procedures considered below.

A minority government could be installed, however, if the formation process were different from that in Figure 2.[23] Consider a model in which there is no income (x) in an organization period. If the formateur proposes a minority government and party l votes to install it, l's payoff will be $0 + \delta\bar{v}^o$. If l rejects the proposal, another organization period will commence, again with no x. The payoff to l will then be $0 + 0 + \delta^2/3(1-\delta)$, where $\delta^2/3(1-\delta)$ is the discounted continuation value from returning to the equilibrium path on which some government is formed. Then, using equation 17 and

$$\delta\bar{v}^o - \frac{\delta^2}{3(1-\delta)} = \frac{\delta(1+\delta)}{6(1-\delta)(2-\delta)} - \frac{\delta^2}{3(1-\delta)}$$
$$= \delta\frac{1-2\delta}{6(2-\delta)},$$

l will support a minority government if $\delta < \frac{1}{2}$. Consequently, a minority government can be formed and sustained in every legislative period in this variant of the basic model, since if l rejects a minority government, it will lose income in the following period. This is a sufficient incentive to allow a minority government to be formed when the political discount factor is low.

[22] This verifies the assumption that $\bar{v}^o \leq 1/(3(1-\delta))$, made in the previous section.

[23] A majority government will also form in an organization period if the current income can be redistributed as part of the government formation proposal.

Extensions of the Basic Model

The model may be extended in a number of directions without changing the qualitative properties of the results. Three extensions are considered in this section. The first relates to the number of parties in parliament. The second involves the share of parliamentary seats. The third concerns the distribution of current income. A fourth extension incorporating required elections is considered later.

An n-Party Parliament. Consider an n-party parliament. First, majoritarian incentives imply that the government formed is a minimal majority. Second, consider the bound $\delta^*(n)$ on the political discount factor, such that for all higher discount factors, income is redistributed only within the majority coalition; that is, cohesion equals one. Majoritarian incentives imply that the party recognized to make a legislative proposal will, for all $x \in X$, seek the votes of all its $(n-1)/(2$ coalition partners if and only if $\delta \geq \delta^*(n) \equiv (n+1)/(n+3)$. That is, the agenda setter redistributes income to a minimal majority of parties, and since the members of the governing coalition have an interest in preserving the government and its agenda control, their votes are less costly than the votes of the opposition. The bound $\delta^*(n)$ is a strictly increasing function of n with a limit of one. Parliaments are thus cohesive over a smaller set of political discount factors, the greater is the number of parties. Political systems with a larger n are thus less likely to be cohesive. With a majority confidence procedure, however, stability equals one for all n, since support from either the government or the out parties is sufficient to preserve the government.

Differences in Party Size. The members of the government coalition (C) could have different probabilities of being the agenda setter in a legislative period if the number of their portfolios or their seat shares differ. Browne and Franklin (1973) and Browne and Frendreis (1980) found that the number of ministries held is strongly proportional to seat share. In this model, seat share corresponds to the probability of being the agenda setter in a period. Suppose that $i \in C$ has probability p of being selected as the agenda setter, and $j \in C$ has probability $1-p$. Also, let $v_i^M(p)$ and $v_j^M(p)$ denote the corresponding continuation values in an equilibrium in which, for all $x \in X$, the agenda setter obtains the vote of the other government member. This requires that

$$\delta v_i^M(p) \geq x_j - x_k, \tag{6a}$$

and

$$\delta v_j^M(p) \geq x_i - x_k. \tag{6b}$$

As p increases, $v_i^M(p)$ increases, and $v_j^M(p)$ decreases, so i has a stronger and j a weaker incentive to remain in the government. This means that as p

TABLE 1. Continuation Values for a Majority Confidence Procedure

	$\delta = 0.9$			$\delta = 0.8$	
p	$v_i^M(p)$	$v_j^M(p)$	p	$v_i^M(p)$	$v_j^M(p)$
0.5	5.000	5.000	0.5	2.500	2.500
0.6	5.333	4.667	0.6	2.807	2.193
0.7	5.667	4.333	0.7	3.016	1.984
0.8	6.000	4.000	0.8	3.156	1.843
0.9	6.333	3.667	0.9	3.257	1.743
1.0	6.667	3.333	1.0	3.333	1.667

Note: The continuation values sum to $1/(1 - \delta)$.

increases, i must redistribute more income to obtain j's vote; when j is the agenda setter, it needs to redistribute less income to i to obtain i's vote. In equilibrium, when δ is such that equations 6a and 6b are satisfied for all $x \in X$, the continuation values satisfy the recursion relations

$$v_i^M(p) = p(1 - E_x b_j^i) + (1 - p)E_x b_i^j + \delta v_i^M(p) \quad \textbf{(1a)}$$

and

$$v_j^M(p) = pE_x b_j^i + (1 - p)(1 - E_x b_i^j) + \delta v_j^M(p), \quad \textbf{(1b)}$$

where

$$E_x b_j^i = \int_{\delta v_j^M(p) - \delta/(3(1-\delta))}^{1} \left(x_j - \delta v_j^M(p) + \frac{\delta}{3(1-\delta)}\right) \cdot 2(1 - x_j)dx_j = \frac{1}{3} - A_j + A_j^2 - \frac{1}{3}A_j^3,$$

$A_j = \delta v_j^M(p) - \delta/(3(1 - \delta))$, and $E_x b_i^j$ and A_i are defined analogously.

Then, defining $p^*(\delta)$ as the maximum p, such that equations 6a and 6b are satisfied, for $p^*(\delta) \geq p \geq \frac{1}{2}$ there is an equilibrium in which government stability and cohesion equal one and the hazard rate is zero. For $p \geq p^*(\delta)$, the equilibrium involves redistribution to the out party, and the characterization of the equilibrium is as above in equations 6–12. As examples, for $\delta = 0.8$ and $\delta = 0.9$, equations 6a and 6b are satisfied for all δ, so $p^*(\delta) = 1$, $\forall \delta \in [0, 1)$. The continuation values $v_i^M(p)$ and $v_j^M(p)$ are presented in Table 1, and as p increases, the continuation value of the party with the higher probability of being the agenda setter increases. If that probability reflects the seat share of the parties in the coalition, then the value of being in the government is increasing in a party's seat share.

These results can be used to extend the government formation results. Suppose, for example, that the various seat shares are such that $p_i > p_k > p_j$ and $p_l/(p_l + p_q)$ is less than $p^*(\delta)$ for all l, $q \neq l$. The formateur thus can choose between forming a government with either a large or a small party. Suppose that i is the formateur, the ministries are allocated in proportion to seat share, and the probability of being selected to make a legislative proposal equals that proportion. Then, i chooses between legislative periods in which its probability of being the agenda setter is $p^{ij} = p_i/(p_i + p_j)$ or $p^{ik} = p_i/(p_i + p_k)$. Since the former is greater than the latter, and the continuation value in Table 1 is increasing in p, the formateur forms a government with the smaller of the other two parties. The government thus includes the smallest party.

The Distribution of Current Income. The above characterization assumes that the distribution of current income is uniform on x and that the sum of current incomes equals one. To investigate the comparative statics of the distribution of income, note that the condition in equation 6 for cohesion to equal one, the condition in equation 7 for stability to equal one, and lemma 2 depend only on the support for the distribution. Consequently, if the distribution of x is such that $Ex_l = \frac{1}{3}$ and $x_l \in [0, 1]$, $l = i, j, k$, then the results in propositions 1 and 2 hold with the exception that the continuation value v_k depends on the probability distribution.

To explore the robustness of the assumption that aggregate income equals one, consider an alternative model in which the current incomes (x_l) have support $[0, x^+]$ and a mean $Ex_l = \frac{1}{3}$.[24] This could represent fluctuations in aggregate economic activity, with incomes high in a growth period and low in a recession. For example, suppose that x_l, $l = i, j, k$, has a triangular density function, $f(x_l) = 8x_l$ if $x_l \in [0, \frac{1}{2}]$, and $f(x_l) = 0$ otherwise. Then, the government never terminates, and cohesion is one for all $\delta \geq \frac{1}{2}$.

For other distributions, however, voluntary termination of the government may occur. Letting v_k' denote k's continuation value, termination results if either

$$x_j - x_k \leq \frac{3\delta}{2}\left(\frac{1}{3(1-\delta)} - v_k'\right) \text{ and } x_i \geq 1 - x_j + \delta\left(\frac{1}{3(1-\delta)} - v_k'\right), \quad \textbf{(18)}$$

or

$$x_j - x_k > \frac{3\delta}{2}\left(\frac{1}{3(1-\delta)} - v_k'\right) \text{ and}$$

[24] Restricting the means to equal $\frac{1}{3}$ allows the expressions for the continuation values in equations 1 and 2 to be used.

$$x_i \geq 1 - x_k - \frac{\delta}{2}\left(\frac{1}{3(1-\delta)} - v'_k\right), \qquad (19)$$

where the first inequalities correspond to equation 6 and the second to equation 7. Examination of these conditions indicates that termination occurs when δ is low and when the incomes x_i and either x_j or x_k are high.

As an example, if $f(x_l)$ is uniform on the interval $[0, \frac{2}{3}]$, then cohesion equals one if $\delta \geq \frac{4}{7}$, and stability equals one if $\delta \geq \frac{1}{2}$. For lower discount factors, however, the government voluntarily terminates with positive probability. As another example, if $f(x_l) = 2(1 - x_l)$ for $x_l \in [0, 1]$, then cohesion equals one for $\delta \geq \frac{2}{3}$, but stability equals one only for $\delta \geq \frac{3}{4}$. For discount factors below $\frac{3}{4}$, the government voluntarily terminates with positive probability.

Termination results when current income is high, since in that case the agenda setter has a strong temptation (x_i high) to terminate the government and a weak incentive ($1 - x_j$ or $1 - x_k$ low) to attract the support of another party to continue the government. In contrast, when the sum of current income is restricted to equal one, a strong temptation (x_i high) to terminate the government is necessarily accompanied by a strong incentive ($1 - x_j$ or $1 - x_k$ low) to continue the government. As lemma 2 indicates, the latter is always stronger than the former.

The case in which the aggregate income is not a constant may be interpreted as representing fluctuations in economic activity. For example, suppose that $x_l = x'$, $l = i, j, k$, where x' is uniformly distributed on $[0, \frac{2}{3}]$. Then, termination occurs only if $\delta < \frac{1}{2}$ and $x' > \frac{1}{2}$. For termination to result, the government must value highly the present relative to the future, and when that is the case, the government terminates in good times and remains in office in bad times. This is consistent with Budge and Keman's (1990) observation that termination may occur when the government parties are strong. This provides an explanation for calling early elections. It also indicates that fluctuations in the economy can affect government duration and termination.

The results from these extensions are summarized in the following proposition.

PROPOSITION 4: *With a majority confidence procedure, (1) cohesion is lower, the greater the number of parties; (2) if seat share increases the probability of being the agenda setter, the value of being in government increases with seat share, and to increase its agenda-setting probability, the formateur will choose as partner the party with the smaller seat share, so the smallest party is in government with probability one; and (3) the government may voluntarily terminate or call an early election, which occurs in good and not bad economic times for the agenda setter and is more likely when the parties are more politically impatient.*

OPPOSITION MOTIONS OF CENSURE

To maintain consistency with the models above, a motion of censure or no confidence made by the opposition will be specified as attached to a counterproposal or an amendment by the opposition.[25] That counterproposal may be thought of as an alternative to the government's proposal. Assume that i in the current government $C = \{i, j\}$ makes proposal b^i to attract j's vote under a majority confidence procedure. To defeat the government, the out party (k) can make a counterproposal (b^k) as a motion of censure and attract j's vote. The parliament votes on the two proposals and then on the winner versus x. If the motion of censure (b^k) fails and b^i defeats x, then the current government remains in office. The expected value for j is then $b^i_j + \delta\hat{v}^c$, where $\hat{v}^c$ is the continuation value of a coalition member in a system with a censure procedure. If the motion of censure succeeds, then the next period is an organization period, and the expected value for j is $b^k_j + \delta/(3(1 - \delta))$. Initially, the equilibrium considered has failure of any censure motion and hence preservation of the government; that is, a bound on the political discount factor will be found such that, for all higher δ, the out party will be unable to offer a successful censure motion for any $x \in X$. Thus, $\hat{v}^c = 1/(2(1 - \delta))$.

In its counterproposal, k is willing to redistribute all the current period income ($b^k_j = 1$) to obtain j's vote on the motion of censure, since its only opportunity to be in government is to unseat the incumbent. Such a proposal would not be credible, however, since on final passage k would vote for x over b^k for any $x_k > 0$. That is, both voting for b^k and voting for x lead to an organization period, so when $x_k > 0$, k will vote for x, which yields income x_k in the current period, over b^k, which offers no income. Hence, the most k can credibly offer to j is $b^k_j = 1 - x_k$. To preclude j's support for a motion of censure, the government agenda setter (i) thus must provide j with income $\hat{b}^i_j$, given by

$$\hat{b}^i_j = \max\left\{0, 1 - x_k - \frac{\delta}{6(1-\delta)}\right\}. \qquad (20)$$

The right-hand side of equation 20 is zero for all x_k if $\delta \geq \frac{6}{7}$ and is strictly less than one, so i can preclude a successful censure motion if the political discount factor is sufficiently high.

Although a successful censure motion attached to a counterproposal can be precluded by the initial choice of proposal b^i, a censure motion can cause the government to terminate. That is, if $\hat{b}^i_j$ in equation 20 is very costly or the current income x_i is very attractive, i could voluntarily terminate the government, resulting in an

[25] An alternative procedure would be a motion of censure by the opposition with no counterproposal. That is, if the motion is successful, then x is distributed in the current period, and an organization period follows. Such a motion would be defeated by either the proposal b^{i*}_j in equation 4 or the proposal with b^{i+}_k in equation 5, so the equilibrium would be the same as that with a majority confidence procedure.

organization period and income $x_i + \delta/(3(1-\delta))$. The agenda setter thus will offer $\hat{b}_j^i$ rather than terminate the government if and only if

$$1 - \hat{b}_j^i + \frac{\delta}{2(1-\delta)} \geq x_i + \frac{\delta}{3(1-\delta)},$$

or

$$\hat{b}_j^i \leq 1 - x_i + \frac{\delta}{6(1-\delta)}. \quad (21)$$

The condition in equation 21 is satisfied for all δ if $x_k \geq 1 - \delta/(6(1-\delta))$, since in that case $\hat{b}_j^i = 0$. If $\hat{b}_j^i > 0$, then equation 21 is

$$1 - x_k - \frac{\delta}{6(1-\delta)} \leq 1 - x_i + \frac{\delta}{6(1-\delta)},$$

or

$$x_i - x_k \leq \frac{\delta}{3(1-\delta)}. \quad (22)$$

The maximum of the left-hand side is one, and if $\delta \geq \frac{3}{4}$, this is satisfied for all x. The agenda setter then proposes $\hat{b}_j^i$, and no censure motion is made.

If $\delta < \frac{3}{4}$, however, then i prefers to terminate the government for some $x \in X$. Letting $\hat{v}^c(\delta)$ be the continuation value for a government party, the condition analogous to equation 22 is

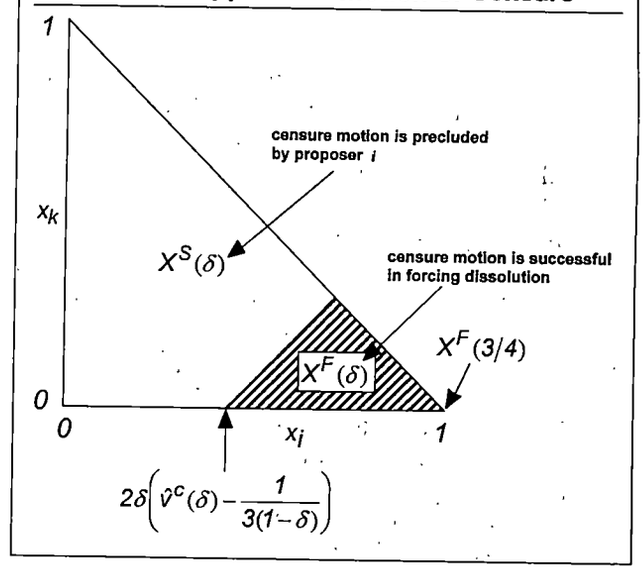
$$x_i - x_k \leq 2\left(\delta\hat{v}^c(\delta) - \frac{\delta}{3(1-\delta)}\right). \quad (23)$$

If this condition is not satisfied, i terminates the government rather than be defeated on a motion of censure. The motion of censure attached to b^k thus never comes to a vote, since the agenda setter terminates voluntarily. It is not the censure motion that i seeks to avoid, since both termination and successful censure result in an organization period. Instead, i seeks to avoid the counterproposal that gives zero to i.[26] A motion of censure thus can be effective in forcing government dissolution.

As illustrated in Figure 4, a censure motion forces dissolution when equation 23 is not satisfied; that is, for $x \in X^F(\delta) \equiv \{x | x_i > x_k + 2(\delta\hat{v}^c(\delta) - \delta/(3(1-\delta))\}$, the agenda setter terminates the government. For $x \in X^S(\delta) \equiv X - X^F(\delta)$, both i and j vote for $\hat{b}^i$ against b^k and against x, so the government is sustained. The stability (s) of the government under a censure procedure is thus in the interval $[\frac{1}{2}, 1]$, and the hazard rate (h) is $1 - s$.[27] For example, if $\delta = 0$, then $s = c = \frac{1}{2} = h$.

In contrast to the case of a majority confidence procedure illustrated in Figure 3, the critical events—those that lead to the dissolution of government—are

FIGURE 4. Opposition Motion of Censure



extreme in the sense that x_i is high relative to x_k. This indicates that it is a combination of opportunity (a high x_i) and a credible threat (a low x_k) that provides the incentive to terminate the government.[28] The success of a censure motion hinges on both. First, for a chance to be in the next government, the out party is willing to offer as much current income as is credible to enter an organization period. Second, although the agenda setter can always offer more than the out party, if the required offer becomes too expensive, it may prefer to terminate the government, take the current income, and enter an organization period. This may be interpreted as calling an early election. This analysis thus indicates that an early election is called when economic times are good for the agenda setter (x_i high) and bad for the out party. Warwick (1992) and Merlo (1997) found that governments are more likely to terminate in bad times than in good. The prediction of my theory is consistent with their findings if the bad times are for the constituents of the out party and the government's partner.

A censure motion attached to a counterproposal thus threatens government stability and cohesion unless the political discount factor is high ($\delta \geq \frac{3}{4}$). In that case, the incentive to maintain control of the agenda-setting process is sufficiently strong for all x that the agenda setter prefers to offer its coalition partner enough to cause it to reject any censure proposal. The government members then capture all the income. For a lower discount factor ($\delta < \frac{3}{4}$), the censure procedure can cause the government to terminate with positive probability, because precluding the motion can be more costly for the agenda-setter than calling an early election when times are good.

The equilibrium with a censure or no-confidence procedure is summarized in the following proposition.

[26] Party k cannot gain by offering an increment (a) to i, since a must be taken away from j.

[27] Cohesion is not defined when the agenda setter terminates the government, so c here is the probability of termination.

[28] Since x_i is high, x_j must be low, so termination results when there is conflict within the government.

PROPOSITION 5: *For $\delta \geq \frac{3}{4}$, the possibility of a censure motion attached to a counterproposal results in the government party proposing $\hat{b}_j^i$ in equation 20 for all x; both coalition members i and j vote for the proposal, and k votes against it. For $\delta < \frac{3}{4}$, dissolution of the government may result, since the agenda setter voluntarily terminates the government if equation 23 is not satisfied. If $\delta \geq \frac{3}{4}$, the stability and cohesion of the government are one, the hazard rate is zero, and income is redistributed only to members of the government. If $\delta < \frac{3}{4}$, stability and cohesion are less than one, and $h = 1 - s \in [0, \frac{1}{2}]$. The out party then receives income x_k with positive probability.*

REGULARLY SCHEDULED ELECTIONS

Regularly scheduled elections place the government's control of the agenda-setting process in jeopardy and hence lessen the value of that control. This reduces the incentive of a government member to preclude a censure motion, since the future value from preserving the government is reduced by the approaching required election. An election may be required every T periods or at T periods after the previous election. The equilibria corresponding to these two cases are similar, so the analysis will assume a required election at T legislative periods after a government is formed either as a result of mandatory election or of voluntary dissolution. As above, in an election it is assumed that no party receives a majority in the parliament, and hence each party is equally likely to be in the next government.

For a given constitutional interelection period T, let $\tau \leq T$ be the number of periods remaining until the next election, where the length of a period depends on how rapidly events occur. The equilibrium to be characterized is one in which the current government precludes a censure motion that would result in its dissolution. If the motion were successful, then the continuation value would be $v^O = 1/(3(1-\delta))$. If the current government were to remain in office until the next election, the continuation value would be

$$v^c(\tau) = \frac{\delta}{2} + \frac{\delta^2}{2} + \cdots + \frac{\delta^\tau}{2} + \frac{\delta^{\tau+1}}{3(1-\delta)}$$

$$= \frac{\delta}{2}\left(\frac{1-\delta^{\tau-1}}{1-\delta}\right) + \frac{\delta^{\tau+1}}{3(1-\delta)}.$$

The agenda setter can preclude a censure motion if and only if

$$\hat{b}_j^i \geq 1 - x_k - \frac{\delta}{3(1-\delta)} + \delta v^c(\tau)$$

$$= 1 - x_k - \frac{\delta}{6}\left(\frac{1-\delta^{\tau-1}}{1-\delta}\right). \quad \textbf{(20a)}$$

The agenda setter will preclude a censure motion rather than terminate if and only if

$$1 - \hat{b}_j^i + \frac{\delta}{2}\left(\frac{1-\delta^{\tau-1}}{1-\delta}\right) + \frac{\delta^{\tau+1}}{3(1-\delta)} \geq x_i + \frac{\delta}{3(1-\delta)},$$

or

$$x_i - x_k \leq \frac{\delta - \delta^\tau}{3(1-\delta)}. \quad \textbf{(22a)}$$

This condition is satisfied for all $x \in X$ if and only if the right-hand side of equation 22a is at least one. Let $\hat{\delta}(\tau)$ satisfy this condition as an equality, or

$$1 \equiv \frac{\hat{\delta}(\tau) - \hat{\delta}(\tau)^\tau}{3(1-\hat{\delta}(\tau))}. \quad \textbf{(24)}$$

For $1 > \delta \geq \hat{\delta}(\tau)$, government dissolution will not occur with τ periods remaining before the next required election. A solution $\hat{\delta}(\tau) < 1$ to equation 24 exists only if $\tau > 4$, and that solution is strictly decreasing in τ with a limit of $\frac{3}{4}$, which is the bound characterized above using equation 22 for a censure procedure with no required election. Table 2 presents values of $\hat{\delta}(\tau)$. To interpret the table, consider a parliamentary system characterized by $\delta = 0.8$, and suppose that $\tau = 10$. Since $\delta > \hat{\delta}(10) = 0.7678$, the condition in equation 22a is satisfied, so the probability of termination in the current period is zero. This is also the case for $\tau = 9$ and $\tau = 8$, but for $\tau = 7$ the probability is positive. As τ decreases, the probability of termination increases until $\tau = 4$.

The probability of termination viewed as a function of τ may be interpreted as a strategic hazard rate, defined by

$$h(\tau) = Pr\left\{x \in X \middle| x_i - x_k > 2\delta\left(v^c(\tau - 1) - \frac{1}{3(1-\delta)}\right)\right\} \in \left[0, \frac{1}{2}\right].$$

For $\tau = 1$, $v^c(1) = 1/(3(1-\delta))$, so $h(1) = \frac{1}{2}$ for all δ. For $\tau = 2$, $v^c(2) = \frac{1}{12} + \delta v^c(1)$, and $h(2) = \frac{1}{2} - \frac{\delta}{6} + \delta^2/72$. For example, if $\delta = 0.8$, $h(2) = 0.3756$. For τ such that $h(\tau) > 0$, the continuation value $v^c(\tau)$ for a member of government is less than $v^M = 1/(2(1-\delta))$.

This characterization provides two predictions for

TABLE 2. Bound on δ

τ	$\hat{\delta}(\tau)$
1.0–4.0	1.0
4.25	0.9628
4.5	0.9328
4.75	0.9084
5	0.8882
6	0.8343
7	0.8046
8	0.7867
10	0.7678
100	0.75

parliamentary governments. First, voluntary dissolution can occur as a result of the approaching required election. A government is thus more likely to call an early election when the present is more important relative to the future and as a scheduled election draws closer. Voluntary dissolution occurs, however, only when the economic prospects (x_i) of the constituents of the agenda setter are good. Second, the hazard rate increases as the required election approaches. King et al. (1990) found that although the probability of government termination decreases over time, it increases at approximately a year before a required election. Beyond that point there may be little reason to call an early election.

The hazard rate estimated in the empirical literature is based on the time since government formation, but the model considered here indicates that the strategically relevant measure is the number of periods remaining before the next required election. Moreover, the strategic hazard rate is not constant; it increases as a required election approaches.[29]

CONCLUSION

Parliamentary systems differ in their institutions, and those differences provide a basis for comparison. In the model considered here, that comparison supplements studies which focus on differences in voter preferences, history, and culture. This model of parliamentary government incorporates government formation, legislation, and the possibility of government reorganization or dissolution. It focuses on distributive politics, that is, government spending or income redistribution. Consistent with the empirical literature, it incorporates attributes of the political system in the form of parliamentary procedures for terminating a government, events that may or may not be critical for termination, and strategies of both the government and opposition parties in responding to those events.

The model is formulated to make government stability difficult, but governments are stable when the future is important relative to the present. This stability results not from party alignments or continuity based on past coalitions. Instead, it originates in the parliamentary institution. The installation of a government through majority support in the parliament is accompanied by a substantial degree of agenda control, and this control allows the government to serve the constituents of coalition members. The prospect of continued agenda control provides an incentive to preserve the government.

A majority confidence procedure is effective in preserving a government, since a government member prefers to retain agenda control for all events that may occur. Voting cohesion, however, may erode for extreme events in which the agenda setter seeks the support of the out party on legislation. Moreover, cohesion is lower, the larger the number of parties in parliament. Challenges to this agenda control through censure motions by the opposition can result in instability and government dissolution, however. Changes in government and the weakening of voting cohesion also result if the parties or their leaders are politically impatient, since the incentive to pursue current opportunities created by the events is then stronger. Government dissolution thus results under two types of conditions. First, challenges by the opposition in the form of a censure or no-confidence motion can force the government to terminate in anticipation of a defeat on the motion. Second, termination can also result from fluctuations in aggregate income. In both cases termination occurs in good rather than bad times for the constituents of the agenda setter, and thus in bad times for the government partner and out party.

The model also provides a formal characterization of critical events. An event is critical if it leads to a change in government or a breakdown in voting cohesion. A critical event is extreme, and the set of critical events depends on the political patience of the parties. The greater the impatience, the larger is the set of potentially critical events and hence the less stable or cohesive is the government. A hazard rate of government termination is also endogenously derived. It varies with the confidence or censure procedure, but it is constant for each party in the absence of required elections. During a constitutional interelection period, the hazard rate increases as a required election approaches. The causal relationship between the hazard rate and government continuation thus does not hinge on time in office but on the time until the next election.

The government that forms in an organization period consists of a minimal majority of the parties. This results from three factors. First, only majority support is required to install a government. Second, the majoritarian incentive to redistribute income among as few coalition partners as possible implies that the formateur prefers to share agenda control with as few others as possible. Consequently, a consensus or surplus government never forms. Third, other parties will not support a minority government because they have an interest in sharing control of the agenda. If the probability of being the agenda setter in a legislative period is proportional to a party's seat share in the government coalition, the formateur forms a party with the smaller of the other two parties. A minority government can form during an organization period if there is no income, since the other parties want to form a government quickly to avoid continued loss of income.

The theory has a number of implications for the empirical study of parliamentary governments. First, the nature and ease of use of confidence and censure procedures affects legislation as well as government stability and cohesion, and thus they should be included in empirical studies. Second, the probability of government termination depends on other institutional features, such as the constitutional interelection pe-

[29] This observation is also made by Diermeier and Stevenson (1996).

riod, and in the context of distributive politics the hazard rate increases as a required election approaches. Third, the importance of future considerations, such as a required election, for a current government depends on the political discount factor, and greater political impatience implies lower government stability and cohesion and a broader redistribution of income or government spending. Fourth, events and the ability of a government to adjust to them affect stability, cohesion, and income redistribution. Critical events are extreme and provide temptation as well as threats from the opposition. The set of critical events depends on the form of the confidence and censure procedures as well as on the political discount factor. Fifth, government termination or the calling of an early election can result from fluctuations in aggregate economic activity. In a stable economy, termination is the result of challenges by the opposition. In a volatile economy, termination also can occur voluntarily when times are good for the agenda setter. Sixth, with an organizational process similar to that in the model, only minimal majority governments form. Moreover, the formateur has an incentive to form a coalition with the smaller of the other parties if legislative agenda control is a function of seat shares in the coalition. Minority governments can form with other organizational processes. Other attributes of the political system, such as party configuration and polarization, and institutional features, such as the electoral system, also affect outcomes, and hence the empirical study of parliamentary governments and their termination should be set in the broader context of parliamentary systems.

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Cohesion in Legislatures and the Vote of Confidence Procedure

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We present a framework to analyze the effects of constitutional features on legislative voting with respect to cohesion and the distribution of payoffs. We then apply this framework to parliamentary democracies and show how a prominent feature of decision making in parliaments, the vote of confidence procedure, creates an incentive for ruling coalitions to vote together on policy issues that might otherwise split them. The key feature that creates cohesive voting is the fact that votes on bills are treated as votes on who controls floor access in future periods. As a consequence, legislative majorities capture more of the legislative rents from the minority in parliamentary democracies than in nonparliamentary settings.

A central difference in legislative behavior between the U.S. Congress and parliamentary systems is the difference in voting cohesion. Members of parliamentary coalitions persistently vote together on different policy issues, while coalitions in the U.S. Congress are issue specific, change frequently, and often cross party lines (e.g., Collie 1985; Loewenberg and Patterson 1979; Mezey 1979; von Beyme 1985).[1] A common explanation for cohesion is that legislators have very similar preferences regarding policy issues. The question is why this is the case in parliamentary democracies but not in the U.S. Congress.

There are various possible answers. One obvious candidate is that political parties are stronger in parliamentary democracies than in the U.S. Congress. Parties in parliamentary democracies often control the nomination or deselection of candidates and therefore can provide incentives for legislators to vote according to the party line. Indeed, voting cohesion in legislatures has often been used as a measure of party strength (e.g., Collie 1985).

Party strength may explain cohesion among members of the same party, but it cannot account for the cohesion observed in multiparty settings. In parliamentary democracies cohesion *across parties* is significantly higher than the level of cohesion among members *of the same party* in the U.S. Congress (Loewenberg and Patterson 1979; Mezey 1979; Von Beyme 1985). So, while a party-based theory of cohesion may be able to account for differences in cohesion between the British House of Commons and the U.S. Congress, it cannot explain why the members of the governing coalition in the German Bundestag (comprised of two parties) vote more cohesively than, say, members of the Republican Party in the U.S. Congress.

This suggests an institutional explanation for voting cohesion that relies on the incentives created by the characteristic features of parliamentary constitutions. Such features include cabinet government, the ability of the prime minister to call for early elections, the vote of no confidence, and extralegislative bargaining mechanisms (Epstein 1964). In this article we provide a game-theoretic explanation for cohesion that focuses on one of the most prominent institutional features of parliamentary democracies: the vote of confidence procedure. The confidence procedure allows a ruling coalition to propose a bill without allowing amendments and to link the adoption of the bill with the survival of the coalition (Huber 1996).[2] It thus creates an incentive for all those who profit from the current government to vote for the government's proposals.

In order to develop a formal model of cohesive voting under a vote of confidence procedure, we need a model with two features. First, it must include a series of legislative proposals and votes. Second, it must capture the consequences of rejecting a bill with a confidence motion attached. The basic structure of our model is similar to the well-known legislative bargaining model of Baron and Ferejohn (1989) and the proposer model of Romer and Rosenthal (1978). In these frameworks a legislator makes a proposal that is accepted if and only if a majority of the legislators vote in favor. Otherwise, players receive a reservation value. As these models demonstrate, proposal rights are valuable, since they allow the proposer to extract benefits from other legislators.

In all legislatures some members enjoy special procedural prerogatives. In the U.S. Congress the Speaker and other party leaders along with the committee chairs have considerable control over the legislative agenda by using gate-keeping authority, closed rules, and the right to offer counteramendments. In parliamentary systems, while committees are relatively weak (Lees and Shaw 1979), cabinet members have impor-

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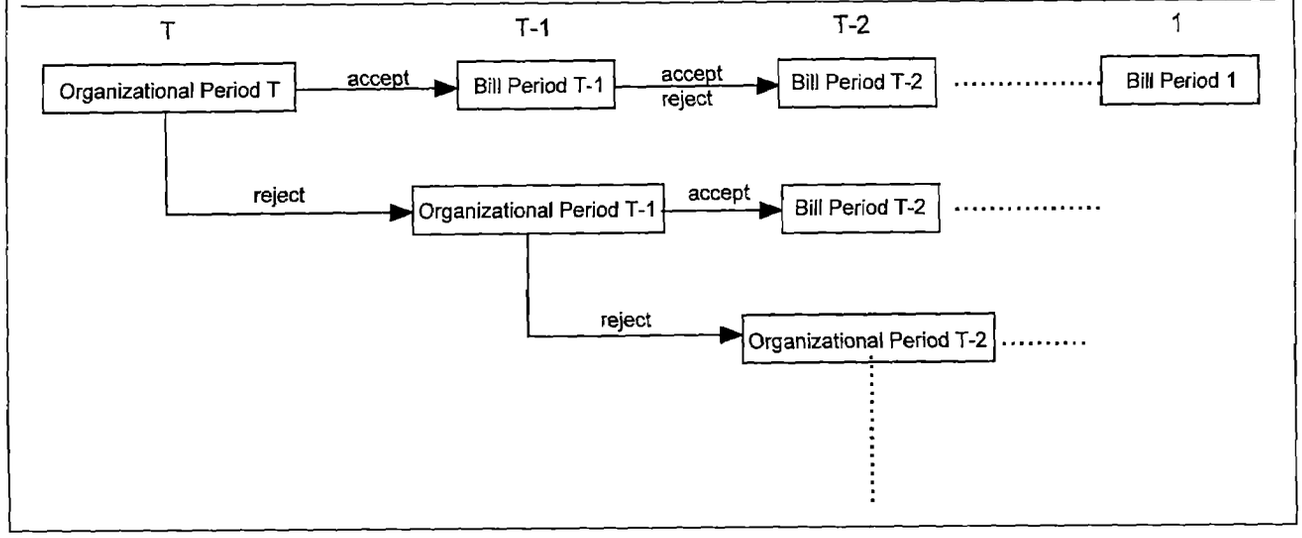
We thank David Baron, Michele Boldrin, John Carey, Gary Cox, John Huber, Ken Janda, Roger Myerson, Wolfgang Pesendorfer, and Al Slivinski. We also thank participants in the 1996 CEPR Summer Workshop in Theoretical Economics at Gerzensee, Switzerland; in the 1996 Annual Conference of the W. Allen Wallis Institute of Political Economy at the University of Rochester; and in seminars at Stanford University, Columbia University, the University of Michigan, and Northwestern University. All errors are our own.

[1] This regularity holds even after the Republican Party captured the majority in the U.S. Congress (Snyder and Groseclose 1997).

[2] While confidence procedures are typically authorized by the constitution, they also may be used by convention. In the case of Great Britain, for instance, constitutional scholars generally agree that votes of confidence are binding. Defeat of a motion of confidence has been treated as fatal since 1832 (e.g., Brazier 1988). Huber (1996, Table 1) gives a detailed overview of confidence procedures in 18 countries.

FIGURE 1. Legislative Bargaining without the Confidence Procedure



tant proposal rights (Doering 1995). In this respect congressional leadership and ministers in parliamentary democracies are functional equivalents (Bagehot [1867] 1963; Cooper 1988; Gilligan and Krehbiel 1987; Rogowski 1990).

Legislatures are fundamentally majoritarian institutions. Any special prerogatives enjoyed by some members must ultimately be approved by a chamber majority. Thus, in our model there are two types of voting: voting on bills and voting on who is entitled to propose bills. We call a *ruling coalition* the set of legislators who have a positive probability of being recognized to make a proposal and a *policy coalition* the set of legislators who vote for a bill.

Our model demonstrates how the vote of confidence procedure makes members of the ruling coalition cheaper to include in policy coalitions than they would be otherwise. The confidence procedure creates a link between voting on a bill and voting on the ruling coalition. Since membership in the ruling coalition is valuable in future periods (it allows the capture of future rents by exploiting proposal power), members of the ruling coalition stand to lose from the rejection of a proposal. Legislators who are not in the ruling coalition gain by defeating a bill with the confidence procedure attached because they may be part of a new ruling coalition. This in turn means that in any given period a selected proposer has to offer less to ruling coalition members to gain their support for a policy proposal; members of the ruling coalition are "cheaper." Consequently, they are included in policy coalitions, whereas legislators who are not in the ruling coalition are excluded from policy coalitions. The result is that the vote of confidence not only creates cohesive voting but also allows ruling coalitions to capture more of the distributive benefits from the legislative process.

In the next section we introduce our formal model. We then discuss our results in the context of a simple example. In the penultimate part of the article we discuss some implications of our results for the comparative study of legislatures. This is followed by a brief conclusion. Proofs of our results can be found in the Appendix.

THE MODEL

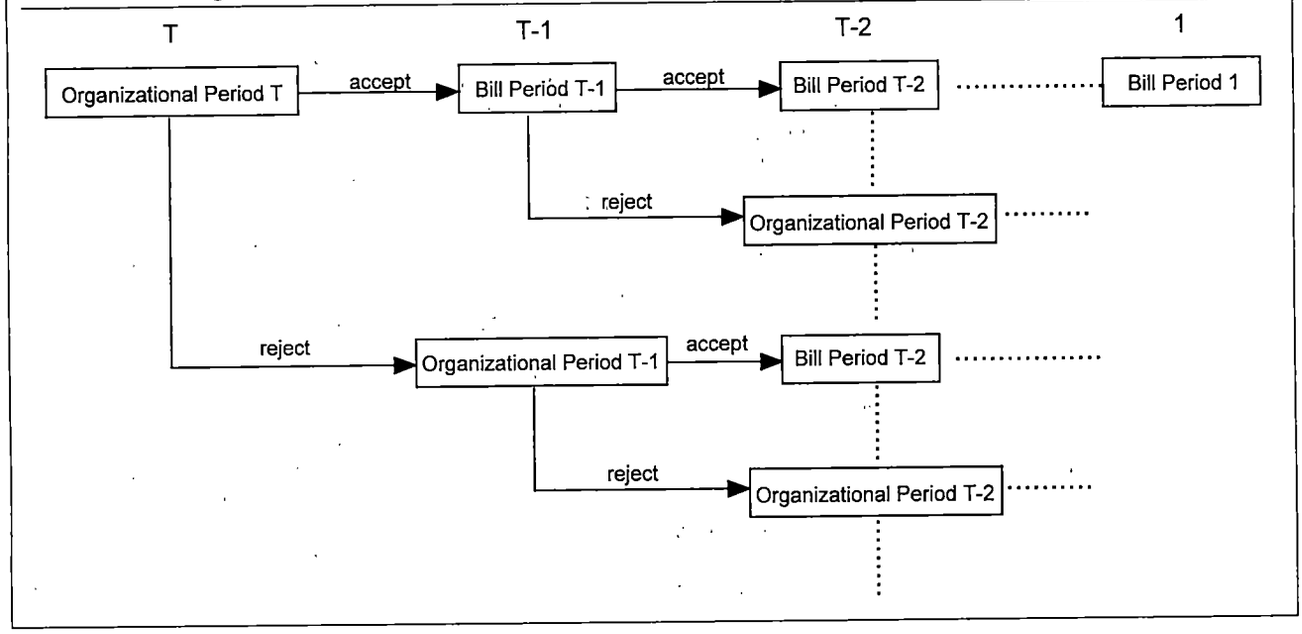
We assume there are n legislators ($n \geq 3$ and n odd) who engage in T periods of legislative bargaining. Let L denote the set of legislators and Λ the set of nonempty subsets of L. In our model there are two types of bargaining: over the distribution of a resource and over who will be able to propose alternative distributions. Bargaining over proposal rights occurs in *organizational periods*, while bargaining over resources occurs in *policy periods*. In organizational periods a legislator is selected at random and proposes a *ruling coalition*. In policy periods a legislator is selected at random from the ruling coalition and proposes a *bill*.

To analyze the effect of the confidence procedure we study two games. The first game, labeled $\Gamma\sim_{con}$, is used to establish a baseline for legislative bargaining without the confidence procedure. The second game corresponds to a legislature with the confidence procedure and is labeled Γ_{con}. In both games the first period is an organizational period. Subsequent periods are also organizational periods unless and until a majority votes to accept a proposed ruling coalition. Once a ruling coalition is approved, the next period is a policy period. If a majority of the legislature votes to accept the proposal in a policy period, then the next period is also a policy period in which the proposer is selected from the same ruling coalition.

The difference between the games occurs when a bill is rejected. In Γ_{con}, if a bill is rejected, then the next period is an organizational period, whereas in $\Gamma\sim_{con}$ the next period is a policy period. Thus, the confidence procedure links the fate of a bill to the fate of the ruling coalition. The structure of the two games is shown in figures 1 and 2.

The history of play prior to each period t determines a subset of legislators whose members may be recog-

FIGURE 2. Legislative Bargaining with the Confidence Procedure



nized to make a proposal. In organizational periods, the set of potential proposers is simply the entire set of legislators; in policy periods, proposers are drawn from the ruling coalition.

Our first assumption requires that each legislator is equally likely to be chosen to make a proposal in organizational periods and that all members of a ruling coalition are equally likely to be chosen to make proposals in policy periods. This rules out proposals in organizational periods that give higher recognition probabilities to some members of the ruling coalition.

ASSUMPTION 1. *Let* $C \in \Lambda$ *be the set of legislators who may be recognized to make a proposal in period* t. *The probability legislator* j *is the proposer in period* t *is* $1/|C|$ *if* $j \in C$, *0 otherwise.*

In every period legislators receive a payoff $x \in X$, where X is the set of possible distributions of a dollar among the n legislators, that is, $X \equiv \{x \in [0, 1]^n$, such that $\Sigma_{i=1}^{n} x_i = 1\}$. The assumption that a dollar is divided in every period implies that our model of legislative bargaining is distributional. While distributional concerns are clearly important in real-world legislative settings, we make this assumption to rule out policy coalitions based on preference similarity. This guarantees that cohesive voting will be entirely a consequence of institutional features. In organizational periods the period payoff to any legislator, $i \in L$, is simply $1/n$, whereas in policy periods the period payoff is either $1/n$ if the proposal $b \in X$ is rejected or b_i if it is accepted.

To model cohesive voting it is necessary to have a series of votes on different issues in which legislators' preferences change according to the issue. For example, in the U.S. House on some issues liberal Democrats may have common interests with conservative Republicans (e.g., NAFTA); on other issues, such as spending on education, preferences may largely follow party lines. The effect of issue-specific policy preferences is that, from the perspective of a proposer, the set of legislators whose votes are easiest to obtain changes from issue to issue. We capture issue-specific preferences in a very stylized way. In a distributive politics setting, proposers have an incentive to offer positive benefits only to those legislators who are cheapest. We model changes in issue-specific preferences by assuming that in every period the proposer is endowed with a strict preference order over the other legislators.[3]

Formally, we assume that in every period nature chooses a strict preference order for legislator j, denoted by P_j, that is defined over the remaining set of legislators $L\backslash\{j\}$. For every $j \in L$, let $\Re_j$ be the set of strict preference orders over the legislators in $L\backslash\{j\}$. What remains to be specified is how issue-specific preferences are incorporated in the divide-the-dollar setting. For technical convenience we assume lexicographic preferences.[4] The preference order combined with the following assumption ensures that proposers are never indifferent among the set of proposals available to them.

[3] Alternatively, we could model issue-specific preferences by assuming that nature draws a new reservation value for each legislator in every period. Each draw of reservation values creates an order over the set of legislators for the proposer from those most inclined to vote for a change in the status quo policy to those least inclined. It is thus sufficient to stipulate directly an ordering over legislators, P_j, for the proposer in each period. An advantage of this approach is that the interpretation of the model does not depend on any specific assumption about legislators' issue-specific preferences. Legislators may care about policy outcomes, distributive benefits, or the probability of winning, as long as any such preferences lead to a strict ordering over the set of legislators.

[4] The assumption of lexicographic preferences for proposers is made for analytical convenience and does not substantively change our results. See Diermeier and Feddersen (1996) for a similar model with nonlexicographic preferences.

ASSUMPTION 2. *Each legislator* $j \in L$ *with preference order* P_j *lexicographically prefers to make a proposal that is accepted by legislator* k *to one that is accepted by legislator* k′ *if and only if* kP_jk'.

Our theory seeks to provide an explanation for cohesion based on the institutional characteristics of parliamentary democracies and not on persistent preference-based alignments of interests. We therefore assume that every preference order is ex ante equally probable. Issue-specific coalitions are thus likely to change from period to period.

ASSUMPTION 3. *In each period* t *and for every* $j \in L$ *the probability nature chooses* $P_j \in \Re_j$ *is* $1/|\Re_j|$.

We now define voting and proposing strategies in each type of period. In an organizational period t, nature chooses a proposer $j \in L$ and preference order P_j. Proposer j learns her preferences over the other legislators along with the history of play and proposes a ruling coalition $C \in \Lambda$. Let H^t be the set of histories up to period t. A proposer's strategy is defined as $C : \Re_j \times H^t \rightarrow \Lambda$. All the legislators observe proposer j's preferences along with the proposed ruling coalition and the history of play and then vote to accept or reject the proposal. A pure voting strategy for legislator i in organizational period t is defined as $z_i : \Re_j \times \Lambda \times H^t \rightarrow \{A, R\}$, where A indicates a vote to accept the proposed coalition, and R indicates a vote to reject. A proposed ruling coalition is accepted if and only if a majority of the legislators vote to accept.

In a policy period t, nature selects a proposer j from the ruling coalition and, as in an organizational period, a preference order P_j. The proposer j learns her preferences, P_j, the history of play, h^t, and proposes a bill. A strategy for j is defined as a bill, $b : \Re_j \times H^t \rightarrow X$. The legislators observe proposer j's preferences along with the proposed bill as well as the history of play prior to period t and then vote to accept or reject the proposal. A voting strategy for legislator i in policy period t is defined as $w_i : \Re_j \times X \times H^t \rightarrow \{A, R\}$, where A indicates a vote to accept the proposed bill, and R indicates a vote to reject. The bill is accepted if a majority votes to accept and is rejected otherwise.

Our final assumption is standard in legislative voting games (Baron and Ferejohn 1989).

ASSUMPTION 4. *All legislators have a lexicographical preference for voting to accept a proposal.*

Given that players have strict preferences over voting and proposal strategies, it is without loss of generality to consider pure strategies and use the iterated elimination of strictly dominated strategies to solve both games.

RESULTS

The logic of our results is easy to see within a fairly simple two-period three-legislator example. We will use this example to illustrate each of the results and then give the general result for the n-legislator and T-period model. The proofs for the general results may be found in the Appendix.

Consider a three-person legislature, $L = \{1, 2, 3\}$. If the last period is an organizational period, then legislators cannot make redistributive proposals, and each legislator receives a period payoff of ⅓. Suppose that the last period is a policy period, and legislator $i \in L$ with preference order P_i is chosen to propose a division of a dollar among the legislators. If the proposal is accepted by a majority, then each legislator receives his or her proposed share. Otherwise, each receives ⅓.

By assumption 4, every legislator strictly prefers to accept any proposal that provides her with at least ⅓ of the dollar. It follows by the elimination of strictly dominated strategies that any proposal which offers at least ⅓ to at least one other legislator will be accepted, while all other proposals will be rejected. The proposer will thus allocate ⅓ to exactly one other legislator. There are two such proposals. Assumption 2 implies that legislator i's preference order, P_i, determines a unique best proposal in which she offers ⅓ to her preferred coalition partner and keeps ⅔ for herself. The proposer and the legislator who is offered ⅓ will vote to accept the proposal, while the legislator who is offered 0 will vote to reject. Thus, if legislator 1 is the proposer and she prefers legislator 2 to legislator 3, then she has a strictly dominant strategy to offer ⅓ to legislator 2 and 0 to legislator 3.

It is now a simple matter to calculate the expected value to each legislator for the last period. Let C be the ruling coalition in the last period and $c = |C|$ be the size of the ruling coalition, where $c \in \{1, 2, 3\}$. It follows from assumption 3 that the expected last-period payoff for legislators who are not in the ruling coalition is simply ⅙, that is, half the time they are in the proposer's preferred coalition and receive ⅓, otherwise they receive 0. The expected continuation value for the members of the ruling coalition is slightly more complicated. By assumption 1, each member of the ruling coalition has probability $1/c$ of being recognized to make a proposal. Conditional on not being the proposer, the expected last-period payoff is the same for members of the ruling coalition as it is for nonmembers. Thus, the expected continuation value for members of the ruling coalition is given by:

$$\frac{1}{c}\frac{2}{3} + \left[1 - \frac{1}{c}\right]\frac{1}{6} = \frac{1}{6} + \frac{1}{2c}.$$

It follows that members of the ruling coalition receive more in expectation than nonmembers, but all legislators expect to receive some part of the dollar.

Lemma 1 below shows that each legislator's expected last-period payoff is identical in both games and depends only on the size of the ruling coalition and whether the legislator is a member of it. For notational convenience, periods are labeled in backward order, that is, the final period is period 1, while the first period is period T. We denote the set of proposers given history h^t by $C(h^t)$. So, if h^t is an organizational period, we have $C(h^t) = L$, while for policy periods $C(h^t)$

corresponds to the ruling coalition. For any history h^t, the expected payoff prior to period t for any $i \in L$ is denoted $v_i(h^t)$.

LEMMA 1. *In either game* Γ_{con} *or* $\Gamma_{\sim con}$ *and for any history* $h^1 \in H^1$ *and legislator* $i \in L$, $v_i(h^1)$ *equals*

$$v_i(h^1) = \begin{cases} \dfrac{1}{2n} & \text{if } i \notin C(h^1), \\ \dfrac{1}{2c} + \dfrac{1}{2n} & \text{otherwise,} \end{cases}$$

where $c = |C(h^1)|$.

Proof. See the Appendix.

We call the set of legislators who vote in favor of a policy proposal a *policy coalition* and say that voting is *cohesive* in two policy periods if the policy coalitions are the same in each period. Proposition 1 shows that in the game without the confidence procedure in any two policy periods, voting will be cohesive with probability strictly less than one, and this probability goes to zero as the size of the legislature increases. Furthermore, the average policy period payoff for coalition members is strictly greater than for noncoalition members.

To see this in the case of the three-legislator example, consider voting and proposing behavior in the penultimate period. Suppose this is an organizational period in which legislator $i \in L$ with preference order P_i is chosen to propose a ruling coalition. It follows from assumption 4 and the continuation values described above ($1/6 + 1/2c \geq 1/3 > 1/6$) that it is a strictly dominant strategy for a legislator to vote for any coalition in which she is included and to vote against otherwise. Thus, only coalitions that are minimum winning or greater will be approved by a majority. The iterated elimination of strictly dominated strategies implies that proposer i will propose a minimum winning coalition of herself and her preferred coalition partner under P_i, and this coalition will be approved. This will also be the case in the n legislator game with and without the confidence procedure.

Suppose that the penultimate period is a policy period with ruling coalition $C = \{1, 2\}$. Lemma 1 demonstrates that voting and proposing behavior in the final period are independent of behavior in the penultimate policy period. It follows that voting and proposing behavior in that period is the same as in the last period. The expected payoff for the last two periods for each member of the ruling coalition is simply $2 \times 5/12 = 5/6$, whereas for legislator 3 it is $2 \times 1/6 = 1/3$. Cohesion occurs when the policy coalitions are the same in the penultimate and last periods. Three policy coalitions may occur: $\{1, 2\}$, $\{1, 3\}$, and $\{2, 3\}$. In each period, coalition $\{1, 2\}$ occurs with probability .5, while the other two coalitions occur with probability .25. It is easy to see that cohesive voting occurs with probability $3/8$.

PROPOSITION 1. *If* $T \geq 2$, *then the following statements are true in* $\Gamma_{\sim con}$: *(i) in the first period,* $t = T$, *a minimum winning coalition is proposed and accepted; (ii) voting is cohesive in any pair of policy periods* t *and* t' *with probability*

$$\frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!} \sum_{k=1}^{\frac{n+1}{2}} \frac{k}{\frac{n+1}{2}} \frac{\binom{\frac{n-1}{2}}{k-1}\binom{\frac{n-1}{2}}{\frac{n+1}{2}-k}}{\sum_{j=1}^{\frac{n+1}{2}}\binom{\frac{n-1}{2}}{j-1}\binom{\frac{n-1}{2}}{\frac{n+1}{2}-j}} < \frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!};$$

and (iii) the average policy period payoff for any member of the ruling coalition is

$$\left(\frac{1}{n+1} + \frac{1}{2n}\right),$$

and for nonmembers it is

$$\left(\frac{1}{2n}\right).$$

Proposition 2 below shows that the addition of the confidence procedure does not change the incentives for forming minimum-winning ruling coalitions in organizational periods, but it does increase the probability of cohesive voting and results in the ruling coalition capturing almost all the distributive benefits. In Γ_{con} the fate of a policy proposal and the ruling coalition are linked because an organizational period follows any policy period in which a bill is rejected. In the three-person legislature this implies that a rejection of the proposal in the penultimate period gives every legislator her reservation value in *both* periods, that is, $2/3$. Thus, in the penultimate period it is a strictly dominant strategy for any member of the ruling coalition to vote for any proposal in which she receives at least $2/3 - 5/12 = 1/4$. Legislator 3, however, is more expensive, since she will only vote for the bill if the proposal offers her at least $2/3 - 1/6 = 1/2$.

The confidence procedure makes it a strictly dominant strategy for proposers to offer $1/4$ to their coalition partner and keep $3/4$ for themselves. It follows that the policy coalition in the penultimate period will always be the same as the ruling coalition, and cohesion occurs with probability $1/2$. Cohesive voting is thus more likely with the confidence procedure than without. It is easy to see that by increasing the number of periods of legislative bargaining the expected value of future proposal rights is strictly increasing, with the consequence that members of the ruling coalition have an even greater incentive to vote together. As Proposition 2 demonstrates, this implies cohesive voting with probability 1 in any pair of policy periods that does not include the last period.

PROPOSITION 2. *If* $T \geq 2$, *then the following statements are true in the game* Γ_{con}: *(i) in the first period,* $t = T$, *a minimum winning coalition is proposed and accepted; (ii) in any pair of policy periods,* τ *and* τ', *such that* $\tau, \tau' \geq 2$, *voting behavior satisfies cohesion with probability 1; and (iii) the average period payoff for any member of the ruling coalition in policy period* t *is*

$$\frac{(t-1)\dfrac{2}{n+1} + \left(\dfrac{1}{n+1} + \dfrac{1}{2n}\right)}{t},$$

and for nonmembers it is

$$\frac{\dfrac{1}{n} + \dfrac{1}{2n}}{t}.$$

The next corollary illustrates that resources are divided more evenly without the confidence procedure than with it. Indeed, the average period payoff for legislators who are not in the ruling coalition goes to 0 in game Γ_{con} as the number of periods gets large.

COROLLARY 1. *In game* Γ_{con}, $\lim_{t\to\infty} v_i(h^t)/t = 1/c$ *for any* $i \in C$, *and* $\lim_{t\to\infty} v_i(h^t)/t = 0$ *for any* $i \notin C$, *whereas in* $\Gamma\sim_{con}$ $\lim_{t\to\infty} v_i(h^t)/t = 1/n+1 + 1/2n$ *for any* $i \in C$, *and* $\lim_{t\to\infty} v_i(h^t)/t = 1/2n$ *for any* $i \notin C$.

Despite the striking distributional effects of the confidence procedure, it is the case in both models that members of the ruling coalition are strictly better off by maintaining the ruling coalition than by making the next period an organizational period. Moreover, this incentive increases in the number of periods remaining.

Our model also implies higher variance in the level of cohesion in legislatures without the confidence procedure than in parliamentary democracies, but lower average levels of cohesion. Previous research on the U.S. Congress, for instance, indicates considerable variation in cohesion (Brady, Cooper, and Hurley 1979; Collie 1985; Cox and McCubbins 1993; Rohde 1991; Snyder and Groseclose 1997).

One concern that readers may have with the model is that the vote of confidence procedure is rarely formally attached to bills in parliamentary democracies. This raises the question of how the confidence procedure can explain cohesion. Yet, simply the threat of the confidence procedure may be sufficient to generate the behavior described above. Consider the following modification to the game with the confidence procedure. In every policy period a member of the ruling coalition is recognized to propose a bill. Next, the legislature votes on the bill. If a majority votes in favor, then the bill is accepted, and the next period is a policy period. If the bill is rejected, then the same bill is automatically reintroduced with a confidence motion attached. Suppose further that there is small error probability in voting decisions. This implies that bills with a confidence procedure are accepted with positive probability and rejected with positive probability. As in Γ_{con}, when the bill with the confidence motion attached is defeated, the next period is an organizational period. Since the only difference between accepting the bill without the confidence motion and rejecting it occurs in the event that the bill with the confidence procedure is rejected, voting and proposing behavior will be identical to the behavior in Γ_{con}.

IMPLICATIONS

The main purpose of our article is to develop a dynamic model of coalition formation and voting that provides an institutional explanation of cohesive voting. Our results demonstrate that the ability of legislators to attach a vote of confidence procedure to a bill is sufficient to generate cohesion. The analysis suggests a variety of additional implications for the comparative study of legislatures.

Cohesion Dynamics

Corollary 1 demonstrates that the value of belonging to the ruling coalition steadily increases in the number of periods left for ruling coalition members but decreases for other legislators. To see the potential consequences of this, consider a variant of our model in which per-period preferences are subject to potentially large random shocks.[5] When the number of remaining periods is large, it takes larger shocks both to upset cohesive voting and to cause a bill to fail than when there are only a few periods left. This suggests that, given identical, independently distributed (i.i.d.) shocks, we should see cohesive voting decline as the next mandated election approaches, while the conditional probability of cabinet termination would be expected to increase toward the end of a legislative session. This is consistent with Warwick's (1992) finding of increasing hazard rates for parliamentary governments.

Votes of Censure, Constructive Vote of No Confidence, Votes of Investiture

One of the most frequently mentioned differences between parliamentary and presidential democracies is the fact that the ruling coalition is elected by the chamber and can be voted out of office by a supporting majority at any time.[6] In our model this election corresponds to a choice of ruling coalitions, while votes of censure correspond to a transition from a bill period to an organizational period. Notice, however, that *in this sense* presidential and parliamentary systems are very similar. A collapse of the government (in the precise sense of a ruling coalition) in a parliamentary system is functionally equivalent to changing the orga-

[5] Random shocks could be incorporated into a model with random reservation values as in an earlier version of this article (Diermeier and Feddersen 1996). See also Baron 1998.

[6] See Bagehot ([1867] 1963): "The House of Commons is an electoral chamber; it is the assembly which chooses our president" (p. 115), and "the principle of Parliament is obedience to leaders. Change your leaders if you will, but obey No. 1 while you serve No. 1, and obey No. 2 when you have gone over to No. 2" (p. 125).

nizational structure of Congress. Corollary 1 establishes that in both systems supporting coalitions have a strict incentive to maintain the current ruling coalition. The fact that the government is accountable to parliament is not sufficient to guarantee cohesive voting.[7] Rather, what matters is whether accountability is linked to voting on policy proposals.

Party Strength

As mentioned in the introduction, political parties may be able to exert influence over the voting behavior of their members in a variety of ways leading to cohesive voting. A party-based explanation of cohesion leaves open the question of why, for instance, strong parties evolved in Great Britain but not in the United States, given that both countries elect the representatives to their lower chamber by first-past-the-post rules. Cox (1987) has pointed out that the development of disciplined electoral parties was preceded by higher levels of cohesive voting in the House of Commons. This suggests that changes in the legislative system (such as the rise in power of Commons during the nineteenth century) may have encouraged the formation of disciplined parties. To see how this might work, consider the Cox and McCubbins (1993) approach to parties. In their approach, parties provide a collective good to legislators, a brand label, that provides information to risk-averse voters and thus increases reelection probabilities. Yet, legislators also have an incentive to maintain individual reputations. Maintaining the brand label thus creates a collective action problem. Parties are viewed as self-enforcing mechanisms that solve this problem by providing selective incentives to legislators. Corollary 1 implies that the expected payoff difference between being a member of the majority and the minority is always strictly higher in parliamentary systems, thus providing stronger incentives to maintain a collective reputation.

France's Fourth Republic

The formal analysis has shown that confidence vote procedures are sufficient for cohesion, but are they necessary? The discussion of strong political parties above indicates that they are not necessary in the case of one majority party. But some empirical evidence suggests the necessity of the confidence procedure in the case of multiparty legislatures in which no party holds a majority of seats. The French Fourth Republic is a notorious puzzle for the comparative study of legislatures. Although it seemingly fulfilled all constitutional prerequisites for a stable parliamentary democracy, it was plagued (among other things) by low levels of cohesion. Our result suggests that the details of the vote of confidence procedure should be investigated to find out whether they force the government to step down in case of defeat.

While the French Fourth Republic did have a vote of confidence procedure, Huber (1996) recently pointed out that it was deficient in a critical way: Abstentions counted against the bill but for the government. Thus, through the strategic use of abstentions, legislators could vote down the bill but avoid the resignation of the government. Hence, a vote on a bill could not be linked to the survival of the government. This example emphasizes that the vote of confidence procedure is an instrument of the government that forces legislators to support the cabinet. The strategic use of abstention in the French Fourth Republic, however, allowed the legislators to untie these knots and only vote on the bill. In this case, our model predicts low cohesion.

Internal Organization of Legislatures

Another significant difference between parliamentary systems and the U.S. Congress occurs on the dimension of legislative organization. In parliamentary systems, legislative bargaining is mainly conducted by government party leaders and ministers. In contrast, bargaining occurs within committees in the U.S. Congress. Only members of the governing coalition are in the cabinet, whereas congressional committees are typically bipartisan, and the minority exercises significant influence. Suppose, following Gilligan and Krehbiel (1987), that committees are understood to provide a collective good in the form of information to the legislature as a whole. In that case there is a greater incentive under a system without the vote of confidence to include members of the minority on the committees. This follows because the minority as well as the majority can expect to gain from better information. The same cannot be said for parliamentary systems. In such systems the vote of confidence procedure permits any informational gains to be entirely exploited by the majority alone, thus reducing the incentive for the minority to contribute any useful information. If the minority is not going to contribute anything by being involved in the policy formation stage, then there is little reason to give it much voice either within the cabinet or on the floor.

Payoff Distributions and the Design of Constitutions

Corollary 1 points out that the design of constitutions may also have consequences for the distribution of expected payoffs among representatives (and thus their constituencies). Ruling coalitions in parliamentary systems capture almost all the spoils, leaving little for the minority. When the vote of confidence procedure is not available, the minority shares in the spoils, although it receives less than the ruling coalition. This result is important in the context of constitutional design, since it points out trade-offs between different goals in

[7] Some parliamentary democracies, such as Germany and Spain, use constructive votes of no confidence instead of a vote of censure. Under these procedures a government cannot be voted out of office, only replaced by an alternative governing coalition. In our model, this would mean that governing coalitions could be replaced directly without moving to an organizational stage first. Again, corollary 1 suggests that the presence of such an explicit procedure is neither necessary nor sufficient for voting cohesion. For a detailed analysis of different kinds of no-confidence procedures, see Baron 1998.

designing a constitution, for example, between stable and predictable policy coalitions and payoff equity. This is especially important if majorities do not change frequently enough from election to election, as could be expected, for example, if social, political, and economic cleavages coincide (Lijphart 1977). The fact that the majority can so effectively exploit the minority under the vote of confidence procedure suggests an incentive for minorities to seek extralegislative remedies. Minorities might gain leverage by engaging in protest activities that constrain the government. The greater incentive to engage in disruptive activities would lead to greater incentives to set up extralegislative mechanisms to diffuse such crises (Lijphart 1977). Thus, our model may provide some leverage for explaining the greater incidence of extralegislative bargaining mechanisms found more frequently under parliamentary systems.

CONCLUSION

We have developed a model to study the formation of legislative coalitions and voting cohesion in parliamentary and nonparliamentary legislatures. Our framework implies low cohesion in systems without the vote of confidence procedure, such as the United States, and high cohesion in systems with the procedure, such as parliamentary democracies, even if the latter are characterized by many parties. The key feature that leads to cohesion is the fact that votes on bills are treated as votes on who controls floor access in future periods. We identified the vote of confidence procedure as one constitutional feature sufficient for cohesion. The fact that ruling coalitions are accountable to the assembly, however, is neither necessary nor sufficient for cohesion.

If confidence procedures provide distributional benefits to ruling coalitions, then it is natural to ask why a legislative majority would not create an analogous institution in the U.S. Congress. Note, however, that in the absence of constitutional commitment devices, such as the confidence procedure, legislative majorities face a credibility problem. The ruling coalition may threaten to step down if its proposal is defeated, but ex post it prefers not to lose its proposal rights, whether the proposed bill passes or not. Rational legislators would thus discount such a threat and vote on their period-specific preferences, which would lead to voting behavior with low levels of voting cohesion.

These commitment problems may be solvable using standard repeated game arguments. In this case, legislative majorities may be able to develop self-enforcing norms that make resignation threats credible. Yet, as the example of the French Fourth Republic and the high variance in cohesion in the U.S. Congress suggest, whether such norms are sustainable in equilibrium may depend on the strategic incentives in a given decision situation. An analysis of the conditions permitting endogenous commitment may be a useful subject of future research.

APPENDIX

It will be convenient to have some notation to denote a proposer's preferred set of coalition partners. We call $L(C, P_j)$ legislator j's *preferred coalition*, given $C \in \Lambda$. The set of legislators $L(C, P_j)$ is simply legislator j's most preferred set of partners in a minimum winning coalition that includes as many members of C as possible. If $|C| \geq (n-1)/2$, $L(C, P_j)$ is the set of $(n-1)/2$ legislators in C (other than j) who are most preferred by j, given P_j. If $|C| < (n-1)/2$, then $L(C, P_j)$ includes all the members of C (other than j) and the $(n-1)/2 - |C|$ most preferred legislators among the remaining legislators.

Suppose, for example, that legislators are numbered $1, \ldots, n$, with the proposer 1 having a preference order such that 2 is preferred to 3, 3 to 4, and so on. Then, $L(L, P_1) = \{2, 3, \ldots, (n+1)/2\}$. Suppose that $C = \{5, 6, \ldots, (n+3)/2\}$. Then, $|C| = (n-1)/2 - 2$, and $L(C, P_1) = \{2, 3, 5, 6, \ldots, (n+3)/2\}$, where legislator 4 is left out of 1's preferred coalition, even though according to P_1, 1 prefers her to all the members C.

LEMMA 1. *In either game Γ_{con} or $\Gamma\sim_{con}$ and for any history $h^t \in H^t$ and legislator $i \in L$, $v_i(h^t)$ equals*

$$v_i(h^t) = \begin{cases} \dfrac{1}{2n} & \text{if } i \notin C(h^t), \\ \dfrac{1}{2c} + \dfrac{1}{2n} & \text{otherwise,} \end{cases} \tag{1}$$

where $c = |C(h^t)|$.

Proof. If period $t = 1$ is an organizational period, then the period payoff is $1/n$ for every legislator. Equation 1 holds since $C(h^1) = L$, and $c = n$. Suppose period $t = 1$ is a policy period with ruling coalition $C(h^1) \in \Lambda$. It follows from assumption 4 and the fact that each legislator will receive a payoff of $1/n$ if the bill is defeated that it is a strictly dominant strategy for every legislator to vote to accept if and only if the bill offers her at least $1/n$. Thus, a bill will pass if and only if it provides $1/n$ to a majority of the legislators. It follows from assumption 2 and the iterated elimination of dominated strategies that each proposer $j \in C(h^1)$ will offer $1/n$ to the members of her most preferred minimum winning coalition and keep the remainder for herself. Formally, a proposer $j \in C(h^1)$ with preference order P_j will propose

$$b_i(P_j, h^1) = \begin{cases} \dfrac{n+1}{2n} & \text{if} \quad i = j, \\ \dfrac{1}{n} & \text{if} \quad i \in L(L, P_j), \\ 0 & \text{otherwise,} \end{cases}$$

for any history $h^1 \in H^1$. Note that every nonproposing legislator will be in the preferred coalition with probability 1/2. Equation 1 follows immediately as a consequence of assumptions 1 and 3. *Q.E.D.*

PROPOSITION 1. *If $T \geq 2$, then the following statements are true in $\Gamma\sim_{con}$: (i) in the first period, $t = T$, a minimum winning coalition is proposed and accepted; (ii) voting is cohesive in any pair of policy periods t and t' with probability*

$$\frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!}\sum_{k=1}^{\frac{n+1}{2}}\frac{k}{\frac{n+1}{2}}\frac{\binom{\frac{n-1}{2}}{k-1}\binom{\frac{n-1}{2}}{\frac{n+1}{2}-k}}{\sum_{j=1}^{\frac{n+1}{2}}\binom{\frac{n-1}{2}}{j-1}\binom{\frac{n-1}{2}}{\frac{n+1}{2}-j}}$$

$$<\frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!};$$

and (iii) the average policy period payoff for any member of the ruling coalition is

$$\left(\frac{1}{n+1}+\frac{1}{2n}\right),$$

and for nonmembers it is

$$\left(\frac{1}{2n}\right).$$

Proof. The proof is by induction. From lemma 1 we know that for period $t = 1$ the following statement holds. For any history h^t in which the set of proposers in period t is $C(h^t)$, the ex ante expected payoff for any $i \in L$ is

$$v_i(h^t) = \begin{cases} t\left(\frac{1}{2c}+\frac{1}{2n}\right) & \text{if } i \in C(h^t), \\ t\frac{1}{2n} & \text{if } i \notin C(h^t), \end{cases} \quad (2)$$

where $c = |C(h^t)|$.

Suppose that the induction hypothesis holds for some period $t \geq 1$. Let period $t + 1$ be an organizational period. The expected payoff to every legislator $i \in L$ if a ruling coalition C is proposed and accepted in period $t + 1$ is $1/n + v_i(h^t)$, where $C(h^t) = C$. It follows that legislator i will vote for any coalition C if and only if $i \in C$. Since $v_i^t(h^t)$ is decreasing in c for members of the ruling coalition, it follows from the iterated elimination of dominated strategies and assumption 2 that the proposer will propose her preferred minimum winning coalition, $C(P_j, h^{t+1}) = L(L, P_j) \cup \{j\}$, and this minimum winning coalition will be accepted. Assumptions 1 and 3 imply that the ex ante expected payoff for every $i \in L$ is independent of the history h^{t+1} and is given by

$$v_i(h^{t+1}) = \frac{t+1}{n}. \quad (3)$$

Suppose period $t + 1$ is a policy period, and the history of play is h^{t+1}. If $C(h^{t+1})$ is the ruling coalition in policy period $t + 1$, then $C(h^t) = C(h^{t+1})$ for any history h^t that is consistent with h^{t+1}. That is, the ruling coalition in period $t + 1$ is also the ruling coalition in policy period t. Suppose a bill b is proposed in period $t + 1$, and let h_a^t be a history consistent with h^{t+1} in which b is accepted and h_n^t in which b is not accepted. The expected payoff to any legislator $i \in L$ if a bill b is accepted is $b_i + v_i(h_a^t)$ and $1/n + v_i(h_n^t)$ if b is rejected. But $v_i(h_a^t) = v_i(h_n^t)$, and $C(h_a^t) = C(h_n^t) = C(h^{t+1})$. It follows that in period $t + 1$ a legislator $i \in L$ will vote for b if and only if $b_i \geq 1/n$. Thus, by the same logic as in lemma 1 it follows that each proposer will propose the bill b such that

$$b_i(P_j, h^{t+1}) = \begin{cases} \frac{n+1}{2n} & \text{if } i = j, \\ \frac{1}{n} & \text{if } i \in L(L, P_j), \\ 0 & \text{otherwise}, \end{cases}$$

and the expected period payoff is

$$v_i^{t+1}(h^{t+1}) = \begin{cases} (t+1)\left(\frac{1}{2c}+\frac{1}{2n}\right) & \text{if } i \in C(h^{t+1}) \\ (t+1)\left(\frac{1}{2n}\right) & \text{if } i \notin C(h^{t+1}). \end{cases} \quad (4)$$

This completes the induction step. In period T a minimum winning coalition C is proposed, and only members of the ruling coalition vote to accept. The same C governs in every policy period, and proposals are always passed by a minimum winning coalition determined only by the identity of the proposer and the proposer's preference order.

To see that claim (ii) in the proposition is correct, let τ and τ' be policy periods. Let S be a policy coalition in period τ, that is, the set of legislators who vote to accept the bill proposed in period τ. By the argument above we know that $|S| = n+1/2$. We also know that the ruling coalition in policy periods τ and τ' is the same in both periods. Denote the ruling coalition by C. We can partition the set of possible policy coalitions in period τ into subsets S_k, where $S_k \equiv \{S \subset L : |S| = (n+1)/2 \text{ and } |S \cap C| = k\}$ and $k \in \{1, 2, \ldots, (n+1)/2\}$. In words, S_k is the set of minimum winning coalitions that contain exactly k members of the ruling coalition. Let $\Pr(S \in S_k)$ be the probability that the policy coalition in period τ contains exactly k members of the ruling coalition C. Then,

$$\Pr(S \in S_k) = \frac{g(n,k)}{\sum_{j=1}^{\frac{n+1}{2}} g(n,j)},$$

where

$$g(n,k) = \frac{n+1}{2}\binom{\frac{n-1}{2}}{k-1}\binom{\frac{n-1}{2}}{\frac{n+1}{2}-k}\frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!}.$$

For any $S \in S_k$ in policy period τ, the probability that the policy coalition in period τ' (S') is the same as S is given by

$$\Pr(S' = S | S \in S_k) = \frac{k}{\frac{n+1}{2}}\frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!}.$$

In words, this is the probability that the proposer in period τ' is one of the k members of the ruling coalition in policy coalition S and that the proposer prefers to include the other members of S in her policy coalition. The probability that voting is cohesive in any pair of policy periods is thus

$$f(n) = \sum_{k=1}^{\frac{n+1}{2}} \Pr(S \in S_k)\Pr(S' = S | S \in S_k)$$

$$= \frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!}\sum_{i=1}^{\frac{n+1}{2}}\frac{i}{\frac{n+1}{2}}\Pr(S \in S_k). \quad (5)$$

Since

$$\sum_{i=1}^{\frac{n+1}{2}} \frac{i}{\frac{n+1}{2}} \Pr(S \in S_k) < 1,$$

it follows that

$$f(n) < \frac{\left(\frac{n-1}{2}\right)!\left(\frac{n-1}{2}\right)!}{(n-1)!}.$$

Statement (iii) follows from equation 2, and $|C(h^t)| = (n+1)/2$ in any policy period t. *Q.E.D.*

PROPOSITION 2. *If* $T \geq 2$, *the following statements are true in the game* Γ_{con}: *(i) in the first period,* $t = T$, *a minimum winning coalition is proposed and accepted; (ii) in any pair of policy periods* τ *and* τ', *such that* $\tau, \tau' \geq 2$, *voting behavior satisfies cohesion with probability* 1; *and (iii) the average period payoff for any member of the ruling coalition in policy period* t *is*

$$\frac{(t-1)\frac{2}{n+1} + \left(\frac{1}{n+1} + \frac{1}{2n}\right)}{t},$$

and for nonmembers it is

$$\frac{\frac{1}{n} + \frac{1}{2n}}{t}.$$

Proof. The proof is by induction. The following statements are true for period $t = 1$ by lemma 1, so suppose that they hold for some period $t \geq 1$. For any history h^t with proposing coalition $C(h^t)$ (where $c = |C(h^t)|$), it is the case that

$$v_i(h^t) = \begin{cases} \frac{t-1}{c} + \left(\frac{1}{2c} + \frac{1}{2n}\right) & \text{if } i \in C(h^t) \text{ and } c \geq \frac{n+1}{2}, \\ \frac{1}{2n} & \text{if } i \notin C(h^t) \text{ and } c \geq \frac{n+1}{2}, \\ v_c^t & \text{if } i \in C(h^t) \text{ and } c < \frac{n+1}{2}, \\ v_n^t & \text{if } i \notin C(h^t) \text{ and } c < \frac{n+1}{2}, \end{cases} \tag{6}$$

where either

$$v_c^t > t/n > v_n^t,$$

or

$$v_c^t = v_n^t = t/n.$$

If period $t + 1$ is an organizational period, then an argument analogous to that in proposition 1 shows that a minimum winning coalition will be proposed and accepted, and only legislators included in the proposed ruling coalition will vote to accept.

Suppose $t + 1$ is a policy period reached by history h^{t+1} with ruling coalition $C(h^{t+1})$, and let $c = |C(h^{t+1})|$. Suppose that a bill b is proposed in this period, and let h_a^t be a t-period history that is consistent with h^{t+1}, where b is accepted, and h_n^t if b is rejected. In Γ_{con} the expected payoff to every legislator i if b is accepted is $b_i + v_i(h_a^t)$, and $1/n + v_i(h_n^t)$ if b is rejected, where $C(h_a^t) = C(h^{t+1})$, and $C(h_n^t) = L$. From equation 6 we know that $v_i(h_n^t) = t/n$; thus, for any history h^{t+1} it follows that legislator i will vote for b if and only if $b_i \geq (t+1)/n - v_i(h_a^t)$. If $c \geq (n+1)/2$, then by equation 6 $v_i(h_a^t) \geq t/n$ for any $i \in C(h^{t+1})$, and $v_i(h_a^t) < t/n$ for any $i \notin C(h^{t+1})$. It follows that a proposer will never propose a bill that gives a positive payoff to any $i \notin C$, and

$$v_i(h^{t+1}) = \begin{cases} \frac{t}{c} + \left(\frac{1}{2c} + \frac{1}{2n}\right) & \text{if } i \in C(h^{t+1}), \\ \frac{1}{2n} & \text{if } i \notin C(h^{t+1}) \cdot \end{cases} \tag{7}$$

So, for a ruling coalition $C(h^t)$, such that $|C(h^t)| = (n+1)/2$, a legislator votes for the proposal if and only if $i \in C(h^t)$.

If $c < (n+1)/2$, then there are two cases. On the one hand, if $v_c^t = t/n = v_n^t$, then the continuation values cancel, and voting and proposing behavior in period $t + 1$ is identical to the behavior in period $t = 1$. It follows that

$$v_i(h^{t+1}) = \begin{cases} \left(\frac{1}{2c} + \frac{1}{2n}\right) + t/n & \text{if } i \in C(h^{t+1}), \\ \frac{1}{2n} + t/n & \text{if } i \notin C(h^{t+1}). \end{cases}$$

Thus, $v_i(h^{t+1}) > (t + 1)/n$ for any $i \in C(h^{t+1})$, and $v_i(h^{t+1}) < (t + 1)/n$ for any $i \notin C(h^{t+1})$.

On the other hand, suppose $v_c^t > t/n > v_n^t$. Since there are not enough coalition members to pass a bill, any proposal must receive the votes of at least $(n+1)/2 - c$ legislators who are not in the ruling coalition in order to pass. If no bill passes in period $t + 1$, then

$$v_i(h^{t+1}) = \frac{t+1}{n}$$

for every $i \in L$. Suppose that a bill passes in period $t + 1$. Legislator $i \in L$ will vote for a proposal b if and only if $b_i \geq (t+1)/n - v_i(h_a^t)$. It follows from $v_c^t > v_n^t$ that ruling coalition members are cheaper from the proposer's perspective than are nonmembers. The proposer will offer $(t+1)/n - v_n^t$ to the minimum number $(n+1)/2 - c$ of legislators not in the ruling coalition necessary to ensure passage of the bill. It follows from assumption 3 that

$$v_i(h^{t+1}) = v_n^{t+1} = \left(\frac{\frac{n+1}{2} - c}{n - c}\right)\left(\frac{t+1}{n} - v_n^t\right) + v_n^t$$

for any $i \notin C(h^{t+1})$, and

$$v_i(h^{t+1}) = v_c^{t+1} = \frac{1}{c}(t + 1 - (n - c)v_n^{t+1})$$

for any $i \in C(h^{t+1})$. Since $t/n > v_n^t$ and $1 > (n+1)/2 - c/n - c > 0$, it follows that $(t+1)/n > v_n^{t+1} > v_n^t$. Similarly,

$$v_c^{t+1} > \frac{1}{c}\left(t + 1 - (n - c)\frac{t+1}{n}\right) = \frac{t+1}{n}.$$

Thus, $v_c^{t+1} > (t+1)/n > v_n^{t+1}$.

This completes the induction step. It follows that every period after the first is a policy period, and thus voting is cohesive in every period but the last. Statement (iii) is implied by equation 7. *Q.E.D.*

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International Crises and Domestic Politics

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Audience costs enable leaders to make credible commitments and to communicate their intentions to their adversaries during a crisis. I explain audience costs by simultaneously modeling crisis behavior and the domestic reelection process. I assume that a leader's ability influences the outcome of a crisis. As such, voters use outcomes as a signal of their leaders' quality. Leaders have incentives to make statements that deter their enemies abroad, since these statements also enhance their standing at home. Yet, such "cheap talk" foreign policy declarations are only credible when leaders suffer domestically if they fail to fulfill their commitments. In equilibrium, false promises are only made by the least competent types of leaders. Leaders that break their promises suffer electorally. Because initial domestic conditions and institutional arrangements affect the vulnerability of leaders to these domestic costs, such factors influence the credibility of policy declarations and, therefore, the crisis outcome.

In 1990, prior to the Gulf War, President Bush made explicit foreign policy statements regarding Iraq's invasion of Kuwait. In short, he threatened Saddam Hussein. When this failed to cow Hussein into withdrawing, Bush followed through on his threats and authorized force to remove the Iraqi army from Kuwait. By comparison, U.S. policy toward Bosnia was less direct. Both presidents Bush and Clinton adopted vague, ambiguous policies toward the Bosnian crisis, and U.S. intervention was limited. Potentially, Clinton could have used direct military force to lift the siege of Sarajevo, but he neither pursued this strategy nor threatened to do so. Sometimes threats carry the weight of direct action. For example, Clinton outlined the contingent circumstances under which he would authorize military intervention in Haiti; just before the announced deadline, the Haitian military junta relinquished power, alleviating the need for military force.

When leaders make threats, these statements are often taken as meaningful. Yet, leaders are not bound by these statements in any formal, legal, or moral sense: Threats are "cheap talk." When statements are nonbinding, why should other nations believe them? A common explanation is that leaders, particularly in democratic systems, pay "audience costs" if they fail to follow through on their commitments (Fearon 1994a, 1997; Martin 1993). Hence, having made belligerent statements and fearing the domestic consequences of reneging, leaders carry out their threats. Audience costs offer a mechanism via which leaders can credibly commit.

The concept of audience costs is problematic, however. It requires that citizens remove leaders who renege on their promises. Yet, if citizens are happy with their leader overall, then why should they throw her out of office simply because she fails to carry out one particular threat? Indeed, they may be delighted that she avoided foreign entanglement once she realized that she was unable to influence the situation. In the literature, audience costs are simply assumed to exist. I provide microfoundations for why citizens want to remove leaders who fail to honor their commitments. In addition to explaining why audience costs work, I predict how domestic conditions and political institutions affect the extent to which these costs bind leaders and, hence, the extent to which threats influence crisis behavior.

Information plays a key role in answering the audience cost puzzle. Leaders vary in quality, and this is reflected in their ability to perform during crises. Talented leaders, with the appropriate skills, are more likely to be successful during a crisis than their less competent peers. Since ability affects outcomes, voters want to remove incompetent leaders, but they can only perceive leaders' ability through actions and performance. Thus, when choosing foreign policy, leaders must consider not only how it influences foreign rivals but also how it will be received at home. All else equal, leaders who expect to perform poorly in conflict avoid foreign entanglement. This is true for two reasons. The first is obvious: Poor performance is inherently undesirable. The second is that revealing low competence harms a leader's electoral prospects. This is particularly important in lopsided conflicts, when the winner may not be in doubt, but efficiency reveals a leader's ability.[1]

In this setting, reneging on a commitment is a sign of incompetence, which the voters punish electorally. To see why, suppose that audience costs do exist and that voters do indeed punish leaders who fail to follow through on threats. For competent leaders, carrying out threats imposes few costs. They expect to do well in the crisis, and it provides them with an opportunity to reveal their abilities to the electorate. For less competent leaders, having their bluff called leaves them between a rock and a hard place. If they renege on their commitment, then they are punished domestically, which means that many of them engage in conflicts they would prefer to avoid. For the least able leaders, however, conflict is impractical. Since their poor performance will be electorally punished anyway,

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An earlier version of this paper was presented at the Peace Science Society meeting, Columbus, Ohio, October 13–6, 1995. I thank the participants at that meeting; John Ginkel, Andrew Martin, Fiona McGillivray, and James Morrow; and several anonymous referees for their useful comments.

[1] The arguments do not require that the marginal effect of ability on outcomes be large.

they have no incentive to intervene, and they renege on their promises. Of course, this provides the justification for the electorate to punish them for failing to honor their commitments. Audience costs are endogenous. Since it is the least competent leaders who renege, broken commitments are a sign of incompetence which the voters punish.

This article ties together crisis behavior and domestic politics, simultaneously addressing how the latter shapes foreign policy and how crisis outcomes affect domestic political survival. In doing so, it produces a theoretical explanation of audience costs. Before presenting the full model, I consider different aspects of the puzzle. I start by examining a stylized model of extended deterrence and use it to explain the incentives that leaders face during international disputes. The conflictual nature of crises hampers communication between nations and leads to credibility problems. I explain why, in order to make threats or credible commitments, leaders must be able to send costly signals. Hence, intrinsically costless policy statements are informative only because of their domestic political implications. In this context, foreign policy choices have both international consequences—influencing the outcome of crises—and domestic consequences that affect voting decisions. After examining the links between domestic and international politics, I formally model foreign policy decision making within international crises against the backdrop of domestic politics. In doing so, I provide an explanation for the international/domestic nexus.

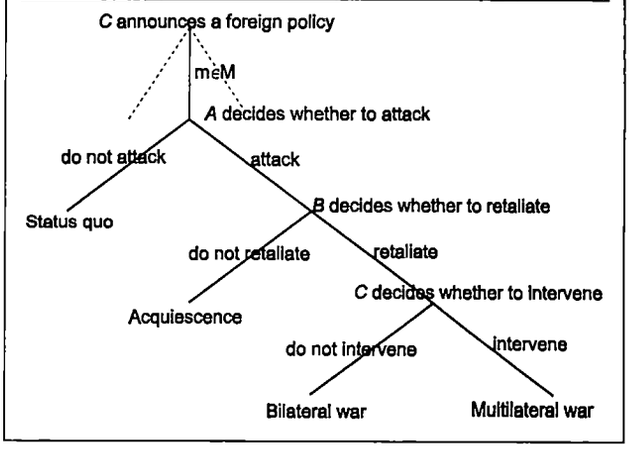
INTERNATIONAL CRISES

I consider the case of extended deterrence among three nations (Huth 1988). I focus on this potential war scenario, but the incentives this type of event creates could apply equally well to less intense forms of conflict.[2] In the stylized model, nation A is in a dispute with nations B and C over some issue. Suppose that nations B and C are initially content with the status quo, but nation A desires change. Nation A can attack nation B. If nation B acquiesces, then nation A gets its desired policy.[3] If nation B resists, then the outcome depends upon which nation prevails in the conflict. I model the outcome of war as a lottery. Once a war starts, nation C can choose to intervene.

I illustrate the extensive form of the international crisis game in Figure 1. If A does not attack, then the status quo prevails. If A attacks and B surrenders, the outcome is acquiescence. Once A attacks and B resists, war occurs. The war becomes multilateral if C intervenes but remains bilateral if C stays neutral.

Each nation wants its preferred issue position to prevail. Yet, nations also want to avoid fighting. Conditional on having attacked, A's preferences, from best to worst, are acquiescence, bilateral war, and multilateral war. When B surrenders, A is certain of attaining its goal. The more resistance A encounters, however, the smaller the chance that it wins. As B becomes likely to resist and C becomes likely to intervene, attacking becomes less attractive. While it is possible that A will attack knowing that this will lead to a multilateral war, the probability that A attacks is much higher if A believes B will surrender.

FIGURE 1. The International Crisis Game



Both B and C want the status quo to prevail. Yet, if A attacks, then B must decide whether to fight or acquiesce. B is more likely to win any war if it receives allied intervention. Hence, B prefers a multilateral to a bilateral war, so its resistance depends in part on its expectations about C. Although C's most preferred outcome is the status quo, it is A that chooses whether to disrupt it. C only gets to choose between intervening and remaining neutral. The decision to intervene depends upon whether the marginal effect of intervention on the probability of victory overcomes the costs of fighting. Intervention allows C to influence directly the crisis outcome, but C also can indirectly influence the course of events. Expectations about whether C will intervene affect whether A attacks and whether B resists. Therefore, if C can simply alter the expectations of A and B, then it can drastically alter the course of events.

CREDIBLE COMMUNICATIONS IN DETERRENT SITUATIONS

The international crisis game illustrates deterrence. Each nation's decision depends upon what it believes the other nations will do. For example, the more likely C is to intervene, the more likely B is to resist. The more likely B is to resist, the less likely A is to attack.

C would like to deter A from attacking B and can do so by convincing A that it is prepared to intervene. As in the standard deterrence problem, however, convincing A is difficult. Suppose that C's leader releases a foreign policy statement saying that she will intervene if A attacks. If A believes this message and is deterred,

[2] In the context of the model, traditional deterrence between two nations is a special case of the extended deterrence model, in which the protegé nation always resists.

[3] In the three examples, acquiescence can be interpreted as the Kuwaiti government accepting the legitimacy of Saddam Hussein's annexation, the citizens of Sarajevo abandoning the city, and opposition parties accepting the junta as the rightful government of Haiti despite electoral results to the contrary.

then C benefits because the status quo is maintained. Yet, C benefits whether or not the policy statement is true. Providing A believes C's claims, C rarely has to intervene, because A is unlikely to attack in the first place. Therefore, independent of what C really intends to do, if A is deterred by threats, then C should threaten to intervene. Unfortunately, this creates a credibility problem: C's messages are unrelated to her intentions. A should ignore threats because they are vacuous, containing no information about whether C will really intervene.

In general, if A's response to any foreign policy statement favors C, then C always sends the intervention message, whether or not it is true. But the message should have no effect on A, since it does not reflect anything about C's likely decision. This creates something of a paradox. Theoretically, neither nations nor domestic electorates have any reason to believe or respond to any foreign policy statements. Yet, empirically, threats and ultimatums sometimes work and announcements of policy often affect public opinion. To rectify this discrepancy, I explicitly model the role of domestic politics within international crises.

In the context of game theory, foreign policy statements that have no intrinsic cost are commonly termed "cheap talk." Jervis (1970) calls such costless messages signals. I show, as Jervis claims, that these signals can be informative, because they affect the beliefs, and hence the behavior, of domestic actors. Fearon (1994a, 1994b, 1997) correctly claims that in the international context alone, messages must be costly to be informative.[4] Yet, foreign policy is not insulated from domestic concerns. Leaders simultaneously act at both the international and the domestic level. Providing that messages are costly in one setting, they can be informative in the other. Fearon assumes that there are exogenous domestic audience costs associated with foreign policy statements, and it is these which enable leaders to make credible threats. Specifically, Fearon assumes that leaders pay a fixed cost in terms of domestic support if they fail to follow through on their policy pledges. Why should voters want to punish leaders who bluff? Fearon assumes the existence of such costs but does not explain how they arise. In the international context, a leader who bluffs may well be acting in her nation's best interest. Suppose a leader, having failed to influence a crisis with threats, resists becoming embroiled in a war. Had her bluff succeeded, the nation as a whole would have benefited. Why should voters punish their leader when she believed that the war was not in her nation's interest? Surely the voters prefer a leader that remains neutral when the war is not worthwhile. Under these circumstances, it seems inappropriate for them to punish leaders who fail to carry out a single threat.

By simultaneously modeling crisis behavior and domestic elections, I offer a theoretical explanation for audience costs. The key is not that voters are unhappy with their leader's handling of the current crisis; rather, the failure to carry out threats reveals information about a leader's ability. In other words, voters are content that their leader kept them out of the conflict, but in the future they want another leader. In addition to providing a microfoundation for audience costs, the model generates testable hypotheses relating domestic politics and foreign policy choice. Before presenting the model, I summarize some of the linkages between internal and external politics.

DOMESTIC POLITICS AND CRISIS BEHAVIOR

Political leaders have two audiences: domestic and international (Putnam 1988). Many scholars have observed that foreign policy and domestic politics are related (Russett and Graham 1989 summarize much of this literature). The more interesting issue is the direction of causality. Does the outcome of international events affect domestic survival, or does domestic politics drive the formation of foreign policy?

Studies of both voting decisions and public opinion conclude that foreign policy performance affects domestic political support (Aldrich, Sullivan, and Borgida 1989; Hurwitz and Peffley 1987; Mueller 1973; Ninic and Hinckley 1991). Although some of this research concludes that the domestic economy rather than foreign affairs is more important in explaining support, none finds international events unimportant. Furthermore, their influence is not limited to democratic systems. Bueno de Mesquita and Siverson (1995; also see Bueno de Mesquita, Siverson, and Woller 1992) find that, for all regime types, international outcomes affect domestic survival. For example, leaders who lose wars are more likely to be removed than successful leaders.

Other scholars suggest that the causal relationship runs in the opposite direction. For example, both Ostrom and Job (1986) and James and Oneal (1991) find that domestic political factors are more important in explaining foreign policy choice than are international factors. Regardless of an independent effect, the role of international events in domestic survival suggests that domestic politics should influence policy choice. Leaders who want to stay in office should select policies that help them achieve that goal. The choice depends upon domestic support and political institutions.

The diversionary war hypothesis suggests that leaders with domestic problems who anticipate being removed often undertake adventurous foreign policies that they would not otherwise attempt (Downs and Roche 1993; Hess and Orphanides 1995; Levy 1989; Richards et al. 1993; Smith 1996a). Since the model I present suggests similar results, it is worthwhile to examine the logic of this argument. Leaders often pursue risky policies abroad in order to divert attention from their domestic failures. Such policies may succeed, but often they do not. Since failure at the international level often results in removal, why would leaders take the chance? They may anticipate being removed anyway, so a policy failure would leave them

[4] Sartori (1996) suggests that reputational issues generate these costs.

TABLE 1. International Payoffs

	Outcome			
Nation	StatusQuo	Acquiescence	Bilateral War	Multilateral War
A	0	1	$1 - q(\Theta) - k_a$	$1 - p(\Theta) - k_a$
B	1	0	$q(\Theta) - k_b$	$p(\Theta) - k_b$
C	1	0	$q(\Theta)$	$p(\Theta) - k_c$

no worse off, whereas success could restore their fortunes. Leaders who enjoy popular support do not enact such policies, not wanting to jeopardize their survival.

The diversionary hypothesis implies that domestic political conditions drive the formation of foreign policy, but the explanation for this linkage requires a more complex understanding of the relationship. It is the domestic consequences of international outcomes that drive leaders to select particular policies. This suggests that causality does not run in a single direction. In my model, in addition to the common goals of the nation, leaders are motivated to select foreign policies because of their domestic consequences. The interaction of policy choice and leader evaluation is embedded within the context of an international dispute.

While it is important never to forget policy differences, within the international context voters typically care about their leader's ability. The outcome of international crises often resemble public goods. When a nation gains control of a resource or a piece of territory, this typically benefits the country as a whole. Therefore, the problem for the voters is not to find a leader with the same preference as themselves: All want their nation to succeed. Rather, at issue is finding a competent leader who can obtain the outcomes desired by voters. At least on international issues, I assume that voters choose between leaders on the basis of ability rather than policy differences.

In common with much of the literature, I assume that leaders differ in ability (Hess and Orphanides 1995; Richards et al. 1993; Smith 1996a). Some are competent, able to choose the right combination of forces, appoint skilled generals, adopt appropriate strategic plans, and persuade their allies to support them. Others are less adept. On average, competent leaders are more likely to be successful during an international crisis than are their less competent peers. More specifically, I assume that once a nation joins a war, the more competent its leader, the greater the probability that it will win. This is not to say that a leader's ability is the only determining factor; obviously, as in the numerical examples I present, such structural differences as military strength may have a dominating influence. The assumption I make is that, at the margin, the quality of leadership affects who wins.

THE MODEL

There are four players in the model: the political leaders in nations A, B, and C and the domestic electorate in C. When I refer to A, B, and C, I mean the leaders as well as the states. The game has two stages, the international crisis phase and the domestic electoral phase. Figure 1 showed the international crisis stage. C sends a costless message indicating her foreign policy. Having observed this message, A decides whether to attack, B decides whether to resist, and C decides whether to intervene. Next, in the domestic electoral phase, the voters in nation C, having observed their leader's message and the outcome of the international crisis game, decide whether to retain their leader.

Of particular substantive interest is how each nation behaves after observing a particular foreign policy message. I define the probability that A attacks after observing message m as $\alpha(m)$. The probability of B retaliating after message m is $\beta(m)$. Given message m, C intervenes with probability $\gamma(m)$.

If a war occurs, then the expected outcome depends at least partly on the competence of the leaders involved. The more competent a nation's leadership, the greater the likelihood the nation will be victorious. Let θ_a represent the competence of A, θ_b represent B's competence, and θ_c represent C's competence. Let $\Theta = (\theta_a, \theta_b, \theta_c)$ be the competence of the leaders in each of the states. The probability that B wins a bilateral war is $q(\Theta)$. As B becomes more competent, $q(\Theta)$ increases. However, B is less likely to win if A is competent: $q(\Theta)$ is decreasing in θ_a. C, by intervening, increases the probability that B wins. The probability that B wins a multilateral war, $p(\Theta)$, is increasing in θ_b and θ_c and decreasing in θ_a. Hirshleifer (1989) provides an interesting discussion of the form that $q(\cdot)$ and $p(\cdot)$ should take as a function of resources. Here I assume that resources are fixed and instead consider how $q(\cdot)$ and $p(\cdot)$ depend upon leader competence. In the numerical examples, for simplicity, I assume that $p(\Theta)$ and $q(\Theta)$ are simple linear functions of Θ. Specifically, $p(\Theta) = (\theta_b + \theta_c - \theta_a)/6 + 0.6$, and $q(\Theta) = (\theta_b - \theta_a)/6 + 0.55$. This means that the most competent type of leader is about 17% more likely to be victorious than the least competent leader.

I assume that competence ranges between 0 and 1. Each leader knows her own ability but is uncertain of that of the others. In terms of incomplete information games, a leader's competence is her type (Harsanyi 1967–68). Let $\mu_i(\theta_i)$ represent the beliefs of the other players about the competence of the leader in nation i. For convenience, I assume that initial beliefs are uniformly distributed over the unit interval.

The payoffs from the international crisis game are summarized in Table 1. The most preferred outcome of B and C is the status quo. If B acquiesces, then B and

C receive a payoff of 0. If a war occurs, then their payoffs are the probability of victory: $p(\Theta)$ if the war is multilateral or $q(\Theta)$ if the war is bilateral, minus the cost (k) of fighting (k_b and k_c, respectively). A receives a payoff of 0 for the status quo; its most preferred outcome is acquiescence. If A becomes involved in a war, then its payoff is its probability of victory: $(1 - p(\Theta))$ for a multilateral war or $(1 - q(\Theta))$ for a bilateral war, minus the cost of fighting, k_a. In addition to caring about international outcomes, C receives a payoff of Ψ if she is reelected in the domestic electoral phase.

Elections

The election that follows the international crisis provides voters in C the opportunity to replace their leader. I assume that they evaluate her on two dimensions. First, they care about competence. Since it affects performance in future international events, the more competent they believe their leader to be, the more likely they are to retain her. Second, voters care about how their leader has performed on all other issues. Rather than explicitly modeling a leader's performance on these other policy dimensions, I assume that her ability can be represented by a single parameter, *bias*.

The bias parameter, which is analogous to Hess and Orphanides's (1995) domestic handling ability characteristic, reflects a leader's performance on issues other than the international dimension. It thus reflects the extent to which voters are predisposed to reelect the incumbent. If, for example, C has performed exceptionally well during her time in office ($bias \gg 0$), then the voters are likely to retain her, independent of how well she performs on foreign policy. The voters are indifferent about retaining a leader with a mediocre record ($bias \approx 0$), so their assessment of her international performance will have a large effect on whether she remains in office. When the electorate is predisposed against a leader ($bias \ll 0$), she is unlikely to survive in office, regardless of foreign policy performance.

I assume that voters are rationally retrospective, using the information they have learned about a leader's performance in deciding whether to retain her.[5] When the election occurs, the voters compare their current leader to the challenger. The greater their predisposition (bias) toward the incumbent and the more competent they think she is, the more likely they are to retain her. Having seen the foreign policy message, m, and the international outcome, z, suppose the voters believe that the expected competence of C is $E[\theta_c|m, z]$. Given these beliefs, the probability they will reelect the incumbent is $\Phi(E[\theta_c|m, z], bias)$, where $\Phi(\cdot)$ is a strictly increasing function of $E[\theta_c|m, z]$ and bias. This function reflects the greater difficulty for a challenger to defeat popular, competent leaders than unpopular, incompetent ones. For the numerical examples, I assume that $\Phi(\cdot)$ is the cumulative density function of a standard normal distribution evaluated at $(E[\theta_c|m, z] - 0.5 + bias)$. Thus, if bias = 0, then, ex ante, the probability of reelection is 50%. For brevity, I drop the bias term from the notation when it is not essential.

Voter Beliefs

Before the international crisis game, the voters' prior beliefs about C, $\mu_c(\theta_c)$, are uniform. Then the voters learn about C's type by her choices and performance. Given C's strategy, Bayes's rule defines the voters' posterior beliefs about C's type. For example, if only extremely competent types send message m', then upon seeing the message m', the voters (and other nations) believe that C is extremely competent. Alternatively, if all types send the same message, then the voters cannot update their beliefs upon observing the message.

Voters also learn about C's type if she engages in a war. Since C's type determines her ability to fight, I assume that C's competence is fully revealed if she becomes involved in a war. This is the updating rule used by Hess and Orphanides (1995), and it is used to calculate the numerical examples. An alternative and equally realistic assumption, however, is that the voters observe C's success during the war but do not observe her competence directly. Since voters know that more competent leaders are more likely to win, they can update their beliefs from the outcome of the war (the updating rule used in Richards et al. [1993] and Smith [1996a]). In this scenario, C's incentives remain as described, although the numerical calculations will differ. The main propositions are not sensitive to this choice of assumption.

RESULTS

I discuss the results in two sections. In the first, crisis behavior in the absence of credible threats is examined. I explain how the domestic conditions in C affect the behavior of all nations involved in the crisis. In addition to generating useful predictions about how domestic political conditions influence foreign policy, these results lay the groundwork for explaining how audience costs are generated. In the second section, I discuss why foreign policy statements are credible and why domestic electorates punish leaders who renege on their threats. I explain the intuition behind the results and illustrate their implications using several numerical examples. The technical results, proofs of the proposition, and equilibrium refinements are discussed fully in the Appendix.

[5] See Alesina, Londregan, and Rosenthal 1993; Ferejohn 1986; Persson and Tabellini 1990; Rogoff and Silbert 1988; and Smith 1996b for discussions about the differences between retrospective and prospective voters. I assume that international competence and domestic performance are independent.

Crisis Behavior in the Absence of Credible Threats: Babbling Equilibria

Consider the case in which foreign policy statements are uninformative. Suppose that, independent of her type, C randomly picks a foreign policy message. Such messages are typically referred to as babbling. Since each type is equally likely to send any particular message, neither foreign nations nor domestic audiences can update their beliefs about C's competence. Since messages are uninformative, all being identically interpreted, C is indifferent about which message to send. I characterize crisis behavior in babbling equilibria as follows.

Crisis Behavior. Competent leaders expect to perform well if they become involved in a war. Therefore, competent leaders are likely to behave aggressively and become involved in war (Banks 1990). The greater C's competence, the larger her marginal effect on the outcome of the war, and the more likely her assessment that intervention is worthwhile. Similarly, competent types of A and B expect to perform well should a war occur. Hence, when B is competent, B is more likely to resist; when A is competent, A is more likely to attack. Therefore, competent A types attack, competent B types retaliate, and competent C types intervene.

PROPOSITION 1: *For any beliefs about* C*'s type* $\mu_c(\theta_c|m)$, *the behavior of nations* A, B, *and* C *can be characterized by a unique triple:* $(\theta_a^*(m), \theta_b^*(m), \theta_c^*(m))$. A *only attacks if its type is greater than* $\theta_a^*(m)$, B *only retaliates if its type is greater than* $\theta_b^*(m)$, *and* C *only intervenes if its type is greater than* $\theta_c^*(m)$. *Having observed* m, *the probability that* A *attacks is* $\alpha(m) = 1 - \theta_a^*(m)$, *the probability that* B *retaliates is* $\beta(m) = 1 - \theta_b^*(m)$, *and the probability that* C *intervenes is* $\gamma(m) = \int_{\theta_c^*}^{1}(m)\mu_c(\theta_c|m)d\theta_c$.

Consider A's decision to attack. Given $\theta_b^*(m)$ and $\theta_c^*(m)$, A knows the probability that B retaliates and that C intervenes. In addition, A knows the distribution of competence for those types that resist and therefore knows how well he expects to do if he becomes involved in war. Anticipating how B and C will respond, each type, θ_a, calculates the expected payoff from attacking. The higher A's type, the larger this expected reward. $\theta_a^*(m)$ is simply the type that is indifferent about attacking. Those types above $\theta_a^*(m)$ attack, those types below $\theta_a^*(m)$ do not. B and C structure their decisions in an analogous manner.

Crisis outcomes are informative; they tell us something about each leader's type. Since the more competent types of A attacks, if the crisis outcome is the status quo, then A must be a lower type. If he were not, then he would have attacked. When A fails to attack, observers should shift beliefs about A's type downward. Alternatively, their beliefs about A's type increase if A attacks.

Observers can similarly update beliefs about B and C given their actions. For example, if a war occurs and C fails to intervene, then C must be a lower type. If she were a higher type, then she would have intervened. Therefore, following a bilateral war, C is viewed as less competent than previously thought. Since voters care about the competence of their leader, they are less likely to retain one who fails to intervene. Next I focus on how this information affects voting decisions and drives leaders toward hawkish policies.

Domestic Politics Makes Leaders Hawkish

PROPOSITION 2: *Domestic politics makes* C *more likely to intervene than international factors alone would predict.*

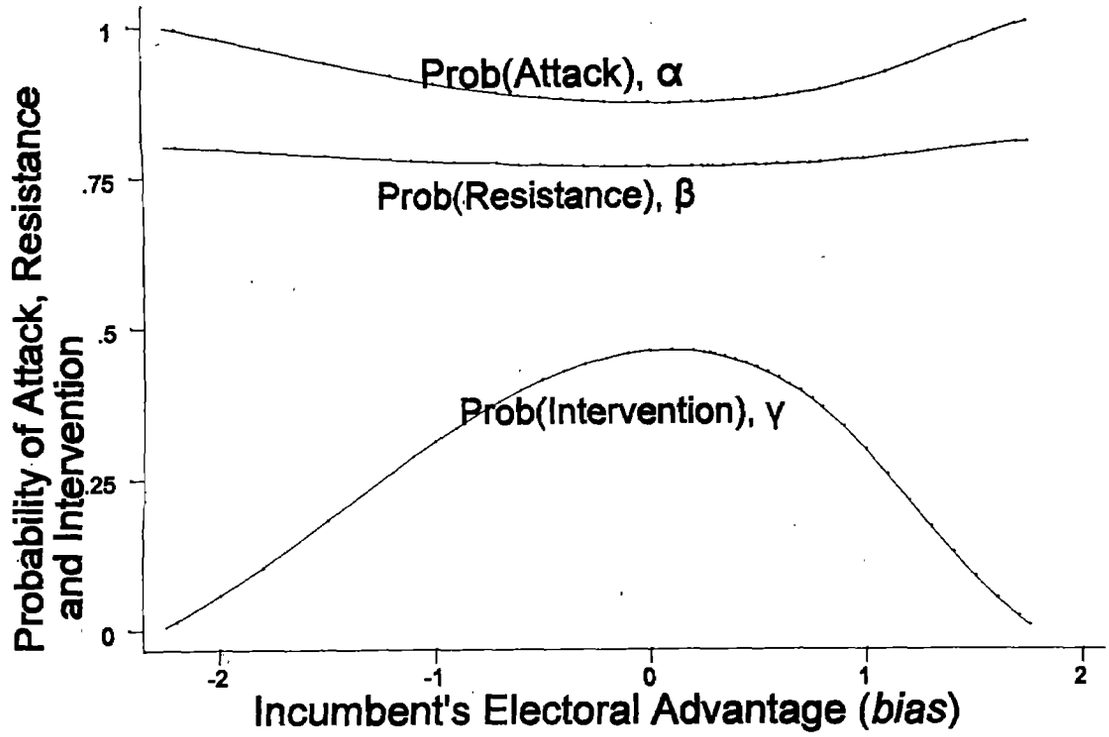
On the basis of international factors alone, C should only intervene if the marginal effect on the probability of victory outweighs the costs of fighting. Mathematically, C should only intervene if $\bar{p}(\Theta) - k_c \geq \bar{q}(\Theta)$, where $\bar{p}(\Theta) = \int_{\theta_b^*}^{1}(m)\int_{\theta_a^*}^{1}(m)p(\Theta)d\theta_a d\theta_b$, the average probability of victory if C intervenes, and $\bar{q}(\Theta) = \int_{\theta_b^*}^{1}(m)\int_{\theta_a^*}^{1}(m)q(\Theta)d\theta_a d\theta_b$, the average probability of victory for B in a bilateral war. Domestic political incentives encourage C to intervene even when this condition is not met. Hence, C may intervene when the costs outweigh the expected benefits. Why? The answer lies in what the voters infer when a leader fails to intervene.

Suppose that leaders decide to intervene on the basis of international concerns only. Under these conditions, more competent leaders intervene, and less competent types remain neutral. Suppose that in a particular case, on the basis of international factors alone, only types with competence greater than ½ intervene. When the voters see the outcome of the crisis, what do they infer about their leader? If C intervenes, then the voters observe C's type directly, so they know exactly how competent she is. If C does not intervene, then the voters only know that C's competence is less than ½. Thus, the expected competence of types who do not intervene is ¼.

Now suppose that C's type is 7/16. Since this is less than ½, on the basis of international considerations alone, she should not intervene. Yet, domestic considerations suggest otherwise. If C remains neutral, then the voters know that her type is less than ½. If she intervenes, then the voters will find out that her type is 7/16. The voters are more likely to retain a leader of competence 7/16 than one of expected competence ¼. By intervening, C enhances her reelection prospects.

Domestic political incentives bias leaders toward hawkish strategies. The magnitude of this effect depends upon domestic conditions. Suppose that C is extremely popular, that is, bias is high, and she is likely

FIGURE 2. Effect of Initial Domestic Conditions on Crisis Behavior (Numerical Example of a Babbling Equilibrium)



Note: The probability of reelection is $\phi(E[\theta_c|m, z] - 0.5 + \text{bias})$, where $\phi(\cdot)$ is the cumulative density function of a standard normal distribution. $p(\Theta) = (\theta_b + \theta_c - \theta_a)/6 + 0.6$, $q(\Theta) = (\theta_b - \theta_a)/6 + 0.55$, $k_a = 0.6$, $k_b = 0.5$, $k_c = 0.2$, $\psi = 1$.

to be reelected. She realizes that her international performance is unlikely to affect her chances in the next election. Since she expects to be reelected whatever her actions, her predisposition toward violence is small. Similarly, if C is extremely unpopular, bias $\ll 0$, then international performance alone is unlikely to make a big difference in the electoral outcome. Again, leader C's prejudice toward violence is small. Leaders have the largest predisposition toward violence when elections are anticipated to be close. When the voters are not heavily inclined toward a leader, their evaluation of international competence has a large effect on their decision. In the numerical example, I model the reelection decision using a cumulative normal density function. As is commonly observed in probit models, the marginal effect of foreign policy evaluation is greatest when, ex ante, the election is anticipated to be close. A numerical example is shown in Figure 2. The probability of intervention, $\gamma(m)$, is greatest when elections are close (bias ≈ 0). As the marginal influence of international competence on an election declines (high or low bias), C is less likely to intervene. This is consistent with empirical observations by Russett (1990; also see Morrow 1991) that mild but not severe recessions are associated with the use of force abroad.

This has interesting implications for the interpretation of foreign policy. To illustrate, consider an extremely unpopular leader (bias $\ll 0$) and one of average popularity (bias ≈ 0). The former is unlikely to survive even with a foreign policy success, therefore, she has little domestic incentive to intervene and the probability of intervention is low. The latter is more likely to intervene, given the marginal effect of foreign policy on elections. How voters interpret nonintervention varies. In the latter case, many types intervene, and the types who fail to intervene must be the very lowest types. If C fails to intervene, then the voters believe she has little competence. When the probability of intervention is low, however, failing to intervene carries less information; an unpopular leader who remains neutral may appear less competent to voters than previously, but the downward shift is small. How the electorate evaluates nonintervention depends upon initial domestic conditions.

The domestic context affects how a policy is interpreted, a leader's choice of actions, whether a leader sends informative messages, and how those messages are viewed. This has implications for empirical studies of the relationship between domestic politics and foreign policy. The model indicates that the magnitude of

foreign policy effects on public opinion and reelection depends upon initial domestic conditions.

The more leaders care about reelection (increasing Ψ), the greater the probability that they will intervene. Policy-driven leaders are less inclined toward ill-advised foreign policy actions than leaders who care primarily about reelection. This has electoral cycle implications (Gaubatz 1991; Morrow 1991; Stoll 1984). As new elections approach, we might suspect that leaders become increasingly concerned about reelection and therefore hawkish late in their term (Downs and Rocke 1993; Hess and Orphanides 1995; Smith 1996a).

Initiation, Retaliation, and Intervention

PROPOSITION 3: *Intervention and initiation are negatively related. As the probability that* C *intervenes increases,* A *is less likely to attack.*

Figure 2 illustrated the relationship between initiation, retaliation, and intervention with a numerical example. *C*'s propensity to intervene depends upon her internal political situation. When the voters' evaluation of her international performance strongly influences the electoral outcomes (bias ≈ 0), *C* is likely to intervene. If *A* attacks under these conditions, then *A* should anticipate a multilateral conflict. Yet, if *C* is popular (bias ≈ 1), then *C* is unlikely to intervene, and *A* should anticipate a bilateral conflict. Since *A* prefers a bilateral to a multilateral war, *A* is more likely to attack when *C* is extremely popular rather than when *C* has mediocre support. Interestingly, *A*'s decision to initiate violence depends, at least in part, upon domestic conditions in another nation.

In general there is an ambiguous relationship between retaliation, β, and intervention, γ. Intervention can either increase or decrease the probability of retaliation, depending upon the parameters of the particular case. Two competing factors influence *B*'s decision. The likelihood of intervention means that *B* anticipates more support in its fight against *A*, which encourages *B* to retaliate. Yet, since intervention should deter *A* from attacking, if *B* is actually attacked, then it knows its aggressor is extremely competent and may decide to acquiesce. In Figure 2 intervention and retaliation are negatively related, the latter influence outweighing the former, but this is not a general result; under other conditions β and γ are positively related.

The significance of proposition 3 is that domestic conditions in nation *C* affect foreign policy decisions in nations *A* and *B* (Morrow 1991). As the probability of intervention increases, the probability of initiation and retaliation declines. This creates a sample selection problem in testing how domestic affairs affect *C*'s foreign policy (Achen 1986; Fearon 1993; Morrow 1989; Smith 1996c). We can only observe *C*'s intervention decision when war occurs, but the probability of war depends upon what *C* is likely to do should war actually occur. In terms of empirical testing, this is problematic and leads to incorrect statistical inference unless we control for the selection effect.

The discussion of uninformative equilibria shows that (1) domestic politics can make *C* hawkish, (2) domestic politics in *C* affect *A* and *B*'s decisions, and (3) initial domestic conditions affect how voters interpret a leader's actions. These properties remain true in informative equilibria.

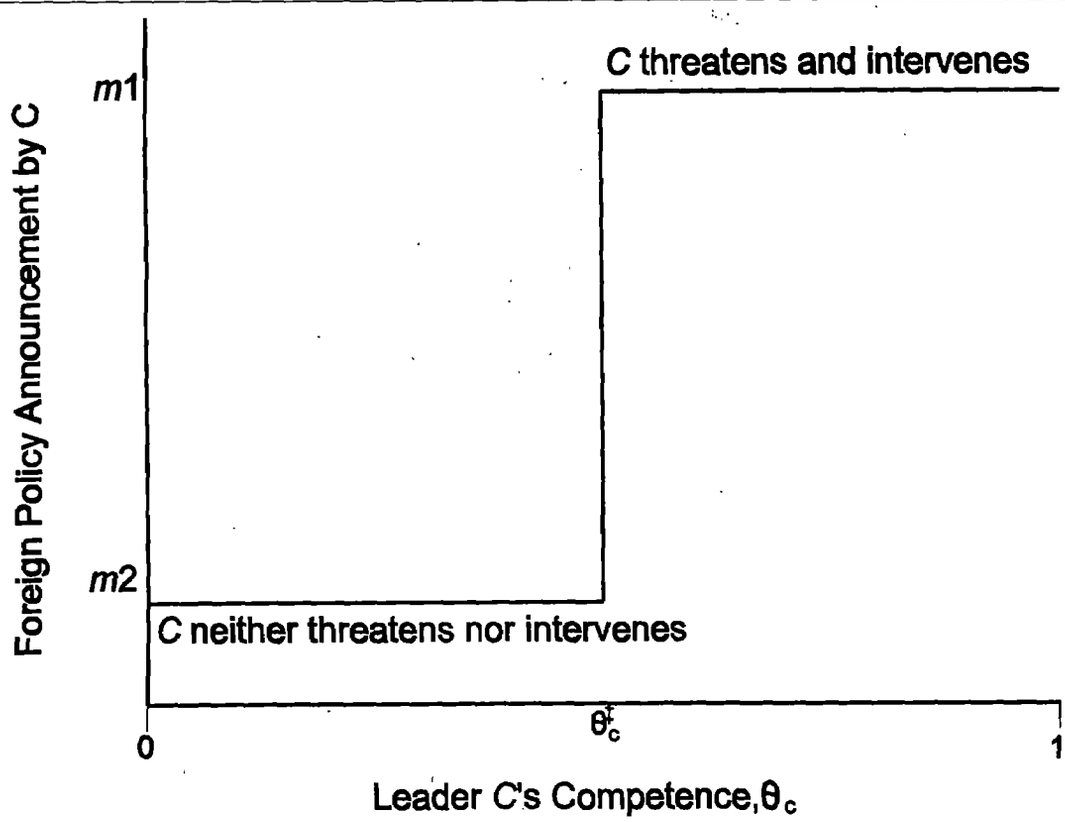
Making Threats Work

In order for threats to work they must convey information about a leader's intentions. When messages are costless to send, it is difficult for leaders to signal their type (Crawford and Sobel 1982). Yet, leaders can make credible threats in the international arena because of the domestic ramifications of failing to follow through. I show that if messages are informative, then the electorate wants to punish leaders who fail to honor their policy commitments. This generates audience costs and allows leaders to send informative messages. I explain the logic of the argument using an example. The properties of informative equilibria are formally characterized in the Appendix.

An Informative Message Equilibrium. In this equilibrium, competent leaders threaten to intervene; less competent leaders do not threaten. Should war occur, only those leaders who made threats intervene. Figure 3 shows a pictorial representation. More formally, there is a critical type $\theta_c^{\ddagger}$. Leaders who are more competent than this critical type ($\theta_c \geq \theta_c^{\ddagger}$) send message m_1 and intervene if a war occurs. Lower types ($\theta_c < \theta_c^{\ddagger}$) send message m_2 and remain neutral if a war occurs. Substantively, m_1 can be thought of as a threat to intervene. Since different types send different messages, the behavior of voters and foreign nations depends upon whether *C* makes threats. Before explaining why *C*'s threats are credible, I examine how voters and foreign nations respond to each message.

If the voters observe a threat (message m_1), then they infer that *C* is competent: Specifically, $\theta_c \geq \theta_c^{\ddagger}$. Therefore, the expected competence of *C* is $(1 + \theta_c^{\ddagger})/2$. Less competent types ($\theta_c < \theta_c^{\ddagger}$) do not make threats (message m_2). The expected competence of a type who sends m_2 is $\theta_c^{\ddagger}/2$. Since $(1 + \theta_c^{\ddagger})/2 > 1/2 > \theta_c^{\ddagger}/2$, leaders receive an increase in electoral support if they threaten, but the electoral support for leaders who remain passive declines. In equilibrium, types who send message m_2 never intervene, so the voters never get the opportunity to update their beliefs about these types. The voters may get additional opportunities to learn about those types who do make threats. If *C*'s bluff is called and a war occurs, then the voters observe whether *C* carries out her threats.

FIGURE 3. Leader C's Strategy in an Informative Equilibrium

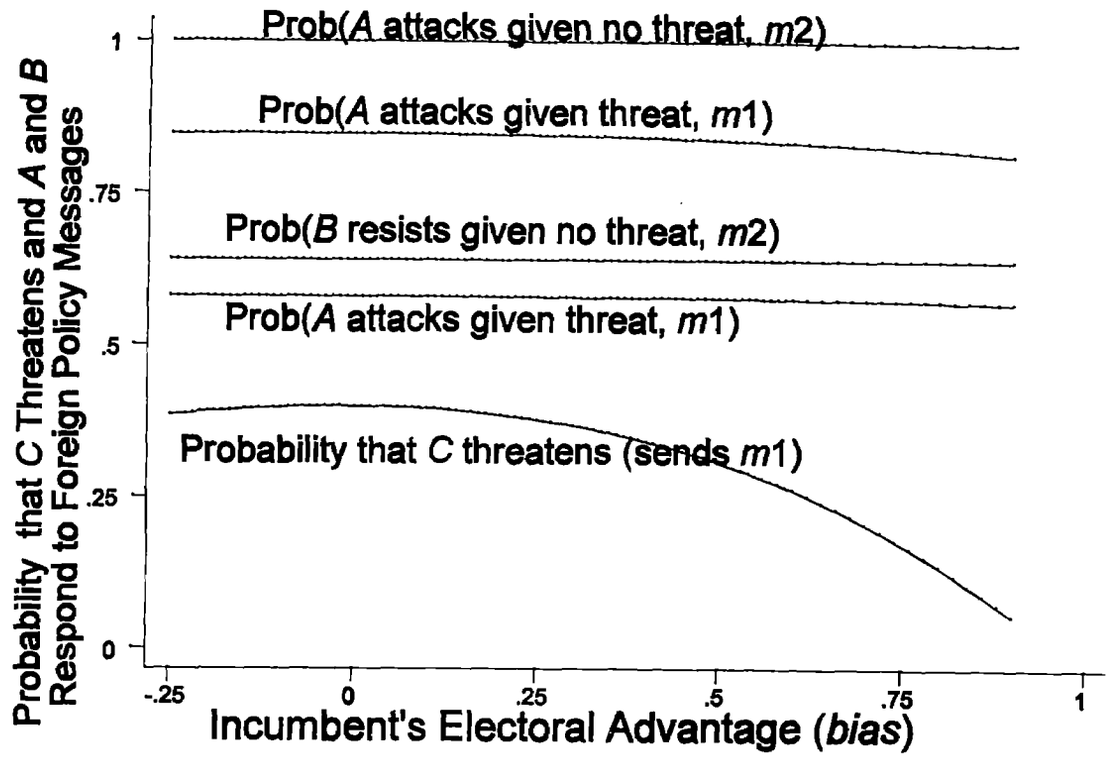


In equilibrium, only competent leaders make threats. These types also carry them out. If C makes a threat, then A knows that C is a competent type ($\theta_c \geq \theta_c^{\ddagger}$) who will intervene ($\gamma(m_1) = 1$). If C makes no threat, then A knows that all wars will remain bilateral ($\gamma(m_2) = 0$). Since A's probability of initiation declines as C intervenes more often, threats deter A from attacking ($\alpha(m_1) < \alpha(m_2)$). By making threats, C benefits internationally because A attacks less often.

There are clear advantages to making threats. C benefits domestically, by increasing her chance of reelection, and internationally, by deterring A from attacking. Why does every type not send m_1? Why are C's threats credible? Despite the advantages of sending m_1, there is the disadvantage of audience costs associated with unfulfilled threats. Leaders are punished electorally by the voters if they fail to keep their promises. In equilibrium, all types who threaten to intervene, m_1, actually do so. What should the voters believe if C threatens but does not intervene? Unfortunately, Bayes's rule is undefined in these circumstances. Therefore, any beliefs are consistent with the sequential equilibrium concept. Standard refinements (discussed in the Appendix) suggest that the type with the largest incentive to remain neutral once having sent m_1 is the lowest type, $\theta_c = 0$. Using this restriction, the voters believe the expected competence of a type who makes threats (sends m_1) but does not intervene is zero: $E[\theta_c|m_1, \textit{Bilateral war}] = 0$. Leaders who fail to fulfill their foreign policy commitments have little chance of reelection. $E[\theta_c|m_1, \textit{Bilateral war}] = 0 < E[\theta_c|m_2] = \theta_c^{\ddagger}/2 < E[\theta_c|m_1] = (1 + \theta_c^{\ddagger})/2$. This reduction in the probability of reelection is what Fearon (1994a) calls a domestic audience cost. It is a loss that leaders suffer when they do not honor their commitments. Yet, it is neither an exogenous fixed cost nor a mechanism to take the decision out of the leader's control (Fearon 1997). Leaders choose to honor their commitments because of the electoral consequences of failing to do so. These costs are endogenously generated by equilibrium behavior.

Although threats deter A from attacking, sometimes C's bluff is called, and C must choose whether to intervene. High types intervene because, being competent, they expect to perform well in the war, and it affords them an opportunity to show their competence to the voters. For less competent types, intervention is unattractive; they not only will perform badly but also will reveal their low competence to the voters. Yet, failing to intervene is also unattractive, since the voters will punish them electorally. Rather than risk this eventuality, low types send message m_2. For high types the cost of carrying out threats is low. For type $\theta_c^{\ddagger}$, the electoral and deterrence advantages of m_1 and the costs of having to intervene should a war occur exactly counterbalance the payoffs associated with sending m_2

FIGURE 4. Initial Domestic Conditions Affect Whether *C* Threatens and How *A* and *B* Respond in an Informative Equilibrium



Note: The probability of reelection is $\phi(E[\theta_c|m, z] - 0.5 + \text{bias})$, where $\phi(\cdot)$ is the cumulative density function of a standard normal distribution. $p(\Theta) = (\theta_b + \theta_c - \theta_a)/6 + 0.6$, $q(\Theta) = (\theta_b - \theta_a)/6 + 0.55$, $k_a = 0.9$, $k_b = 0.6$, $k_c = 0.6$, $\psi = 1$.

and remaining neutral (type $\theta_c^{\ddagger}$ is indifferent between sending m_1 and m_2). Types below $\theta_c^{\ddagger}$ prefer to play safe, sending m_2, rather than risk sending m_1 and having their bluff called. Types above $\theta_c^{\ddagger}$ prefer to send m_1 because, being competent, they face lower costs for honoring their commitments.

Figure 4 illustrates a numerical example of the informative message equilibrium. In the example, A always attacks unless it receives a threat, m_1. Upon observing a threat, A believes that C will intervene. C's cheap talk partially deters A from attacking because C's threat is credible.

Leaders are able to send credible messages because they mortgage their domestic political survival on their ability to honor their commitments. Without the ability of the electorate to remove leaders, leaders cannot credibly communicate their intentions. Domestic conditions limit whether leaders can make credible statements. Absolute rulers, who cannot be removed, cannot communicate their intentions to other nations. Similarly, lame-duck presidents find it hard to make credible commitments. Without voter retaliation to keep them honest, their threats are less likely to be believed.

Since domestic politics enables leaders to communicate credibly, domestic circumstances affect which messages are sent and how these messages are interpreted. The electoral advantage of convincing the voters that you are competent depends upon your initial domestic prospects. Thus, domestic factors affect both the probability with which C threatens and how other nations interpret these threats. Figure 4 showed that if C is popular (bias ≈ 0.75), and hence likely to get reelected, then fewer types consider it worthwhile to threaten (send m_1). Note the probability that A will attack, given the threat $\alpha(m_1)$, declines as bias increases. As fewer types send the threat, the average competence of the types who intervene increases. Since A has to combat a more competent type, it attacks less often when threatened. Interestingly, it is when the domestic incentives to send threats are small, and hence few types make threats, that threats have the largest deterrent effect.

General Properties of Informative Message Equilibria. The properties of informative message equilibria are characterized in the Appendix (lemmas 2, 3, and 4). The following statements summarize the substance of these lemmas. *In general, the existence of informative message equilibria depends upon the ability of leaders to*

mortgage their domestic political future. Equilibria can only be informative if war occurs with positive probability.

Without the possibility of war, leaders never face the prospect of having to honor their commitments. Under these circumstances, all types send any message that deters A, since they know that they will never have to make good on their promise. Since all types can send any message that benefits them, there is no useful communication. The possibility of war is necessary to keep leaders honest.

CONCLUSION

The model leads to two sets of results. First, it characterizes crisis behavior and shows how domestic conditions within a single nation affect foreign policy decisions by all nations involved in a crisis. Second, it shows how this linkage between domestic politics and foreign policy enables leaders to issue credible threats that deter adversaries. By explaining why voters punish leaders who fail to follow through on their threats, I generate a theory for the creation of audience costs. These costs make foreign policy statements meaningful.

Political leaders have two audiences, their domestic constituents and their foreign rivals. When forming foreign policies, leaders simultaneously balance these internal and external constraints. Hence, international events and domestic political survival are intrinsically linked, not through a simple unidirectional causal pathway, but via a series of strategic interactions at both the international and the domestic level.

A consequence of these linkages is that leaders adopt hawkish policies. Voters want to retain competent leaders and remove incompetent ones. Since competent leaders expect to perform better, they are more likely to intervene in international disputes. Given this, nonintervention signals lower competence. Leaders who want to retain office avoid being labelled incompetent by intervening. This desire to separate themselves from the low competence types creates a bias toward violent behavior. They enter wars in which, on the basis of international factors alone, they should not intervene.

The extent of this hawkish bias depends on domestic political circumstances. When the voters' evaluation of a leader's competence will have little effect on the election, leaders have little incentive to behave aggressively, and the bias toward violence is small. As a result, leaders who are either extremely secure or extremely insecure base their policies mainly on international considerations. For example, absolute rulers or lame-duck presidents intervene on the basis of international conditions alone. When leaders anticipate close elections, their competence is an important issue, and the incentive to distinguish themselves make them extremely hawkish. This does not necessarily imply that they become involved in more wars. As domestic politics makes leaders more aggressive, it also deters other nations from initiating violence. As the probability of intervention increases, $\gamma(m)$, the probability of initiation, $\alpha(m)$, declines. So elections reduce the opportunities for intervention but increase a nation's propensity to intervene in each particular case. In general, the net effect of domestic politics on the occurrence of war is ambiguous; it depends upon the characteristics of each case.[6]

The interaction of domestic and international politics enables leaders to make credible threats. Simple statements of foreign policy are informative because of the domestic electoral implications of not following through with an announced policy. The model explains how these audience costs arise. Since voters punish leaders who do not carry out their threats, leaders run a risk if they make threats. If their bluff is called and a war occurs, then they must decide whether to intervene, and failure to do so harms reelection prospects. A leader's competence determines the benefit of intervening. A highly competent leader may actually welcome the opportunity, since it allows her to demonstrate her superior abilities to the voters. Less competent leaders, although they want to avoid fighting, are also compelled to intervene to avoid being punished electorally. For the least competent leaders, intervention is not an option; their poor performance would provide yet more evidence of their low quality, and they will suffer the same electoral fate whatever they decide to do. This provides the justification for audience costs in the first place. If a leader fails to follow through on a threat, then the voters should infer that she is incompetent and want to replace her.

The model explains the origin of domestic audience costs. Threats deter foreign aggression through two mechanisms: commitment and information. Audience costs allow leaders to commit to a course of action that they otherwise would not undertake. Once a threat is made, a leaders faces a cost for backing down, and this cost makes intervention more attractive. Threats also work because they inform aggressors that their adversary is competent. Since competent leaders face the lowest costs for carrying out threats, they find the use of threats attractive. When an aggressor receives a threat, he knows that the leader not only is committed to carrying it out but also is competent. The prospect of becoming involved in a multilateral conflict against a competent adversary deters aggression.

When leaders make threats they jeopardize their domestic political prospects. The extent of their domestic vulnerability is associated with the degree of audience costs. This supports Fearon's (1994a) conjecture that electorally vulnerable democratic leaders

[6] Other studies that observe this ambiguity include Fearon 1994a, Morrow 1994, and Smith 1995.

have larger audience costs than their autocratic rivals. At the extreme, when leaders have nothing to risk, they cannot commit to carrying out their threats. Therefore, their foreign policy announcements are uninformative.

Foreign policy statements convey information about a leader's type and propensity to intervene. Such statements can deter foreign aggression because the pledge to intervene is credible. They also affect public opinion because they tell voters about the government's level of competence. The exact interpretation of a foreign policy message and its effect on other nations and public opinion at home depends upon initial domestic conditions. Empirical assessments of foreign policy should consider the context in which a particular policy is taken.

APPENDIX

Rigorous Description of the Model

C announces a foreign policy message, $m \in M$. Having observed this message, A chooses whether to attack (*att*, ~*att*). If A attacks, then B chooses whether to retaliate (*ret*, ~*ret*). If B retaliates, then C chooses whether to intervene (*int*, ~*int*). Let $Z = \{MUWAR, BIWAR, ACQ, SQ\}$ be the set of international outcomes. If A attacks, B retaliates, and C intervenes, then a multilateral war occurs, *MUWAR*. If A attacks, B retaliates, but C does not intervene, then a bilateral war occurs, *BIWAR*. Acquiescence (*ACQ*) occurs if A attacks but B does not resist. Finally, the outcome is the status quo (*SQ*) if A does not attack. The payoffs from the international outcome, shown in Table 1, depend upon the competence of the nations.

The competence, or type, of the nations is $\Theta = (\theta_a, \theta_b, \theta_c)$. Let $\mu_i(\theta_i)$ be the prior probability density over θ_i. Although I assume that θ_i $i = a, b, c$ is distributed uniformly over the unit interval ($\mu_i(\theta_i) = 1$ for $\theta_i \in [0, 1]$), all that is important for the following results is that the distribution over types is continuous and bounded away from zero for all types. Each nation knows its own type but only knows the distribution of the other nations' competence. Competence affects the expected outcome of the war: $p(\Theta) \rightarrow [0, 1]$ and $q(\Theta) \rightarrow [0, 1]$. B's probability of winning increases if C intervenes: $p(\Theta) > q(\Theta)$. I assume that both $p(\cdot)$ and $q(\cdot)$ are differentiable in all components: Specifically, $dp(\Theta)/d\theta_a < 0$, $dq(\Theta)/d\theta_a < 0$, $dp(\Theta)/d\theta_b > 0$, $dp(\Theta)/d\theta_b > 0$, $dp(\Theta)/d\theta_c > 0$, and $dq(\Theta)/d\theta_c = 0$. The competence of any nation that becomes involved in a war is revealed.

C's strategy is the pair (σ_c, s_c), where $\sigma_c : \Theta_c \times M \rightarrow [0, 1]$ and $s_c : \Theta_c \times M \rightarrow [0, 1]$. A's strategy is $s_a : \Theta_a \times M \rightarrow [0, 1]$. B's strategy is $s_b : \Theta_b \times M \rightarrow [0, 1]$. Thus, $\sigma_c(m, \theta_c)$ is the probability that type θ_c sends message m; $s_a(\theta_a, m)$ is the probability that type θ_a attacks having observed the message m; $s_b(\theta_a, m)$ is the probability that type θ_b retaliates having observed the message m; and $s_c(\theta_c, m)$ is the probability that type θ_c intervenes given message m. Let the notation $m(\theta_c)$ mean that θ_c sends message $m(\theta_c)$ with positive probability.

In addition to payoffs from international outcomes, the leadership of C receives a payoff of $\Psi > 0$ if reelected by the voters following the international crisis. The probability that the voters reelect C, $\Phi(E[\theta_c|m, z], bias)$ increases with the expected competence of C and the electorate's bias toward C (Hinich 1978). The justification for this assumption is that, after the international crisis but before the election, the voters learn about the qualities of the alternative leadership. At the election, the voters choose the leadership they prefer. When the government makes its decisions it does not know what information the voters will receive. If the information about the opposition party is distributed normally, then when the leader makes her decision, the voters' decision rule appears probabilistic. Once the information is revealed the actual voting decision is deterministic. Yet, prior to this revelation, voting appears probabilistic (Austen-Smith 1987; Smith 1996b).

The solution concept is sequential equilibrium (Kreps and Wilson 1982). This requires that the strategies of A, B, and C are utility maximizing, given the strategy of the other players and their beliefs. The strategy of the voters, reelecting C probabilistically, is utility maximizing by assumption. In addition, sequential equilibria require that all players' beliefs are consistent with the Bayesian rule.

Results

C's Decision to Intervene. C's expected utility for intervening is

$$U_c(int|\theta_c, m) = \int_0^1 \int_0^1 \mu_a(\theta_a|att)\mu_b(\theta_b|ret)p(\Theta)\, d\theta_a\, d\theta_b - k_c + \Psi * \Phi(E[\theta_c|m, MUWAR]),$$

where $\mu_a(\theta_a|att)$ is posterior beliefs about A's type, given that it attacks, and $\mu_b(\theta_b|ret)$ is posterior beliefs about B's type, given that it retaliates.

C's expected utility for not intervening is

$$U_c(\sim int|\theta_c, m) = \int_0^1 \int_0^1 \mu_a(\theta_a|att)\mu_b(\theta_b|ret)q(\Theta)d\theta_a d\theta_b + \Psi * \Phi(E[\theta_c|m, BIWAR]).$$

Since $p(\Theta)$ is increasing in θ_c, $U_c(int|\theta_c, m)$ is strictly increasing in θ_c. $U_c(\sim int|\theta_c, m)$ is constant in θ_c. Define $\theta_c^*(m)$ as the type indifferent about whether to intervene (if such a type does not exist, then set $\theta_c^*(m)$ to the appropriate limiting value; that is, if all types prefer to intervene, then $\theta_c^*(m) = 1$; if all types prefer not to intervene, then $\theta_c^*(m) = 0$). Thus, C's equilibrium strategy is $s_c(\theta_c, m) = 1$ if $\theta_c \geq \theta_c^*(m)$, and $s_c(\theta_c, m) = 0$ otherwise.

Let $\gamma(m)$ represent the probability that C intervenes, given the message m. Thus, $\gamma(m) = \int_{\theta_c^*(m)}^1 \mu_c(\theta_c|m)d\theta_c$, where $\mu_c(\theta_c|m)$ is the posterior distribution of θ_c, given the message m.

B's Decision to Retaliate. If B retaliates, then its expected utility, given its type, is

$$U_b(ret|\theta_b, m) = \int_0^1 \mu_a(\theta_a|att)\left(\int_{\theta_c^*(m)}^1 \mu_c(\theta_c|m)p(\Theta)d\theta_c + (1 - \gamma(m))q(\Theta)\right) d\theta_a - k_b.$$

If B does not retaliate, then it receives a payoff of $U_b(\sim ret|\theta_b, m) = 0$.

Since $p(\cdot)$ and $q(\cdot)$ are increasing in θ_b, $U_b(ret|\theta_b, m)$ is strictly increasing in θ_b. Define $\theta_b^*(m)$ as the type indifferent about whether to retaliate (if such a type does not exist, then

set $\theta_b^*(m)$ to the appropriate limit). Thus, B's equilibrium strategy is $s_b(\theta_b, m) = 1$ if $\theta_b \geq \theta_b^*(m)$, and $s_b(\theta_b, m) = 0$ otherwise. Ex ante, the probability that B retaliates, given the message m, is $\beta(m)$, where $\beta(m) = \int_{\theta_b^*(m)}^1 \mu_b(\theta_b) d\theta_b = 1 - \theta_b^*(m)$.

A's Decision to Attack. If A attacks, then its expected utility is

$$U_a(att|\theta_a, m) = 1 - \beta(m) + \int_{\theta_b^*(m)}^1 \int_{\theta_c^*(m)}^1 \mu_c(\theta_c|m)$$

$$(p(\Theta) - k_a) d\theta_b d\theta_c + (1 - \gamma(m)) \int_{\theta_b^*(m)}^1$$

$$(q(\Theta) - k_a) d\theta_b.$$

If A does not attack, then its expected payoff is $U_a(\sim att|\theta_a, m) = 0$. Unless $\beta(m) = 0$, in which case all types attack, A's expected payoff from attacking is strictly increasing in θ_a. Define $\theta_a^*(m)$ as the type indifferent about whether to attack (if such a type does not exist, then set $\theta_a^*(m)$ to the appropriate limit). Thus, A's equilibrium strategy is $s_a(\theta_a, m) = 1$ if $\theta_a \geq \theta_a^*(m)$; and $s_a(\theta_a, m) = 0$ otherwise. Ex ante, the probability that A attacks, given the message m, is $\alpha(m)$, where $\alpha(m) = \int_{\theta_a^*(m)}^1 \mu_a(\theta_a) d\theta_a = 1 - \theta_a^*(m)$.

PROPOSITION 1. *For any posterior beliefs about* C*'s type, given the message* m, $\mu_c(\theta_c|m)$, *there exists a unique triple:* $(\theta_a^*(m), \theta_b^*(m), \theta_c^*(m))$. A *only attacks if its type is greater than* $\theta_a^*(m)$, B *only retaliates if its type is greater than* $\theta_b^*(m)$, *and* C *only intervenes if its type is greater than* $\theta_c^*(m)$. *Having observed* m, *the probability that* A *attacks is* $\alpha(m) = 1 - \theta_a^*(m)$, *the probability that* B *retaliates is* $\beta(m) = 1 - \theta_b^*(m)$, *and the probability that* C *intervenes is* $\gamma(m) = \int_{\theta_c^*(m)}^1 \mu_c(\theta_c|m)\, d\theta_c$. (*Proof follows directly from the above description of* A, B, *and* C*'s decisions.*)

Bayesian Updating and Out-of-Equilibrium Beliefs. C's strategy, $\sigma_c(m, \theta_c)$, is the probability with which type θ_c sends message m. By Bayes's rule and initial uniform beliefs, the posterior beliefs about C's type are $\mu_c(\theta_c|m) = \sigma_c(m, \theta_c)/\int_0^1 \sigma_c(m, \theta_c) d\theta_c$. The posterior beliefs on the equilibrium path can be characterized, given the strategies above. If A attacks, then $\mu_a(\theta_a|att) = 1/(1 - \theta_a^*(m))$ if $\theta_a \geq \theta_a^*(m)$, otherwise $\mu_a(\theta_a|att) = 0$. If A does not attack, then $\mu_a(\theta_a|\sim att) = 1/\theta_a^*(m)$ if $\theta_a < \theta_a^*(m)$, otherwise $\mu_a(\theta_a|\sim att) = 0$. If B retaliates, then $\mu_b(\theta_b|ret) = 1/(1 - \theta_b^*(m))$ if $\theta_b \geq \theta_b^*(m)$, otherwise $\mu_b(\theta_b|ret) = 0$. If B does not retaliate, then $\mu_b(\theta_b|\sim ret) = 1/\theta_b^*(m)$ if $\theta_b < \theta_b^*(m)$, otherwise $\mu_b(\theta_b|\sim ret) = 0$.

Given the beliefs about C's type, $\mu_c(\theta_c|m)$, if C intervenes, then $\mu_c(\theta_c|m, MUWAR) = \mu_c(\theta_c|m) \cdot s_c(\theta_c, m)/\int_0^1 \mu_c(\theta_c|m) \cdot s_c(\theta_c, m)\, d\theta_c$; if C does not intervene, then $\mu_c(\theta_c|m, BIWAR) = \mu_c(\theta_c|m) \cdot (1 - s_c(\theta_c, m))/\int_0^1 \mu_c(\theta_c|m) \cdot (1 - s_c(\theta_c, m))\, d\theta_c$.

Along the equilibrium path, the Bayesian rule defines beliefs, but the rule does not tell us what nations should believe if they observe an action that should not occur in equilibrium. The assumptions we make off the equilibrium path are important because they affect incentives. The standard refinements (Banks 1991, 14–7) rely on finding the type with the largest incentive to deviate from the equilibrium path; players believe that the out-of-equilibrium message (action) is sent by this type. I use this intuition to define out-of-equilibrium beliefs. If nations take aggressive actions (A attacks, B retaliates) out of equilibrium, then they are believed to be the highest type, $\theta_i = 1$. I assume that the lowest types take out-of-equilibrium passive actions (A does not attack, B does not retaliate, C does not intervene). These refinements are important for constructing the example, but no propositions rely on these assumptions. Note that C's type is revealed if it fights, so beliefs are well defined if a multilateral war occurs.

Proof of Proposition 2. Given any set of beliefs, $\mu_a(\theta_a)$, $\mu_b(\theta_b)$ and $\mu_c(\theta_c|m)$, domestic reelection makes C more likely to intervene than international factors alone predict: $d\gamma(m)/d\Psi \geq 0$.

Proof: C is indifferent about intervening if

$$\int_0^1 \int_0^1 \mu_a(\theta_a|att)\mu_b(\theta_b|ret)[p(\Theta) - q(\Theta)] d\theta_a d\theta_b - k_c$$

$$+ \Psi(\Phi(E[\theta_c|m, MUWAR]) - \Phi(E[\theta_c|m, BIWAR])) = 0.$$

Given C's strategy, $s_c(\theta_c, m(\theta_c))$, $E[\theta_c|m, BIWAR] = \int_{\theta c(m)}^0 \mu_c(\theta_c|m)\theta_c d\theta_c$. If C intervenes, then, by assumption, its competence is revealed, $E[\theta_c|m, MUWAR] = \theta_c$. Therefore, $E[\theta_c|m, MUWAR] - E[\theta_c|m, BIWAR] > 0$. Since $\Psi > 0$, and Φ is increasing in expected competence, then $\Psi(\Phi(E[\theta_c|m, MUWAR]) - \Phi(E[\theta_c|m, BIWAR])) > 0$. Notice that this is true even if we relax the assumption that C's type is revealed when a war occurs. If C's type is not revealed, then $E[\theta_c|m, MUWAR] = \int_{\theta_c(m)}^1 \mu_c(\theta_c|m)\theta_c\, d\theta_c > E[\theta_c|m, BIWAR]$.

If domestic politics is unimportant, that is, either $\Psi = 0$ or $\Phi(\cdot)$ is constant in $E[\theta_c|m, z]$, then the type θ_c that satisfies the above equality is higher than the type that satisfies the equality if domestic politics matters. Therefore, domestic reelection makes leaders more hawkish. *Q.E.D.*

PROPOSITION 3. *For any* $\mu_c(\theta_c|m)$, *if a change in an exogenous parameter increases the probability that* C *intervenes, then it (weakly) decreases the probability that* A *attacks:* $(d\alpha(m)/d\gamma(m) \leq 0$, *where* α *and* γ *are differentiable.*

Proof: But suppose not. Suppose a change in an exogenous variable, such as bias or Ψ, increases the probability of intervention and increases the probability of initiation: $\gamma'(m) > \gamma(m)$ and $\alpha'(m) > \alpha(m)$.

(1) Consider B's decision to retaliate. $\alpha'(m) > \alpha(m)$ implies that $\theta_a^*(m)' > \theta_a^*(m)$. Therefore, if A attacks, then on average B fights a less competent type if it resists. In addition, since some types of C intervene that previously did not, and $p(\Theta) > q(\Theta)$, B's payoff for resisting strictly increases:

$$U_b(ret|\theta_b, m)' = \int_{\theta_a^*(m)'}^1 \left(\int_{\theta_c^*(m)'}^1 \mu_c(\theta_c|m) p(\Theta)\, d\theta_c \right.$$

$$\left. + (1 - \gamma(m)) q(\Theta)\, d\theta_a - k_b \right) > U_b(ret|\theta_b, m)$$

$$= \int_{\theta_a^*(m)}^1 \left(\int_{\theta_c^*(m)}^1 \mu_c(\theta_c|m) p(\Theta) d\theta_c \right.$$

$$\left. + (1 - \gamma(m)) q(\Theta)\, d\theta_a - k_b \right).$$

B's payoff if it surrenders is 0. Therefore, the type that is indifferent about retaliating is lower, that is, $\theta_b^*(m)' \leq \theta_b^*(m)$, which implies $\beta(m)' \geq \beta(m)$.

(2) Consider A's decision. In some cases in which B previously backed down, it now fights back. In addition, in some of these wars C now intervenes, whereas previously it did not. Therefore, A's value from attacking is reduced, as

more often aggression ends in war, and more often these wars are multilateral rather than bilateral. Hence, $U_a(att|\theta_a, m)$ decreases for all θ_a. But $U_a(\tilde{}att|\theta_a, m)$ is constant for all changes in $\theta_c^*(m)$ and $\theta_b^*(m)$. Therefore, the type that is indifferent about attacking is higher following the exogenous change: $\theta_a^*(m)' \geq \theta_a^*(m)$, which implies $\alpha(m)' \leq \alpha(m)$. This contradicts the initial supposition that $\alpha(m)' > \alpha(m)$. *Q.E.D.*

LEMMA 1: *In equilibrium, each nation's expected utility is a weakly increasing, continuous monotone in its type.*

Proof: I consider only nation C; the proofs for A and B are analogous.

(i) First show that $U_c(\theta_c, m(\theta_c))$ is weakly increasing in θ_c.

$$U_c(\theta_c, m(\theta_c)) = 1 - \alpha(m(\theta_c) + (1 - \alpha(m(\theta_c)\beta m(\theta_c))\Psi\ \Phi(E\ [\theta_c|m(\theta_c)]) + \zeta(\theta_c, m(\theta_c)),$$

where

$$\zeta(\theta_c, m(\theta_c)) = MAX\left\{\int_{\theta_a^*(m(\theta_c))}^{1}\int_{\theta_b^*(m(\theta_c))}^{1} p(\Theta)d\theta_a\, d\theta_b - k_c + \Psi\Phi(\theta_c''), \int_{\theta_a^*(m(\theta_c))}^{1}\int_{\theta_b^*(m(\theta_c))}^{1} q(\Theta)d\theta_a\, d\theta_b + \Psi\Phi(E[\theta_c|m(\theta_c), BIWAR])\right\}.$$

Since $p(\Theta)$ is increasing in θ_c and $p(\Theta) > q(\Theta)$, $\zeta(\theta_c, m(\theta_c))$ is weakly increasing in θ_c. Consider two types, $\theta_c' < \theta_c''$. If type θ_c'' mimics type θ_c', then its payoff is at least as good as the lower types: $U_c(\theta_c'', m(\theta_c')) \geq U_c(\theta_c', m(\theta_c'))$. Therefore, $U_c(\theta_c'', m(\theta_c'')) \geq U_c(\theta_c', m(\theta_c'))$.

(ii) Second, show that $U_c(\theta_c, m(\theta_c))$ is a continuous function in θ_c. Consider any two arbitrary types, θ_c and θ_c', where $\theta_c' = \theta_c + \varepsilon$. In equilibrium, $U_c(\theta_c', m(\theta_c')) \geq U_c(\theta_c, m(\theta_c))$, and $U_c(\theta_c, m(\theta_c)) \geq U_c(\theta_c, m(\theta_c'))$. As $\varepsilon \to 0$, $\zeta(\theta_c, m(\theta_c)) \to \zeta(\theta_c', m(\theta_c))$, and $\zeta(\theta_c, m(\theta_c')) \to \zeta(\theta_c', m(\theta_c'))$. Therefore, as $\varepsilon \to 0$, $U_c(\theta_c', m(\theta_c')) \to U_c(\theta_c, m(\theta_c))$. This implies that $U_c(\theta_c, m(\theta_c))$ is continuous. *Q.E.D.*

LEMMA 2: *If* $s_c(\theta_c', m(\theta_c')) = 0$, *then for all* $\theta_c < \theta_c'$ *either (i)* $\alpha(m(\theta_c))\beta(m(\theta_c)) = 0$, *or (ii)* $s_c(\theta_c, m(\theta_c)) = 0$.

Proof: Consider the type θ_c', which does not intervene given its equilibrium message $m(\theta_c')$. Now consider another type, $\theta_c'' < \theta_c'$, which sends the equilibrium message $m(\theta_c'')$ with positive probability. Suppose that, given this message, war occurs with positive probability, $\alpha(m(\theta_c''))\beta(m(\theta_c'')) > 0$. If type θ_c'' mimics type θ_c', then $U_c(\theta_c'', m(\theta_c')) = U_c(\theta_c', m(\theta_c'))$, because neither type intervenes given the message $m(\theta_c')$, $(\theta_c'' < \theta_c' < \theta_c^*(m(\theta_c')))$.

Having sent its equilibrium message $m(\theta_c'')$, suppose that type θ_c'' will intervene if a war occurs. The expected payoff for θ_c'' is

$$U_c(\theta_c'', m(\theta_c'')) = \theta_a^*(m(\theta_c'')) + \int_{\theta_a^*(m(\theta_c''))}^{1}\int_{\theta_b^*(m(\theta_c''))}^{1} p(\Theta)\, d\theta_a\, d\theta_b - k_c + \alpha(m(\theta_c''))\beta(m(\theta_c''))\Psi*\Phi(\theta_c'') + (1 - \alpha(m(\theta_c''))\beta(m(\theta_c'')))\Psi*\Phi(E[\theta_c|m(\theta_c''), MUWAR]).$$

Because $p(\Phi)$ is increasing in θ_c, type θ_c' receives a higher payoff than θ_c'' if it sends message $m(\theta_c'')$ and intervenes should a war occur, $U_c(\theta_c', m(\theta_c'')) > U_c(\theta_c'', m(\theta_c''))$. Since $m(\theta_c'')$ is an equilibrium message for type θ_c'', $U_c(\theta_c'', m(\theta_c'')) \geq U_c(\theta_c'', m(\theta_c'))$. Given that $U_c(\theta_c'', m(\theta_c')) = U_c(\theta_c', m(\theta_c'))$, this implies $U_c(\theta_c', m(\theta_c'')) > U_c(\theta_c', m(\theta_c'))$, but this is a contradiction of lemma 1. *Q.E.D.*

LEMMA 3. *If there exists an equilibrium message* m′ *such that* $\alpha(m') = 0$, *then either (i) all messages are uninformative, or (ii) there exists an alternative message* m″, *such that* $\alpha(m'') > 0$, *and* $E[\theta|m''] > E[\theta|m']$.

Proof: Consider an alternative message m''. There are three cases:

(1) $\alpha(m'') = 0$,
(2) $\alpha(m'') = 1$, $\beta(m'') = 0$, and
(3) $\alpha(m'')\beta(m'') > 0$.

Consider (1). $U_c(\theta_c, m') = 1 + \Psi\Phi(E[\theta_c|m'])$ and $U_c(\theta_c, m'') = 1 + \Psi\Phi(E[\theta_c|m''])$ for all θ_c. Therefore, if both messages are sent in equilibrium, then $E[\theta_c|m'] = E[\theta_c|m'']$, and messages are uninformative.

(2) $U_c(\theta_c, m') = 1 + \Psi\Phi(E[\theta_c|m'])$, and $U_c(\theta_c, m'') = \Psi\Phi(E[\theta_c|m''])$. Therefore, $E[\theta_c|m''] > E[\theta_c|m']$.

(3) $U_c(\theta_c, m'') = 1 - \alpha(m'') + (1 - \alpha(m'')\beta(m''))\Psi\Phi(E[\theta_c|m'']) + \alpha(m'')\beta(m'')MAX\{\bar{q}(\Theta) + \Psi\Phi(E[\theta_c|m'', BIWAR]), \bar{p}(\Theta) - k_c + \Psi\Phi(E[\theta_c|m'', MUWAR])\}$, where $\bar{q}(\Theta)$ and $\bar{p}(\Theta)$ represent the average value for type θ_c of bilateral and multilateral wars, respectively. Given $s_c(\theta_c, m)$ or the out-of-equilibrium beliefs, $E[\theta_c|m'', BIWAR] \leq E[\theta_c|m'', MUWAR]$. Since $MAX\{\bar{q}(\Theta), \bar{p}(\Theta) - k_c\} < 1$ for all Θ, $U_c(\theta_c, m'') \geq U_c(\theta_c, m')$ for some θ_c, which implies that $E[\theta_c|m''] > E[\theta_c|m']$. *Q.E.D.*

LEMMA 4. *If there exists an equilibrium message* m′ *such that* $\beta(m') = 0$, *then either (i) all messages are uninformative, or (ii) (nongeneric case) there exists an alternative message* m″ *such that* $\alpha(m'') = 0$, $\beta(m'') > 0$ *and* $E[\theta_C|m''] < E[\theta_C|m']$ *or (iii) there exists an alternative message* m″, *such that* $\alpha(m'') > 0$, $\beta(m'') > 0$, *and* $E[\theta_C|m'', BIWAR] < E[\theta_C|m']$.

Proof: Consider an alternative message m''. There are three cases:

(1) $\beta(m'') = 0$,
(2) $\alpha(m'') = 0$; $\beta(m'') > 0$, and
(3) $\alpha(m'')\beta(m'') > 0$.

Consider (1). Note that $\beta(m'') = 0$ implies $\alpha(m_c'') = 1$. $U_c(\theta_c, m') = \Psi\Phi(E[\theta_c|m'])$, and $U_c(\theta_c, m'') = \Psi\Phi(E[\theta_c|m''])$ for all θ_c. Therefore, if both messages are sent in equilibrium, then $E[\theta_c|m'] = E[\theta_c|m'']$, and messages are uninformative.

(2) $U_c(\theta_c, m') = \Psi\Phi(E[\theta_c|m'])$, and $U_c(\theta_c, m'') = 1 + \Psi\Phi(E[\theta_c|m''])$. If both messages are sent in equilibrium, then $U_c(\theta_c, m') = U_c(\theta_c, m'')$. Therefore, $E[\theta_c|m'] - E[\theta_c|m''] = 1/\Psi$. Note that this case is nongeneric.

(3) $U_c(\theta_c, m') = \Psi\Phi(E[\theta_c|m'])$ for all θ_c. Consider the payoff associated with message m'': $U_c(\theta_c, m'') = 1 - \alpha(m'') + (1 - \alpha(m'')\beta(m''))\Psi\Phi(E[\theta_c|m''']) + \alpha(m'')\beta(m'')MAX\{\bar{p}(\Theta) - k_c + \Psi\Phi(\theta_c), \bar{q}(\Theta) + \Psi\Phi(E[\theta_c|m'', BIWAR])\}$, where $\bar{q}(\Theta)$ and $\bar{p}(\Theta)$ represent the average value for type θ_c of bilateral and multilateral wars, respectively.

Case a: Suppose that having sent message m'', some types do not intervene: For some θ_c, such that $\sigma_c(m'', \theta_c) > 0$, $s_c(\theta_c, m'') < 1$. Thus, $U_c(\theta_c, m'') = 1 - \alpha(m'') + \alpha(m'')\beta(m'')\bar{q}(\Theta) + (1 - \alpha(m'')\beta(m''))\Psi\Phi(E[\theta_c|m''']) + \alpha(m'')\beta(m'')\Psi\Phi(E[\theta_c|m'', BIWAR]) = \Psi\Phi(E[\theta_c|m']).$

Since $\bar{q}(\Theta) > 0$, $(1 - \alpha(m'')\beta(m''))\Phi(E[\theta_c|m''']) + \alpha(m'')\beta(m'')\Phi(E[\theta_c|m'', BIWAR]) < \Phi(E[\theta_c|m'])$.

Given $s_c(\theta_c, \cdot)$, $E[\theta_c|m''] \geq E[\theta_c|m'', BIWAR]$. This implies that $E[\theta_c|m'', BIWAR] < E[\theta_c|m']$.

Case b: Suppose that having sent message m'', all types intervene: For all θ_c, such that $\sigma_c(m'', \theta_c) > 0$, $s_c(\theta_c, m'') = 1$. Let $\underline{\theta}_c$ be the smallest θ_c that sends m'' and intervenes. Since C's expected payoff, $U_c(\theta_c, m'') = 1 - \alpha(m'') + (1 - \alpha(m'')\beta(m''))\Psi\Phi(E[\theta_c|m''])) + \alpha(m'')\beta(m'')(\bar{p}(\Theta) - k_c + \Psi\Phi(\theta_c))$, is strictly increasing in θ_c, $U_c(\underline{\theta}_c, m'') \geq U_c(\theta_c, m')$. Therefore, $\sigma_c(m'', \underline{\theta}_c) > 0$, and $s_c(\theta_c, m'') = 1$ implies that if $\sigma_c(m', \theta_c) > 0$, then $\theta_c \leq \underline{\theta}_c$. Therefore, $E[\theta_c|m']) \leq \underline{\theta}_c < E[\theta_c|m'']$. $U_c(\underline{\theta}_c, m'') \geq U_c(\theta_c, m')$ implies

$$1 - \alpha(m'') + (1 - \alpha(m'')\beta(m''))\Psi\Phi(E[\theta_c|m''])$$
$$+ \alpha(m'')\beta(m'')(\bar{p}(\Theta) - k_c + \Psi\Phi(\theta_c))$$
$$- \Psi\Phi(E[\theta_c|m']) \geq 0.$$

Thus, $1 - \alpha(m'') + \alpha(m'')\beta(m'')(\bar{p}(\Theta) - k_c) < 0 \Rightarrow \bar{p}(\Theta) - k_c < 0$. Yet, since $\underline{\theta}_c$ intervenes, $\bar{p}(\Theta) - k_c + \Psi\Phi(\underline{\theta}_c) \geq \bar{q}(\Theta) + \Psi\Phi(E[\theta_c|m'', BIWAR])$. This implies that $E[\theta_c|m''] > E[\theta_c|m'] > E[\theta_c|m'', BIWAR]$. *Q.E.D.*

An Informative Equilibrium. In the informative equilibrium, if C is a type above $\theta_c^{\ddagger}$, then it sends message $m_1 \in M_1$ and intervenes if a war occurs. If C is a low type, $\theta_c < \theta_c^{\ddagger}$, then it sends message $m_2 \in M_2$ and does not intervene if a war occurs. If a multilateral war occurs, then the voters learn C's type. If C sends message m_1, then the voters believe that its average competence is $E[\theta_c|m_1] = (1 + \theta_c^{\ddagger})/2$. This is greater than the average competence of C if message m_2 is sent, $E[\theta_c|m_2] = \theta_c^{\ddagger}/2$. Yet, since types that send message m_2 do not intervene, C pays no domestic political cost for not intervening. Types that send message m_1 pay a huge domestic political cost for not intervening if a war occurs because voters believe them to be completely incompetent.

More formally, the following strategy profile $(\sigma_c, s_a, s_b, s_c)$ and beliefs (μ) constitute a sequential equilibrium if there exists $\theta_c^{\ddagger} \in (0, 1)$ that satisfies equations 1 and 2 below:

For any $m_1 \in M_1$ and $m_2 \in M_2$, such that $M_1 = M \backslash M_2$, $\sigma_c(m_1, \theta_c) > 0$ if $\theta_c \geq \theta_c^{\ddagger}$, $\sigma_c(m_1, \theta_c) = 0$ otherwise; $\sigma_c(m_2, \theta_c) > 0$ if $\theta_c < \theta_c^{\ddagger}$, $\sigma_c(m_2, \theta_c) = 0$ otherwise; $s_i(\theta_i, m_1) = 1$ if $\theta_i \geq \theta_i^*(m_1)$, $s_i(\theta_i, m_1) = 0$ otherwise; and $s_i(\theta_i, m_2) = 1$ if $\theta_i \geq \theta_i^*(m_2)$, $s_i(\theta_i, m_2) = 0$ otherwise for $i = a, b, c$, where $\theta_i^*(m)$ satisfies the definitions in proposition 1, and μ represents posterior Bayesian beliefs.

$$\theta_c^*(m_1) \leq \theta_c^{\ddagger} \leq \theta_c^*(m_2); \text{ and} \tag{1}$$

$$U_c(\theta_c^{\ddagger}, m_1) = U_c(\theta_c^{\ddagger}, m_2). \tag{2}$$

Proof: The beliefs of the players, given their strategies, were characterized above. The optimal strategies, given these beliefs, are uniquely characterized in proposition 1. In characterizing $\theta_c^*(m_1)$, I assume the out-of-equilibrium belief refinement that the type which does not intervene having sent message m_1 is $\theta_c = 0$.

Given that s_i are optimal strategies, all that remains is to show that C's message-sending strategy is optimal. If type θ_c sends message m_2 and does not intervene if a war occurs, then its expected payoff is $U_c(\theta_c, m_2) = 1 - \alpha(m_2) + \int_{\theta_a(m_2)}^{1} \int_{\theta_b(m_2)}^{1} q(\Theta) d\theta_a d\theta_b + \Psi[\Phi(\theta_c^{\ddagger}/2)]$. Given equation 1, if C sends message m_2, then its payoff is constant in θ_c. If C sends message m_1 and intervenes, however, then its payoff, $U_c(\theta_c, m_1) = 1 - \alpha(m_1) + \int_{\theta_a(m_2)}^{1} \int_{\theta_b(m_2)}^{1} (p(\Theta) - k_c)\, d\theta_a d\theta_b + \Psi[(1 - \alpha(m_2)\beta(m_2))\Phi((1 + \theta_c^{\ddagger})/2) + \alpha(m_2)\beta(m_2)\Phi(\theta_c)$, is strictly increasing in θ_c. If equations 1 and 2 hold, then $\sigma_c(m, \theta_c)$ is utility maximizing, given the strategies of the other players. *Q.E.D.*

Figure 4 showed a numerical example of an informative equilibrium.

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Hugo Grotius on Ethics and War

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Interest in the thought of Hugo Grotius on international law and ethics is justified inasmuch as he attempted to define a theoretical position between an idealism he thought counterproductive and an amoral realism he found unacceptable. Grotius constructed a system in which the moral authority of natural law was combined with the flexibility of human law. This required him to develop a special understanding of the nature and relation of these two types of law. In giving the law of nations, as a product of human will, the authority to suspend provisions of natural law, he provided for a code of international conduct that could permit injustice where necessary, without abandoning moral ideals altogether.

A certain ambivalence hovers today over the legacy of Hugo Grotius. The publication in 1625 of his great work, *The Law of War and Peace* (*De Jure Belli ac Pacis*), almost immediately brought him great renown throughout Europe, and for perhaps two centuries thereafter Grotius was considered authoritative not only in the fields of international law and ethics but also in natural law and moral philosophy generally. Then his reputation suffered a severe decline, due to the rise of positivism in international law and the disfavor of natural law thinking in moral philosophy. During the late twentieth century, Grotius's stock has risen again, at least in the fields of international relations and international law. In the latter, the intellectual force of positivism has been blunted by renewed appeals to objective moral principles, such as human rights. In the former, Grotius has been made the eponym of a general approach said to be a pragmatic mean between amoral Machiavellianism and idealistic Kantianism (Bull 1990; 71–2; Wight 1966, 89; cf. Lauterpacht 1985, 12).

Renewed interest in Grotius is appropriate today, inasmuch as the issues with which he grappled are permanent problems of foreign policy and ethics. In his approach to these problems, Grotius did indeed try to define a middle position between raw realism and the idealism of much of the natural law tradition. *The Law of War and Peace* represents not just a catalog of the provisions of natural law and the law of nations on a wide variety of subjects; it is an ambitious attempt to forge a theoretical system that can account for these two types of law and define their relationship. This system exhibits sufficient flexibility to take account of historically variable state practice, reflected in the law of nations, while retaining a grounding in the unchanging moral principles of natural law. My interest is in seeing how, or whether, Grotius succeeds in weaving these two principles into a just yet flexible code of international behavior.

Grotius's motives in attempting this synthesis are similar to those that drive much of current writing about international ethics. He tells us of his desire to combat the age-old view that there is no justice, or no justice in the relations of states.[1] He is also moved by horror at the savagery of the wars fought in Europe in the decades before his writing, wars motivated largely by religion. "Throughout the Christian world," he laments, "I observed a lack of restraint in relation to war, such as even barbarous races would be ashamed of . . . it is as if, in accordance with a general decree, frenzy had openly been let loose for the committing of all crimes" (*Prol.* 28, p. 20). Consequently, some were led to make the extreme claim that Christianity forbids all use of arms, says Grotius, but however pure their motives, these writers actually undermine the prospects for restaint by going too far in the opposite direction. Their extremism only discredits reasonable moral argument and robs morality of what effect it might have on practice. "For both extremes therefore a remedy must be found," he concludes, "that men may not believe either that nothing is allowable, or that everything is" (*Prol.* 29, p. 20). Excessive idealism only marginalizes itself; a more responsible approach must begin by making peace with certain uncomfortable realities.

We must always remember that Grotius's search for a middle path between realism and idealism is motivated by an essentially moral concern. His desire is for an *effective* set of moral restraints on states. This is what he tries to create by combining natural law with the more flexible institution of positive law, rooted in human volition. The resulting mixture anticipates the development of modern right per se, a development with which Grotius would only partially agree.

THE FOUNDATION AND POWER OF NATURAL LAW

Contemporary students of political philosophy may know of Grotius only through Rousseau's highly visible attack. In the opening chapters of *On the Social*

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Work on this essay was greatly facilitated by grants from the John M. Olin Foundation and from the National Endowment for the Humanities.

[1] *Law of War and Peace, Prolegomena* 3, p. 9. Hereafter, citations to this work will be either to the *Prol.* or by book, chapter, and section number, with page references to the Carnegie Endowment translation (Grotius [1625] 1925). Quotations are from that translation. My treatment of Grotius's thought will rely exclusively on this work. His earlier *De Iure Praedae Commentarius (Commentary on the Law of Prize)* (Grotius [1604] 1950) differs in some significant respects from the *Law of War and Peace*, so attempts to draw on it might cloud rather than clarify matters. One discussion of the differences between these two works can be found in Tuck 1993, chapter 5.

Contract, Rousseau ([1762] 1978. I.2, 4, 5; II.2) repeatedly rebukes Grotius for being a defender of slavery, both individual and collective, and a flatterer of tyrants. Grotius does take the position Rousseau describes, but for complicated reasons. He believes that slavery and absolutism are the exception rather than the rule, and even somehow against nature, but they can become legitimate under certain circumstances. They are important examples of one type of flexibility that Grotius finds in the interplay of natural and human law.

Following his predecessors in the natural law tradition, Grotius finds the root of natural law in human nature, which is "the mother of the law of nature" (*Prol.* 16, p. 15); natural law commands us to follow precepts that accord most with our true nature.[2] On Cicero's authority, Grotius (I.1.3, 34; I.2.1, 51–4, and passim) asserts that that nature is essentially social and rational. But unlike Cicero, Grotius construes the law that flows from this in a very restricted or minimalist way. Referring to the title of his treatise, he says (I.1.3, 34) that he uses the word *law* (*ius*) in a negative rather than an affirmative sense: whatever is not unjust is just. When he later limits the unjust to what is "utterly repugnant" to rational and social nature (I.2.1, 52), the requirements of justice seem to have been made as minimal as can be. Grotius's natural law bars only clear, positive injustices, acts that are unambiguously destructive of society. It is directed only to the bare preservation of social peace (Bull 1990, 78; Haakonsen 1985, 242–3; Tuck 1979, 73).

Grotius is taking issue with almost the whole of the late classical and Christian tradition of natural law. He acknowledges that some have declared all honorable things, and all of virtue, to be part of the natural law, but he is adamant that this is a misuse of the term (I.1.10, 39; cf. Kingsbury 1997, 30). For the Christians, indeed, natural law was a counsel of perfection, encompassing all the natural and perhaps even the supernatural virtues (Fortin 1996a, 206–10). The classical doctrine was likewise connected to a notion of higher perfection. Grotius follows Cicero in separating lower and higher principles of our nature and in stipulating that the duties coming from the higher principles, sociability and rationality, are more authoritative (I.2.1, 51). But it becomes clear rather early in the *Law of War and Peace* that Grotius does not mean this in the same way as his classical predecessor (Tuck 1993, 159). For Cicero and the Stoics, who fit broadly within the Socratic tradition in classical political thought, the precepts of rational nature form the basis of a moral outlook that is highly perfectionist and culminates in a form of rational justice that cannot be fully realized in any actual political order (Cicero 1975, 281–5). Grotius reinterprets natural law in such a way as to downplay if not remove entirely the higher reaches of the classical doctrine. In his opening outline of the various meanings of justice, Grotius adopts Aristotle's (1977) distinction between contractual justice (which he calls "expletive justice," *iustitia expletrix*), and distributive justice ("attributive justice," *iustitia attributrix*; I.1.8, 36–7). The first is based purely on legal possession, while the second relies on differential individual merit (in Aristotle's example, the flute is properly his who can use it best; *Politics* 1282b32–1283a3). For Aristotle, the second is clearly higher, though the first is obviously necessary for political order. Grotius, however, says that only the first is justice properly speaking (I.1.8, 36–7; see Tuck 1979, 68, 75, and 1993, 196–7). He acknowledges the second as part of morality, but not of justice (I.1.8, 37; I.1.7, 36; I.1.4, 35; I.1.9, 38).

Grotius in effect upbraids Aristotle for failing to see that justice is of this minimal nature. He is fully aware of the classical argument that the function of rational justice is preeminently to establish a proper distribution of goods in accordance with merit, but he is explicit in rejecting it: "Long ago the view came to be held by many, that this discriminating allotment is a part of *ius*, properly so called; nevertheless *ius*, properly defined, has a far different nature, because its essence lies in leaving to another that which belongs to him, or in fulfilling our obligations to him" (*Prol.* 10, p. 13). Grotius approvingly relates the well-known tale from Xenophon's *Cyropaideia* in which the young Cyrus is whipped for exchanging the cloaks of a large and a small boy. Cyrus took justice to be natural fit rather than legal or conventional ownership, but he was mistaken: Justice considers only the legal property rights of each (I.1.8, 37). The role of the legislator is to protect these, not to correct or improve upon them: "Society has in view this object, that through community of resource and effort each individual be safeguarded in the possession of what belongs to him" (I.2.1, 53). Justice has to do fundamentally with possessions or property, and it is determined by what one has rather than any notion of what one should have or deserves to have. Politics is thereby shorn of what used to be regarded as its higher and truest purpose.

This parsimonious understanding of justice is one reason Grotius gives a central place to "subjective right." He is often seen as a forerunner if not a pioneer of modernity in the emphasis he places on rights inhering in the individual, rather than "objective right," or justice defined with reference to some objective state of affairs.[3] Under the latter view, characteristic of

[2] Olivecrona (1971, 14–16) asserts the weakness of this inference from nature to duty ("the naturalistic fallacy") and infers as a result that Grotius's true argument must be that we are bound by God's authority, as the author of human nature (cf. Finnis 1980, 44; Olivecrona 1977, 79, 100–1). He then concludes that, *pace* many interpreters, Grotius's theory is in no way secular. I do not follow this reading, since Grotius gives no sign of believing the inference from nature to be a fallacy (cf. Rabkin 1997, 297). According to John Finnis, Aquinas also based right on this fallacy, if it is a fallacy (Finnis 1980, 46). Indeed, Olivecrona (1971, 17) continues that, on his understanding, there is no reason we should be bound by God's command any more than by conformity to nature, apparently leaving no basis for duty at all.

[3] The concept of subjective right was not new with Grotius. It has been discerned in various medieval authors, and especially in the late scholastic Suárez ([1612] 1944), who briefly preceeded Grotius (Haakonssen 1996, 23; Nederman 1996; Villey 1964). Grotius's innovation consists not only in the decisive preference he gives to subjective right but also in the minimal interpretation he imposes on it. This strongly points to modern theories of individual right. See Finnis 1980, 206; Haakonssen 1985, 240; Kingsbury 1997, 17; Rabkin 1997; Tuck 1979.

the classical and Thomistic approaches, the task of politics is to arrange society in accordance with an objectively ascertained just order, which is ultimately a suprapolitical order (Villey 1964, 120). This view has an obvious kinship with the perfectionist notion of right as fit or merit, though Grotius argues, interestingly, that both objective and subjective right could be interpreted in a perfectionist manner (I.1.4, 35). He claims that perfectionist subjective right is what Aristotle called "worthiness" (ἀξία, I.1.7, 36), viewed as an entitlement of the individual. This is highly anachronistic, as doctrines of objective right classically viewed justice in terms of duty rather than entitlement (Strauss 1953, 146–8, 181), but it matters little, since Grotius goes on to assert that this is not true or "perfect" subjective right in any case. True subjective right is only the legal property right that he earlier identified as the basis of justice strictly so called (I.1.4, 35). Social justice is thus the sum of entirely adventitious claims of individual right, not adherence to any objective "just order" (see Villey 1962, chapter 11). This outlook was shared by many of Grotius's philosophical successors.

Once again, Grotius does not deny that virtues other than justice may inspire us to greater perfection, or that there are obligations to go beyond the minimal dictates of justice in our moral lives. But these are "imperfect" obligations, which men may not be compelled to honor except by a duly established human law (I.2.1, 52; cf. II.25.3, 580). Thus, laws have been passed in some states against waste, ingratitude, or idleness, going beyond what is strictly obligatory by nature. Previous natural law thinkers would have placed at least some of these duties within that law. But by shedding the perfectionism of the older outlook, Grotius gains a great deal of latitude. At a minimum, he avoids the mistake of the pacifists, who set overly high moral standards and thereby lost all influence over the actual behavior of states (*Prol.* 29, p. 20). By focusing attention on those precepts of natural law that he finds to be truly obligatory, Grotius gives them a better chance of actually being observed. For in addition to being fewer in number, these are necessarily the easiest to follow (I.1.10, 38–9; I.2.1, 52).

NATURAL LAW AND HUMAN LAW

Moral minimalism is not the only way Grotius creates greater flexibility to cope with the "realist" pressures of international politics and war. The precepts of natural law themselves come to have a remarkable adaptability in the *Law of War and Peace*, accommodating themselves in effect to the unfolding of human history. Grotius follows a long-standing tradition in identifying a precivil state when men lived without benefit of government. In part because of his belief in human sociability, Grotius does not treat this state as normative in the way that the later social contract theorists did. Yet, he agrees with them that the law of nature derives in some sense from this original condition. The result is a strangely ambivalent view of the status or authority of natural law.

The evolution of property is a key example. Grotius, as did virtually every thinker before Locke, denies that property is established by natural law. Originally, he says, there was no private ownership; all was held in common, each taking whatever he needed for subsistence (I.1.10, 39; II.2.2, 186).[4] This condition was necessarily a state of great simplicity and innocence (characterized by "ignorance of vices rather than knowledge of virtue," II.2.2, 187), for communism is not compatible with either an advanced state of society or of human development. It was necessarily abandoned, Grotius says, when men were no longer content to forage for food and live in caves, and when they turned to the development of the arts and other kinds of knowledge (II.2.2, 188–9). Yet, since communism was originally established by nature, Grotius maintains that it is according to natural law (I.1.9, 39). What then happened to this portion of the natural law in the human march toward civilization?

Grotius's answer is that it was not overthrown but in effect was modified, on behalf of private property. This modification was instigated by human action, specifically by the compact or agreement that established the convention of private property (II.2.2, 189). Grotius says that when men agreed among themselves to establish private property, natural law supported the new state of affairs: It dictated that the new form of property be respected (I.1.10, 39; II.14.8, 385; III.20.9, 808). In a sense, natural law bent to the will of man, though Grotius traces the accommodation to a principle deeper than communism or private property, a principle of providing for human sustenance. Common ownership was the appropriate means to this end in the primitive condition, just as private property is today (II.2.2., 186).[5] Though natural law mandated a certain type of subsistence right in the beginning, it adapted itself to evolving human practices.

Slavery, one subject of Rousseau's attack on Grotius, is a more pointed case of natural law's modification. Grotius believes that men are naturally free: "Apart from a human act, or in the primitive condition of nature, no human beings are slaves" (III.7.1.1). He even admits that, "in this sense, it is correct to accept what was said by the jurists, that slavery is contrary to nature" and to natural law (III.7.1, 690). Nevertheless, he insists, this does not mean that the social institution of slavery is illegitimate (II.5.27, 255; II.22.11, 551). The reason, once again, is the primitiveness of the original condition to which this provision of the law of nature refers. Grotius suggests that condition was so

[4] In a curious (though by no means unique) anticipation of Locke, Grotius says that, in this condition, when one man had taken something from the common for his own needs, it became unjust for another to take it from him (II.2.2, 186). He does not, however, make this the root of a natural right to property. For extensive discussions of Grotius's theory of property, see Buckle 1991, chapter 1; Olivecrona 1977.

[5] This is also the foundation of the right to take others' property in extreme need. Grotius says that the original agreement on property must be understood to have a clause allowing such appropriation in cases of necessity, since its goal was to provide for human subsistence (II.2.6, 193). For a good discussion of the way private property flows from original communism by rational adaptation, see Buckle 1991, 9–13, 43–4.

primitive as to be virtually nonhuman. "For when liberty is said to be an attribute by nature of men and of peoples this must be understood of the law of nature which precedes all human conditions" (*jure naturae praecedente factum omne humanum*, II.22.11, 551). Why should civilized man be bound by a law adapted to such a condition? We are free by nature in the sense in which we are propertyless by nature, or indeed in which we are naked by nature (II.2.2, 187). If the original state is to be normative for society, we might as well conclude that we have a duty to remain unclothed. For the natural law to provide for both the primitive and the civilized condition, its provisions must be alterable, and some of its original precepts retired. In Grotius's view, natural liberty is one of these.

Grotius did not originate the notion that the provisions of the natural law, owing to their primitive origins, might be bent by civilized man. As indicated by his reference to the Roman jurists, cited above, the argument has late classical roots. Grotius the jurist was thoroughly familiar with Roman law, and cites the Roman legal writers more than any other sources in the *Law of War and Peace* (Zuckert 1994, 129). If one consults Justinian's *Institutes* on the subject of slavery, one finds in the opening chapters of Book One the assertion to which Grotius refers, that slavery is contrary to the law of nature (I.2, I.3, I.5). But the authors of that compilation then blandly assert that slavery has nevertheless been established by the law of nations (*ius gentium*; Justinian 1987, I.3, I.5). They offer no apology or explanation for this paradox. Its origin seems to lie in the way they understand the relation of the law of nations to the law of nature. The former is the set of practices settled upon by all or most nations. It is a common adaptation to common sets of circumstances (Justinian 1987, I.2), that is, to the common circumstances of developed society. For the jurists, its universal or near-universal character is testimony to the soundness or rationality of the adaptation. The law of nations can thus be interpreted as a law of reason, embodying reason's considered response to the (civilized) human condition (Justinian 1987, I.2). It arises in contradistinction to the law of nature, which derives fundamentally from the precivil condition. The law of nature, then, is primitive or incomplete, not fully adequate to the needs of men in society. The law of nature stands in need of supplementation, or even correction, by civilized man. The precepts of the law of nations, where they diverge from natural law, are therefore to be viewed as inventions of collective human reason to make civilized life possible. They may accordingly be placed above the law of nature, which is seen as primitive, low, or base. The law of nations, not the law of nature, provides for the needs of civil man.[6]

Thus, like property and other institutions, slavery might paradoxically be justified as a requirement of civilized life under certain circumstances. Rousseau ([1762] 1978, III.15) himself conceded this once, noting (without attempting to resolve) the contradiction of this principle with the rest of his thought. Grotius might have accused Rousseau of thereby failing to confront honestly an important political and theoretical problem. Grotius might find an even deeper problem in Rousseau's account of natural law, which begins surprisingly like his own. Rousseau ([1755] 1964, 95–6), famously, prided himself on discovering the true ground of natural law in a supremely primitive condition. The state Rousseau claimed to discover could be described, with even more justice than that envisaged by Grotius, as "preceding all human conditions." Grotius and the Roman jurists ask the sensible question whether a condition so remote, remote in more than time, can be of sufficient relevance to society to provide absolute moral standards of the type for which Rousseau argues so vehemently (Fortin 1996b, 162). If not, simple reliance on the precepts of a natural law arising out of such a condition is neither theoretically nor practically satisfactory. Grotius argues that for natural law to be relevant to civilized man, it must adapt itself to his condition.

These adaptations, moreover, are triggered by human will. In contrast to the Roman jurists, Grotius grounds the law of nations squarely in volition or consent, rather than rationality per se (I.1.14, 44).[7] Both slavery and private property, as institutions, are seen by him as products of compact. Rousseau maintained that human will is not competent to overcome natural freedom, but Grotius gives it greater power. Despite natural freedom, despite even the fact that liberty is part of the individual's "perfect" subjective right (I.1.5, 35), Grotius maintains that the individual is able to alienate his freedom, to give himself voluntarily into slavery. He admits that such a choice is base, unless motivated by some necessity, but insists that it is legitimate even if undertaken without good reason (II.5.27, 255). Absolute government, being a kind of collective slavery, is validated in the same way. All society, Grotius says, originates in a compact whose purpose is escape from the vulnerability of the precivil state (I.4.7, 149). But the compact itself can take many forms and need not provide for popular liberty. Peoples, like individuals, may contract themselves into slavery for any reason, or no reason (I.3.8, 103–4). They may even do so tacitly, for example, by acquiescing in a sovereignty taken by force (II.4.14, 229). And once such consent is given, it is irrevocable. Grotius

[6] I follow the interpretation of the Juristic treatment of natural law proposed by Zuckert (1989), who argues that the philosophic orientation described is a deliberate response on the part of the Roman jurists to the challenge to positive law created by the philosophic notion of a natural law. In order to defuse this challenge, they interpreted natural law as primitive or low, and positive law (including the law of nations) as civilized and rational. (See also Buckle 1991, chapter 1; Kingsbury 1997, 30; Nussbaum 1950, 20.) Thinkers before Grotius had adopted in various ways the view that natural law may be modified by human practice, such as Thomas Aquinas (1952, 1a 2ae, 94.5) and Suárez ([1612] 1944, 2.14.6, 16, 17). The view was also common among Renaissance humanists (Tuck 1979, chapter 2).

[7] The *ius gentium* for Grotius is properly speaking *only* a product of agreement, though the term, he notes, is sometimes used improperly to describe parts of the natural law as well (*Prol.* 16–17, p. 15; 40, p. 24). Here he diverges from Gentili ([1612] 1933, 8–10) and many of his other predecessors. For Grotius the set of norms binding on states, what we might call "international law," includes precepts of both the *ius gentium* and natural law.

does not shrink from following the logic of his position to the conclusion that a case may arise in which a foreign nation legitimately makes war to save a people from oppression, though the people have no right to revolt on their own behalf (II.25.8, 584). Natural law would be the basis of the the foreigner's right of "humanitarian intervention," but the oppressed have contracted their own right away.

Human choice is much less restricted by natural law in the creation of political order for Grotius than for many of the later contractarians. Grotius denies that sovereignty always resides in the people, or that government is ordained always for the benefit of the governed, principles that some had thought, and others would later think, to be binding on all societies (I.3.8, 103, 109). In his view, these are matters to be determined by the founding compact. It hardly needs to be pointed out that this does not mean all sovereignty is absolute. To the contrary, absolute power of the sort just described is probably a rarity. Grotius is explicit that most of the kings of his day do not hold absolute sovereignty,[8] and in most states political power *has* been ordained for the benefit of the governed (I.3.8, 110). But the natural law, despite its favor for "natural liberty," does not prevent human beings from choosing other arrangements.

This is not to say that human will is completely unrestricted in its choice of social institutions, or that positive law receives its authority exclusively from will. Though Grotius claims in his treatise scrupulously to distinguish principles that come from nature and those that are part of positive law (*Prol.* 30, p. 21), the two are not completely distinct for him. In the case of property, natural law supports the arrangements of human will, lending its authority to the very institution that alters it. The same kind of authority is extended to all of human positive law by a provision Grotius finds in the natural law, commanding men to abide by promises. Thus, property, slavery, and the political constitution, as creatures of compact, all receive the sanction of natural law (II.11.1, 328–30; Olivecrona 1971, 13, 282). Grotius says that natural law sanctions agreements in this way because there is no other way men could establish obligation among themselves (*Prol.* 15, p. 14).

It might be fair to say that, for Grotius, natural law passes through the very refractive medium of human choice or will on the way to positive law, lending its authority to a very wide array of human arrangements that alter its original content.[9] As Grotius says at one point, if human nature is the mother of natural law, it is only the "great-grandmother" of civil law (*Prol.* 16, p. 15). But just how refractive can this medium become? Are there any limits to how far or in what directions human agreement can bend natural law? In some cases, Grotius notes, human positive law forbids things merely discouraged by natural law. Here it may be said to tighten the moral laxity of the natural law. Thus, human law may mandate monogamy, while natural law allows polygamy (II.5.9, 234–5). In other cases, human law may decide questions left in suspense by natural law. The latter would permit ownership of some portions of the sea, but the law of nations has forbidden it (II.3.10, 210). Grotius also says that natural law limits ownership of property to those with reason, but the domestic law of many nations has overridden this limitation (II.3.6, 208). The same has happened to the natural law restricting the obligation of promises to those with reason (II.11.5, 332). Similarly, under natural law, only contracts equitable to both sides are valid, but the law of nations has made binding all formally valid contracts, "no matter how unfair" (II.19.6, 461; see II.12.26, 360). Natural law also countenances the punishment only of guilty individuals, but the nations have sanctioned reprisals and the "law of prize," whereby the goods of subjects can be seized for sovereign debts or as an act of war (III.2.1-2, 623–4). Finally, usucaption—title to property based merely on long possession, regardless of how it was originally acquired—is not recognized by natural law but has been established by the law of nations (II.4.2, 220; II.19.6, 461).[10]

With these last examples, we see human law becoming morally more permissive than natural law, perhaps even sanctioning violations of it. This would be highly questionable under traditional natural law doctrine, to say the least. Grotius takes care to point out why many of these exceptions to natural law were authorized by human law: If contracts or titles to property could be contested by anyone alleging the imperfect rationality of a contractor, the inequitable price of a commodity, or some ancient act of dispossession, there would be no end of domestic or international disputes (II.12.26, 361). Still, these cases do not seem to be like property and slavery, where natural law adapted itself to advancing human civilization. In order to account for them, we need to find a source of flexibility greater than any we have seen so far. Grotius finds such a source in the concept of "permissions."

"PERMISSIONS" AND THE LAW OF WAR

Grotius did not invent the notion of permissions under the natural law. Francisco Suárez, a late scholastic follower of Thomas Aquinas, discussed extensively whether human law could grant dispensations from the natural law, or "permissions" to violate it. In a treatise published only thirteen years before the *Law of War and Peace*, Suárez ([1612] 1944) acknowledges that this is allowable in a sense, but he follows Thomas in

[8] He says that most of them hold their thrones "as if in usufruct," with various limitations on their power stemming from popular consent (III.20.5, 806).

[9] This has led to the argument that the main role played by natural law in Grotius is to bolster human arrangements, through its principle *pacta sunt servanda* (Lauterpacht 1985, 17; Onuma 1993a). Zuckert (1994, chapter 5) argues that this combination of natural law and human agreement is what made Grotius so popular among seventeenth-century English Whigs. His natural law–based rationale for allegience to a historically conditioned constitutional order fit their needs perfectly.

[10] Grotius does say that this right is "in some degree derived from the law of nature." Moreover, the law of nature generally prefers the status quo, for the sake of peace (II.4.7, 225), a principle that would strongly favor usucaption.

limiting it largely to permissions that are merely apparent violations of natural law. Suárez brings private property under this head, understanding its relation to natural law in much the same way as Grotius. The natural law recognizes variations in circumstances, and on those grounds it can be said loosely to suffer "exceptions" which in reality are not exceptions (Suárez [1612] 1944, II.13.7, 262, and II.14.6, 270). Since reason will acknowledge these exceptions, they are ipso facto within the natural law and are recognized as such by God (I.9.6, 109). Suárez is adamant, however, that human law cannot abrogate, or grant a dispensation from, any true precept of natural law, for human law gains authority only from its grounding in natural law (II.14.8, 272, and see I.9, 105, 107). Suárez follows Aquinas in viewing human law essentially as an implementation of natural law.

There is one important type of exception to this that Suárez grudgingly acknowledges. Since for him, as for his predecessors in the Christian natural law tradition, the natural law is a mandate of moral perfection, commanding all the virtues (Suárez [1612] 1944, II.20.3, 352), not all of its precepts can realistically be enforced by the political order. Thus, lying and many other vices condemned by natural law go largely unpunished under human law. In that sense, the human law can be said obliquely to grant "permissions" for such acts. When this happens, human law does not abrogate the natural law, but it does grant (earthly) impunity to its violation. Suárez (II.20.4, 353) acknowledges that we must concede this power to human law, but we must also be clear that this is an unfortunate concession to human frailty, a sign of the grave imperfection of the political order.

Since Grotius's theme is war, itself a monument to human imperfection, perhaps the impression is inevitable that his view of permissions is much more expansive than that of Suárez. As a matter of fact, Grotius takes the concept of permission and makes it central to his account of the law of nations and the law of war. The use he makes of permissions in the arena of international relations is so extensive as to raise with new urgency the question about the relative authority of natural and human law. Grotius gives human beings greater authority to suspend the operation of natural law, by agreement among themselves, than any in the Thomistic tradition would have been willing to grant.[11] We must remember also that the natural law from which permissions are granted is itself much more lax for Grotius than for the Thomists.

Grotius places the subject of permissions before our eyes from the beginning of his work, and he does not conceal their morally questionable character. In the *Prolegomena* he makes a sharp distinction between acts that are permitted and those that are genuinely free from fault (*Prol.* 35, p. 22; see III.4.2, 641–3). Again and again, he reminds us that the granting of permissions does not make acts morally right (e.g., II.12.22, 358; II.12.26, 360; III.4.5, 645; III.7.6, 693; III.10.1, 716; III.10.3, 718–9). In the work's opening chapter, he says that a permission is not strictly speaking an operation of law or right at all, but a negation of its operation: It only suspends the action of a law under certain circumstances (I.1.9, 38). Somewhat later, he makes it clear that permissions may be extended to acts contrary to the law of nature, though such permissions are not "complete." They confer impunity but not a true moral liberty to perform the act in question (I.1.17, 49). Nonetheless, a permission does forbid others from hindering the act. Outside parties are effectively barred from enforcing whatever legal provision is addressed by the permission, even if it is a provision of the natural law.

In Book Two of the *Law of War and Peace*, Grotius says that human law cannot command what natural law forbids or forbid what that law commands (II.2.5, 192). But what kind of limit does this impose on human law? To begin with, it allows human law to forbid or command anything merely permitted by natural law or indifferent under it. Thus may polygamy be banned, though permitted by natural law. This kind of discretion was always allowed by natural law theory, as we saw earlier. Much more controversial is another possibility allowed by this formulation: Human law may permit an act forbidden by natural law (though it may not *command* it). Suárez conceded the necessity of this kind of operation, sometimes; but for him, to repeat, the natural law commanded perfection, not merely the "not unjust." This is not the case with Grotius, who in addition finds permissions that go considerably beyond such peccadillos as lying. Nor are such permissions uncommon: "The law of nations," he asserts at one point, "permits *many* things that are forbidden by the law of nature (III.4.15, 651–2; emphasis added). The result is nothing less than the sanctioning of injustice, often great injustice, in the conduct of war and elsewhere (see III.4.2, 642; III.4.15, 651–2, and generally the whole section on permissions in the law of war, III.2–III.9). Many of the rules having to do with the extent of belligerent violence, for example, and the treatment of prisoners of war are, according to him, the product of permissions against natural law. But the first and, as it were, the foundational compromise with justice in the law of war has to do with what should count as a legitimate cause of war, with who has a right to fight in the first place. In these matters, by Grotius's account, the law of nations has simply overthrown the notion of just war.

When speaking of the dictates of the law of nature in war, Grotius hews rather closely to traditional doctrine about just causes of war; he agrees that natural law dictates only one party (at most) in any war can be in the right (II.23.13, 565). It seems that any theory which takes seriously the notion of *ius ad bellum*, justice in the causes of war, must draw this conclusion. Yet, this entails that only one party in each war (at most, again) is authorized by justice to take up arms (II.1.18, 185;

[11] Outside the Thomistic tradition, arguments had been made that human law could grant genuine dispensations from the natural law. Suárez ([1612] 1944, II.14.1–4) addresses some of these arguments.

sec III.11.1, 722).[12] As to the other party, it must simply surrender without a fight (so long as its opponent fights within the rules of justice). It is enough to state this principle to see its futility as a practical norm for warfare. For this reason, traditional just-war doctrine fails the test of practicality proposed by Grotius in the *Prolegomena*, where he rejected pacifism on the grounds that it asks the impossible (*Prol.* 29, p. 20). As Grotius points out, both sides in a war typically claim to be in the right. It is often difficult for third parties to make an independent judgment on the matter, and it is always dangerous to act on it (III.4.4, 644; III.9.4, 704). As a result, the law of nations has decreed that both combatants will be extended belligerent rights, such as the right to kill, the right to plunder, the right to acquire territory, so long as they abide by such purely formal requirements as public declarations of war by the sovereign power (ibid.; also I.3.4, 97; III.6.2, 664). Furthermore, the outcome of the struggle, whatever it may be, will be accorded legitimacy under the same minimal restrictions (III.9.4, 704). For the ideal of just war, the law of nations has substituted the institution of legal war.

What this signifies, of course, is a blanket permission for violations of natural law by unjust combatants, so long as they abide by certain formalities. For Grotius, it is important to understand why this has come about. It is not only that the just and unjust parties to a conflict are often difficult to make out. Peace itself requires the abandonment of just-war doctrine. If rigorously applied, this doctrine would occasion endless disputes over the validity of past victories or defeats, the legitimacy of existing boundaries, the proper extent of damages or reparations (Kosai 1993, 275; Tanaka 1993, 279, 306). Just as the law of nations has been forced to suspend the natural law requirements for the equity of contracts and the rationality of contractors in order to avoid endless lawsuits, it must abridge just-war doctrine in order to prevent old conflicts being constantly reopened in the name of justice. By sanctioning the new status quo, the law of nations at least gets closure at the end of wars (III.20.11, 809; see II.4.8, 225; III.10.5, 720). Just-war doctrine could lead in practice to results the opposite of those it presumably intends.

The institution of legal or "public" war is not the only abridgement of justice in war allowed by the law of nations on humanitarian grounds. Its rules for *ius in bello*, justice in the conduct of war, grant combatants power over prisoners of war that extends far beyond what is allowed by natural law. To begin with, all captives may be made slaves. Slavery itself is countenanced by natural law in the roundabout way we have seen, but only if it arises by consent or as a punishment for crime (III.7.1, 690). By the law of war, however, captives become slaves regardless of personal guilt (ibid.).[13] Moreover, there is no suffering or torture to which these slaves cannot be subjected, even unto death, though by natural law masters do not have the power of life and death over slaves (II.5.28, 257; III.7.6, 694). The humanitarian motive Grotius finds behind this permission is to save the lives of the captives. If victors were not given absolute mastery over the vanquished, they might simply slaughter them (III.7.5, 692). It is in order to prevent this greater evil that a privilege is conferred on conquerors which is condemned by natural law. And it bears repeating that this privilege, like all the privileges of war, is granted to unjust as well as just combatants.

There is undeniably something tragic about permissions like this in the law of war, and something terrible about the necessity that compels the society of nations to extend them. Yet, as Grotius presents it, the law of nations has no choice but to bow to this necessity and sanction injustice in the name of humanity itself. The tragedy stems from the fact that precisely those who seek peace and are devoted to the cause of humanity must grant permissions to cruelty and injustice. The harsh realities of international politics, especially war, sometimes make justice impossible or even counterproductive. In one of the most harrowing passages of the *Law of War and Peace*, dealing with the root necessity to extend the rights of war to just and unjust combatants alike, Grotius states frankly that "the laws have come to terms with crimes (III.4.5, 646)."[14] Humanity and the common good alike require it. Grotius does nothing to disguise the ugliness of this fact. A casuist, using Grotius's own principles, might suggest that any injustices truly required by the common good could be brought under natural law via its grounding in rationality and sociability. Grotius does not attempt such an argument, and indeed it would be disingenuous to do so. The law of nations has simply granted impunity to crimes.

The permissions under discussion, however tragic, do at least have the redeeming justification of minimizing suffering. But there are others for which Grotius never offers any justification beyond the explicit or tacit agreement by which the community of nations has established them. For example, the law of war has decreed that states at war may kill anyone they find in enemy territory, whether soldiers or not, whether citizens of the enemy state or not (III.4.3, 644; III.4.6, 646). Grotius offers the explanation that harm may be feared from them (III.4.6, 646).[15] There is presumably no such justification for the slaughter of women and children, also permitted by the law of war (III.4.9,

[12] I am leaving out of account the one partial exception generally acknowledged by the just-war theorists: If both parties sincerely believe they are in the right, they both fight blamelessly, though one of them must be mistaken (II.23.13, 565; Suárez [1612] 1944, II.18, 337–8; Vitoria [1539] 1991, 282, 312–3). Gentili ([1612], 31–3) who in other respects was very influential on Grotius, is one exception.

[13] This appears to be one origin of absolute government in Grotius, sovereignty by conquest (just or unjust): III.8.1–2, 697–9.

[14] Grotius is here quoting Cyprian. In the same place, he also quotes a phrase used by Lucan, "right given over to crime" (III.4.5, 646).

[15] Grotius says that the law of nations also allows combatants to kill any subjects of the enemy nation, wherever they may be found—in their homes, at sea, or in foreign lands—on the grounds that war against a state is war against all its people (III.4.8, 647). He later suggests that the law of nature itself allows this (III.4.18, 654).

648).[16] With cases like these, in which grave injustices are permitted without any discernible humanitarian benefit, we are confronted with the most questionable exercises of the human power to make laws, or rather to grant permissions from natural law. These force us to return to the question of the potency of human will relative to natural law.

HUMAN WILL AND NATURAL JUSTICE

The structure of the third and final book of the *Law of War and Peace* suggests deep ambivalence on Grotius's part about the status of human permissions in the law of war. The first chapter presents a brief synopsis of what the law of nature permits in war. Chapters 2–9, roughly 100 pages, then discuss what is permitted in war by the law of nations. It is in this section that all the permissions discussed above appear. Finally, the last sixteen chapters of Book Three, roughly 150 pages, constitute an extended and very detailed plea for states to follow the higher standards of the law of nature, or the morality of pure Christian duty. This is the concluding section of the *Law of War and Peace* as a whole, and the way Grotius introduces it seems to question the ability of human law to issue permissions of the sort he has just discussed. The tenth chapter opens with almost a statement of contrition:

> I must retrace my steps, and must deprive those who wage war of nearly all the privileges which I seemed to grant, yet did not grant to them. For when I first set out to explain this part of the law of nations I bore witness that many things are said to be 'lawful' or 'permissible' for the reason that they are done with impunity, in part also because coactive tribunals lend to them their authority; things which, nevertheless, either deviate from the rule of right (whether this has its basis in law strictly so called, or in the admonitions of other virtues), or at any rate may be omitted on higher grounds and with greater praise among good men (III.10.1, 716).

Interpreters wonder, naturally enough, whether this means that Grotius denies to human law the power to grant permissions from natural law after all. The ensuing chapters are filled with statements of what nations should or should not do by the lights of morality, often explicitly contrasted with the permissions extended by the law of nations. Are these permissions legitimate or not? The matter is not a little muddled by Grotius's tendency in this extensive final section to appeal sometimes to natural law, sometimes to admonitions of virtues above and beyond natural law, and sometimes to the "law of love" found in the Gospel. This mixed procedure is visible already in the opening paragraph transcribed above. It makes sense perhaps as a rhetorical strategy to bring states around to a more moral behavior (See Onuma 1993b, 340; Tanaka 1993, 303), but it leaves Grotius's true attitude toward the power of human law unclear.

We might put the question this way: In Grotius's view, are permissions granted by human law through a true moral power it possesses, or do these permissions merely represent the impunity that results from the nations deciding in concert to ignore justice? If the society of nations conspires to permit a certain injustice, no earthly power will be able to prevent it. Are such permissions only an expression of this power, or do they convey some measure of genuine right? Grotius is certainly capable of using the concept of impunity in the former sense. He notes, for example, that all things have been said to be permitted to kings, merely because kings are beyond the reach of human punishment (III.4.2, 643; cf. II.12.26, 360). Yet, this permission has no moral standing. Similarly, he says that when a manifestly unjust legal decision is rendered, it does not carry any moral obligation, though citizens cannot legally (*licite*) resist the decision. If another nation or its citizens are harmed by such a decision, they may prosecute their claim by force (III.2.5, 627; cf. *Prol.* 25, p. 18). Grotius also stated that there is this truth to the proposition that right is merely the advantage of the stronger, that law largely fails of its outward effect unless a sanction is attached to it (*Prol.* 19, p. 16). Are the permissions of the law of war, which effectively block enforcement of parts of the law of nature, simply a manifestation of this Machiavellian truth?

This does seem to be the attitude suggested by the opening paragraph of Book Three's tenth chapter, cited above. Again, though, we should note the rhetorical interest of Grotius in making the permissions outlined in the previous section look questionable—he wants to dissuade nations from taking advantage of them. More important, we should recall that Grotius has laid the groundwork for permissions granted by human law from the beginning of his treatise. In the first chapter of Book One he asserts that, although permissions are not strictly speaking an operation of law, they do *oblige* third parties not to enforce the law being remitted (*obligat*, I.1.9, 38). Yet, to speak of obligation in this way grants a kind of moral standing. The obligation stems from the consent that Grotius finds at the root of the law of nations, an act of human will whose potency we have already explored.

Nor should we forget that Grotius placed an absolute limit on the ability of human power to abrogate natural law. Human law cannot command what natural law forbids or forbid what that law commands (II.2.5, 192): It may permit injustice but not command it. Though the permission itself may be worthy of condemnation from a moral point of view, it is at least theoretically coherent to say that human law may grant such permissions as required by the grave imperfections of this world. Thus did Suárez countenance certain permissions to vice. The permissions of the law of war are much more devastating, violating some of the most basic restrictions of natural law, but the pressures under which it operates are correspondingly more severe. Some of the permissions it grants have humanitarian justifications, stemming from the perverse realities of war. And the rest may be justified as implicit determinations on the part of the community of nations that it cannot effectively bar the conduct in

[16] Grotius finds that the law of nations is unsettled on the question of rape in war. It is "the law not of all nations, but of the better ones" that rape is impermissible (III.4.19, 657).

question, at least for the present. It is better to permit it than condemn the law to futility.

This still leaves Grotius open to the objection that he places no limit on the power of human law to suspend justice. Human law cannot command anything against the law of nature, but is there anything it cannot permit? Grotius does not seem anywhere to identify such a limit. Thus Rousseau's charge that he is a mere apologist for tyrants. Thus also does Kant charge that Grotius's "law" provides no meaningful restraint on the behavior of states and can be enlisted by any conqueror who cares to have such justification.[17] Kant does go on to say that the desire of nations to seek such moral cover itself reveals an impulse that promises eventual progress toward greater morality in state behavior. The hope for such progress is something Kant and Grotius share. Grotius is of the belief that throwing up a utopian ideal for international behavior, as pacifism or pure just-war doctrine do, is not the way to gain such progress. He believes we must grant the law of nations legitimacy as a genuine form of law, despite its moral flaws. We may, we must, point out those flaws and hold up to the nations a higher moral standard rooted in natural law and the virtues. But until the community of nations comes around to such a standard, we must realize that the law of nations is the only effective system of norms we have in international politics. Its imperfections are the price we pay to obtain something like the rule of law on this difficult terrain (cf. Lauterpacht 1985, 28). This is not an inconsiderable good. We allow law to bend before the necessities of international politics and war, that it might not break. Grotius's intellectual schema of mixed natural and human law is devised to allow for such flexibility.

It is precisely this flexibility which, while at present sanctioning crime, also allows for future progress. This, from Grotius's point of view, might be the greatest advantage of conceiving the relation of natural and human law the way he does. It allows him to countenance the moral permissiveness of international law without abandoning or compromising the superior standard of natural law. This, in turn, allows him to be the strong advocate of moral progress in international affairs that he is. The *temperamenta* that occupy the last 150 pages of his book testify to Grotius's hope that the moral law—natural law as well as the more stringent requirements of virtue and the Gospel—can exert influence on the behavior of states for the better. This influence can be increased if morality is championed by prudent spokesmen such as Grotius, whose exhortations make the necessary concessions to practical reality. Grotius speaks approvingly of progress that the European nations of his day have made in renouncing the enslavement of war captives (III.7.9, 696). Many of the developments of international law since his day would likewise be applauded by him, such as the acceptance of noncombatant immunity and the desuetude of the law of prize.

Grotius might say, with Kant, that the moral force of the natural law has inspired and directed this progress, whether or not his own exhortations contributed significantly to it. But he would also say that state behavior still has not come up to the level of natural justice, and that it remains important to give standing to international legal norms despite their imperfections. For we must realize that some of these imperfections will never be overcome. Some undoubtedly *should* never be overcome, inasmuch as unleashing the pure principles of justice in war and international politics might lead to more intractable conflict. Even in the concluding section of the *Law of War and Peace*, Grotius does not suggest that we will ever be able to overcome the necessity to grant belligerent rights to all legal combatants, just or unjust. Yet, this is the foundational breach of justice in the law of war. Twentieth-century thinking about war may have moved strongly toward differentiating aggressors and defenders, just and unjust, but it still has not been able to dispense with this basic principle within the law of war itself. Grotius's approach to the problem of morality in international affairs bows to this necessity and others. It allows us, and encourages us, to make moral progress where progress is possible, but it provides for the rule of law even where necessity is too strong to be overcome by the gentle promptings of justice.

[17] In this passage, Kant ([1795] 1983, 116) levels the same charge against Pufendorf and Vattel.

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The Political Determinants of International Trade: The Major Powers, 1907–90

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We test three arguments about the effect of international politics on trade flows. The first argument states that trade flows are greater between states with similar interests than those with dissimilar interests, the second that trade flows are greater in democratic dyads than nondemocratic dyads, and the third that trade flows are greater between allies. We examine trade flows between the major powers from 1907 to 1990. This period provides variation on all three independent variables of interest and allows us to separate the three arguments empirically. We estimate a gravity model of trade with the above political variables added. Our results demonstrate that joint democracy and common interests increase trade in a dyad, but alliances generally do not, even when controlling for polarity of the system.

Politics shapes international trade. We evaluate and elaborate three broad arguments that seek to explain how international politics shapes trade between states. These arguments explicitly assume that political factors have observable effects on international trade flows. This is so since governments in free market economies still set the rules under which firms import and export, while governments in managed economies directly negotiate the terms of trade.

Although all three arguments relate trade to politics, their foundations are different. Two of the arguments address the differing responses of economic actors to politics. The first is that conflict and the anticipation of conflict reduce trade flows between states, because economic actors respond to the risk conflict adds to a trading enterprise (Pollins 1989a, 1989b). The second argument is that pairs of democratic states will trade more than other pairs of states (Dixon and Moon 1993). Limited government assures economic actors that their positions will be protected from government fiat, and democracies and limited government have gone hand-in-hand historically. The third argument centers on the direct effect of politics on trade. According to this argument a state will be reluctant to trade with another state that may use its added wealth to increase its military capability against it. Furthermore, this effect is argued to be stronger in a bipolar system than in a multipolar system (Gowa 1994; Gowa and Mansfield 1993).

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We acknowledge with gratitude the support of this research by the University of California's Center for German and European Studies and Institute on Global Conflict and Cooperation. We thank Neal Beck for his answers to our methodological questions, Edward Mansfield for making his data available to us for comparison with our data, Scott Bennett for his help in establishing the reliability of some of our data, Michael Lewis-Beck for his comments on an earlier draft of this article, and the members of the Politics and Organization Seminar at Stanford University for their discussion of an earlier version.

We test these three arguments simultaneously in this article. Separate tests could be misleading because the different factors driving each scenario often occur together in the historical record. In particular, democracies have tended to have close relations and formed alliances with one another (Siverson and Emmons 1991). A test of one of these arguments that does not control for the other two may produce spurious results. Our analysis assesses the political determinants of trade flows among the major powers during most of the twentieth century. Those powers encompass dyads with good and poor relations, democracies and nondemocracies, and alliances in both bipolar and multipolar systems. This set of states offers variance on the three candidate explanations for the political determinants of trade. The data provide annual observations on trade flows between all directed dyads of major powers between 1907 and 1990, except during the two world wars and the year immediately following the end of each.[1]

Briefly, our results indicate that close political relations increase trade in a dyad; a militarized dispute does not significantly reduce trade between the disputants; democratic dyads trade more than other types of dyads; and an alliance appears to reduce trade between its members, which was especially the case after World War II.

The first two results address the argument on political conflict and trade, the third the argument on regime types and trade, and the fourth the argument about security concerns and trade. These results hold even when controlling for the other arguments, the distance between trading states, and the size of their population and economy.

This research has important implications for the broadest theoretical arguments in international politics. Realists contend that the demands of state survival in the international system mean that domestic political systems are irrelevant to international politics. In

[1] By directed dyads we indicate that each case reports state *i*'s exports to state *j* in a given year. Our data begin in 1907 because the availability for all series before that date is irregular.

the realist view, states must be concerned with the distribution of the benefits of trade because economic strength can be converted to a military advantage. Our evidence challenges this view, a theme to which we return in the conclusion.

We begin with summaries of the three arguments and the existing evidence. Next, we specify our testable hypotheses from these summaries. We then describe our sample of the great powers, their trade flows, their economic characteristics that we use as control variables, and the relevant political characteristics. Because the data are a cross-section of time series, we explain our estimation strategy. The results are presented, and we conclude with an evaluation of the three arguments and a discussion of directions for further research.

HOW DOES POLITICS AFFECT TRADE?

In this section we review the arguments about how politics can affect trade, and from these we develop the set of hypotheses to be tested. The first argument is that the political relations of states affect their levels of trade. The second asserts that democratic dyads trade more than other pairs of states. The final broad argument is that alliances between states affect trade, but the strength of the relationship is contingent upon the polarity of the international system.

The State of Political Relations

Political relations between states can directly and indirectly influence their trade. When political conflict arises between two states, if one of them enjoys a position of economic strength over the other, then it may consider restricting trade to advance its side of the conflict (Hufbauer, Schott, and Elliott 1990). Short of war, interstate conflict can lead to embargoes in the place of overt military violence; for example, it is illegal to bring a Cuban cigar into the United States. This is a direct effect; political conflict leads to government policy to restrict trade.

Conflict also can have an indirect effect on trade flows, since individual economic agents assess the possibility of political disruptions of their business when they consider establishing and continuing a trading enterprise. Such disruptions create a political risk for actors in trade. The more likely the conflict, the more profitable the trade must be to compensate for the risk of disruption. Consequently, political risk rises with both the likelihood and probable intensity of conflict; war produces the greatest disruption of trade, even though it is the least likely form of conflict. In peacetime, the anticipation of conflict determines the risk premium that trade enterprises face. If agents believe that conflict is likely, then only the most profitable businesses engaged in trade can justify the added political risk. All else equal, pairs of states with good political relations should have more trade than states with poor political relations. They are less likely to be in conflict in the future, and so the political risk to trade is relatively small. This effect of conflict is indirect; the threat of future government action to restrict trade leads to less international trade.

Drawing his theoretical position from a model based upon the choices of individual economic agents, Pollins (1989a) presents clear evidence that political relations affect trade flows. He begins with an economic model that analyzes how prices affect trade flows between pairs of states and then adds a measure of the amity/hostility between those states based on the annual summaries of the Conflict and Peace Data Bank (COPDAB) data (Azar 1980). Drawing data from the yearly exports of twenty-five states to six carefully chosen importer states in the period 1955–78, he found that imports increased as political relations improved after controlling for price differences.[2] In a parallel study, using a slightly different research design based on a gravity model of international trade (which we discuss below), Pollins (1989b) obtained similar results.

While Pollins joins together both the direct and indirect effects of conflict on trade, we wish to test them separately in order to assess the sensitivity of trade to their individual effects. Our first hypothesis addresses the *direct effect* of conflict on trade:

HYPOTHESIS 1a: *States in conflict with each other will have lower levels of trade than states not in conflict.*

In particular, we will examine how militarized interstate disputes affect trade flows between the disputants. Such disputes are public political conflicts of the highest magnitude short of war; if disputes do not reduce trade flows, then it is unlikely that any conflict short of war has a direct effect on trade.

Pollins (1989b, 739) recognizes the possibility of indirect effects of conflict on trade, but he does not explicitly test for them. Dixon and Moon (1993), however, report evidence of these indirect effects. Their results indicate that states whose interests are closest to those of the United States, as measured by similarity of voting in the United Nations, have higher levels of trade with the United States than other states.

Our second hypothesis completes this picture by addressing the *indirect* effect of conflict on trade flows.

HYPOTHESIS 1b: *The greater the degree of common interests between a pair of states, the greater will be their trade flows.*

It is important to note that this hypothesis assumes that common interests are an indicator of less conflict in the future. Divergent interests reduce trade indirectly by increasing the risk premium to firms engaged in international trade.

Joint Democracy

Internal political factors may also affect trade flows. Dixon and Moon (1993) argue that pairs of states with

[2] The combined exports of the twenty-five states in the data used by Pollins is approximately 67% of the world total. The six importers are the United States, the Federal Republic of Germany, the Soviet Union, the German Democratic Republic, Egypt, and India. These states are also included among the exporters.

similar political systems should have more trade than those with dissimilar systems. Nations may manage their trade policy to direct trade flows toward nations with similar systems. Added trade could enhance the political stability of their trading partners or reward them for their prior cooperative actions. Barriers to trade, then, are likely to be lower between states with similar systems than between states with dissimilar systems. Moreover, states with limited government should be particularly conducive to developing trading relations, since these systems have open polities and judicial systems (North 1990). Firms considering foreign trade must assess the political risk posed by the domestic institutions of the countries in which they deal. Confiscation of assets by a foreign government is one such risk; the chance that a foreign firm's position will not be taken seriously in a legal dispute with domestic actors is another. These risks reduce the expected profitability of businesses in international trade and reduce overall trade flows because businesses with marginal profitability drop out of the market. Of course, exceptionally profitable trade will still be pursued in the face of this political risk; at the margin, however, limited government in democratic systems encourages trade by reducing the political risk of arbitrary treatment by the host government. Limited government based on the rule of law gives foreign firms greater confidence that their interests will not be handled arbitrarily. Foreign firms then should be more willing to operate in economies overseen by limited governments.

In the period we examine, democracies typically have limited government and provide greater protection for all firms than do other systems. Their visible political-legal systems increase the confidence of firms that their interests will be protected by law, thus reducing the risk and raising trade flows into democracies. Furthermore, democracies are less subject to political pressure to protect domestic firms from international competition. Although all governments provide some protection to domestic firms, the open political processes of democracies are more likely than other systems to render the costs of such protection visible and allow countervailing pressure groups to organize and lobby against such protection (cf. Root 1994, chapter 3). Leaders in other systems are more likely to rely on the provision of private goods to their supporting coalition to hold office than are democratic leaders, and trade barriers that favor particular firms are one way for leaders to provide benefits to their supporters (Bueno de Mesquita et al. n.d.). To be sure, democracies are not immune to protectionist pressures, but they are better able to contain them than are more closed systems of government.

Economic agents also have material incentives to prefer trade with countries that have political and economic systems similar to their own. Business disputes in international trade may be resolved in the legal and political system of a trading partner. Economic agents are more likely to understand how to operate within political systems similar to their own than in those that differ. Firms familiar with operating in corrupt systems are more likely to understand the rules of the game in other corrupt systems than are firms accustomed to operating under the rule of law.

Economic systems themselves also matter, and they are closely connected to political systems. In particular, trade between an open and a managed economy presents special problems, since firms in open economies are likely to be privately owned, as opposed to state-owned firms or agencies in closed economies. Each type is accustomed to operating under a different system, free markets versus centrally planned economies, and neither type deals with the other frequently except in international trade. Different economic systems resolve disputes between economic agents in different ways, undermining trading agents' ability to predict their legal vulnerability. Open economies and open polities typically occur together, so the coincidence of these political systems reinforces higher levels of trade. Mansfield, Milner, and Rosendorff (1997) argue that pairs of democracies negotiate lower barriers to trade than do nondemocratic dyads precisely because legislatures in democracies often represent protectionist interests. The threat of trade war, and the higher barriers that would result, give democratic leaders the ability and motivation to negotiate lower trade barriers than leaders in other systems would negotiate.

Dixon and Moon (1993) test the effect of joint democracy on trade flows by analyzing U.S. exports to a range of countries between 1966 and 1983. They estimate the effect of common political systems on trade by determining whether the importer was democratic, as measured by the Polity II democracy scale (Gurr, Jaggers, and Moore 1989, 1991). Because the United States was a democracy during this period, the dyad was democratic if the importing nation was also democratic. The results indicate that U.S. exports were greater to democratic trading partners.[3]

Bliss and Russett (1998) draw upon a body of data that includes between 882 and 1,042 pairs of states in the period 1962 to 1989 to assess the extent to which shared democratic political institutions have an effect on trade between states. Though they use a broader sample of states and employ different control variables, they arrive at results fully consistent with those of Dixon and Moon: Democratic states trade more with each other than they do with states that have other

[3] Their results are more complex than this summary. Dixon and Moon report that the coefficient for institutionalized democracy has the correct sign but falls short of conventional levels of statistical significance. By examining the interaction of democracy and UN voting agreement, they conclude that joint democracy is contingent upon voting patterns; that is, joint democracy has its greatest effect when similar UN voting patterns are absent. That said, their initial judgment about joint democracy may be too conservative for two reasons. First, their hypothesis is clearly directional, and under the appropriate one-tailed test, the reported t-ratio of 1.8 is significant at the .05 level. Second, their data set includes only states that actually received exports from the United States. Nondemocracies such as North Korea, North Vietnam, Cambodia, Mongolia, Iran, and Cuba are missing, given the period of the study (1966–83). The inclusion of these states would undoubtedly have strengthened the relationship between democracy and trade.

types of political systems, even when controlling for such things as distance, the size of the economy, and alliance bonds.

Our next hypothesis deals with the effect of political systems on trade.

HYPOTHESIS 2: *Democratic dyads have greater trade flows than nondemocratic dyads.*

Limited government encourages trade by reassuring foreign firms that their trading interests will be protected from arbitrary treatment. Since democracies, in our sample, are generally also limited governments, they should be more able to resist pressure to shield domestic firms from international competition than are other political systems. Trade flows between democracies should be greater than between similar nondemocratic nations or pairs of states with differing systems. Because democracies tend to have limited government and an open economy, democratic dyads have matching economic and political systems that encourage trade.

Security

International trade may affect national security. Since trade increases both states' wealth, if a state were to invest its gains from trade into increased military capability, then it would gain a military advantage over its trading partner. Trade then could create a "security externality." A state might impede trade with another for fear that the latter would use the benefits of the trade to build up its military and so pose a greater threat to the former.

Gowa (1989, 1994) and Gowa and Mansfield (1993) develop a rigorous argument based on this idea. Enemies have good reason to fear what one another might do with their gains from trade, but allies presumably pursue similar ends and so could gain from increases in one another's military. Put another way, trade poses a negative externality between enemies and a positive externality between allies. The former must fear what each other would do with the gains from trade, while the latter can only benefit from greater military spending by their trading partner.

The polarity of the international system adds a further dimension to this argument. It is widely recognized that alliances do not always last, and today's ally may be tomorrow's enemy. Nations should be more willing to trade with their allies when they believe in the credibility and durability of their mutual commitment. When a nation fears that its ally will exit their alliance for the other side, it will be reluctant to allow that ally to gain the benefits of trade. Neorealists assert that alliances are more reliable in a bipolar system than in a multipolar system, and so the fear of exit is less in bipolar systems (Waltz 1979). Consequently, allies in a bipolar system should have higher trade than other dyads, since they anticipate that their alliance will persist and so do not fear the security externality that trade poses. In contrast, alliances should have little effect on trade in a multipolar system. According to neorealists, this is so since systemic pressures make one's ally as likely to be one's future enemy as any other state. Given the propensity for changes of alliance partners in a multipolar system, trading with one's ally can be as dangerous as trading with one's enemy.

Gowa and Mansfield (1993) test this state-centered argument by examining trade flows between the major powers in nine different years between 1905 and 1985. In their cross-sectional design they estimate the effects of alliances on trade flows in each of the nine years in their sample and find that bilateral alliances in the bipolar era have the strongest positive effect on trade, followed by multilateral alliances under bipolarity. Alliances in the multipolar era generally have no statistically discernible effect on trade between the major powers. Interestingly, Gowa and Mansfield use their data to explore other reasonable explanations for trade flows, including, most notably, joint democracy, but they report that the results show a positive effect for this in only one of their nine cross-sections.

The following two related hypotheses test this argument.

HYPOTHESIS 3a: *In a multipolar system, trade between allies should not differ from trade between nonallies.*

HYPOTHESIS 3b: *In a bipolar system, trade between allies should be greater than trade between nonallies.*

The reader may be concerned that Hypothesis 1b is so similar to Hypothesis 3a and Hypothesis 3b as to be indistinguishable. Alliances generally do indicate similarity of relations, but this is not necessarily the case (Dingman 1979; Morrow 1991, 906). Nations with very close relations do not necessarily need alliances to coordinate their policies. Indeed, as we show below, our operational indicator of close relations within a dyad is not too highly correlated with an alliance in a dyad, although it is based on alliance data. There are, consequently, sound theoretical and empirical reasons for maintaining all three hypotheses.

Our test of all five hypotheses allows us to sort out possible confounding effects among the variables we use to test the three general arguments. Democratic dyads have less conflict than other pairs of states (Bremer 1992; Maoz and Abdolali 1989). Democracies tend to have close political relations and enter into alliances with one another (Gaubatz 1996; Siverson and Emmons 1991). Allies generally, but not always, have good relations. Given these possible relationships, it is conceivable that a separate test of any one of the five hypotheses could be confounded by the effects of variables from another of the arguments. Our test controls for such confounding effects by examining all the variables at once.

Furthermore, the results reported by Pollins as well as Dixon and Moon are based upon data from the Cold War, an unusual period of history. Two strong ideological blocs faced each other, and domestic systems, common interests, and alliances are all closely linked in the history of the Cold War. To separate the effects of the three arguments, we must analyze trade patterns from both the Cold War and some other period. If the arguments above are truly general, they will survive a

competitive test across different periods of international history.

DESCRIPTION OF THE DATA AND MEASUREMENT OF VARIABLES

To test our hypotheses we rely on a cross-sectional time series broader than any used to date. This design has two important advantages: First, the tests allow us to separate the effects of each variable while controlling for the other possible political effects on trade flows. Second, the longer historical period introduces greater variation than previously in all the independent variables.

We collected data for all the major powers at the beginning of the twentieth century—the United States, Great Britain, France, Germany, Russia, and Italy—matching the sample of Gowa and Mansfield (1993). Each directed dyad of these major powers forms one of our cases. For example, exports from France to Germany is one case, and exports from Germany to France is another. The seven major powers produce 42 cases. Our period is 1907 to 1990.[4] The years 1914–19 and 1939–47 are excluded, due to the lack of trade data during the wars and its low reliability in the years immediately afterward. Each observation records the trade flow from one major power to another during one year in this domain.[5] Each of the 42 cases has 71 yearly observations, giving a total of 2,982 possible observations, but missing data reduce our set to 2,631 cases.[6]

Controls for economic conditions must be introduced to discern the effects of political variables by themselves. Like Pollins (1989b), Dixon and Moon (1993), and Gowa and Mansfield (1993), we begin with a "gravity model" of international trade drawn from economics (Aitken 1973; Anderson 1979; Bergstrand 1985). The gravity model is purely an empirical framework that has had success in predicting trade flows; it lacks a theoretical basis within the economic literature because it is not derived from macroeconomic premises. It serves us as a useful control for economic determinants of aggregate trade flows. The gravity model assumes that the physical distance between trading partners, the size of their economy, and their level of development affect the trade between them. The farther apart two states, the less their trade will be, since transportation costs are higher. The greater each state's GNP, the greater will be the amount of trade between them, because the marginal propensity to import is positive. Finally, all else equal, the greater is each state's population, the less trade, because larger domestic economies typically engage in less international trade.

We use constant values in U.S. dollars to measure exports and GNP in order to minimize the effects of changing price levels and inflation over time.[7] We first convert nominal values in national currency to current U.S. dollars and then convert the current dollar values to constant 1947–48 dollar values. The current exchange rate controls for national currency inflation relative to the dollar, and the constant dollar conversion controls for inflation of the U.S. dollar.

Our control variables from the gravity model are the *GNP of the Exporter* (GNPI) and *GNP of the Importer* (GNPJ), in millions of constant U.S. dollars; the *Total Population* of each trading partner (POPI and POPJ), in millions of people; and the *Distance* between states (DISTANC).

Our dependent variable is the value of *Exports from State i to State j*, in millions of constant U.S. dollars (EXP).

To test our five hypotheses, we employ independent variables measuring conflict, interests, joint democracy, and the presence of an alliance under conditions of both bipolarity and multipolarity. We discuss each of these in turn. Other studies of the relationship between conflict and trade use event data to assess the level of conflict in a dyad. Instead, we use the presence of a *Militarized Dispute* for three reasons. First, event data include a variety of acts that directly reduce trade, such as retaliatory sanctions and trade embargoes. The first argument contends, however, that conflict leads states to adopt measures that directly reduce trade. Including measures that reduce trade as evidence of conflict biases the study in favor of a positive result. Second, if international conflict leads to national policies to reduce trade with the other side, then certainly militarized disputes—the most signal conflicts in which nations engage, short of war—should lead to a reduction in trade. If disputes do not reduce trade, then we must question the direct effect of political conflict on trade flows. Third, event data measuring conflict between states in the period before 1939 are not available. Our use of disputes as the measure of political conflict allows us to include years before World War II, increasing the variance in our independent variables.

We test the direct effects of conflict on trade flows by

[4] After 1949, data are for the Federal Republic of Germany only; prior to 1917, data for Russia are used.

[5] A referee was concerned that our use of directed dyads could result in observations that are not independent of one another. In particular, many of our measures of the independent variables are dyadic, and so we have two directed dyads with the same values of many of the independent variables. A conservative approach would be to eliminate one of each pair of directed dyads in our sample, which would reduce our sample size by one-half. An alternative adjustment would be to consider our effective sample size to be one-half the actual sample size. In that case, simply divide all the t-scores by $\sqrt{2}$. We discuss the consequences of such an adjustment for our results in a subsequent footnote.

[6] The missing data are limited to the economic components of the gravity model. The elements of the missing data have different consequences. There are 17 nation-years for which export data are lacking. Because we use directed dyads, this results in missing data for 102 cases. Ten nation-years have no population figures, which creates missing data for 120 cases, since the population of the exporting state in one directed dyad becomes the population of the importing state in the respective directed dyad for the importer. Similarly, GNP is missing for 21 nation-years, creating missing data for 252 cases. Of course, in some cases the missing data overlap. In total, because one element or another of the economic and demographic information is missing, the size of the data set is reduced by slightly more than 11%.

[7] Details on the sources of the data and measurement procedures for the economic variables are given in the Appendix.

examining whether the importing and exporting states were engaged in a militarized interstate dispute (MID) with each other in that year. A militarized interstate dispute occurs when at least one member of the dyad has made a threat to use force against the other (Gochman and Maoz 1984). We code dyad-years with a militarized interstate dispute as 2, those without as 1.[8] The hypothesized direct relationship between political conflict and trade (Hypothesis 1a) predicts that the sign on this coefficient should be negative and statistically significant.

Following Bueno de Mesquita (1975, 1981), we operationalize *Common Interests* in a dyad of states by the tau_b correlation of their alliance portfolios (TAU). This revealed-choice measure of interests assesses the complete array of alliances that the member states of the dyad hold in common and apart. To assess the tau_b measure for a dyad, a four-by-four table that compares both states' alliance commitments is constructed. The four rows and columns of the table correspond to the four levels of commitment, in descending order, given in the Correlates of War military alliance data: defense pact, nonaggression pact, entente, and no alliance. Each state in the system, including the members of the dyad, is placed in the cell corresponding to the commitments that each member of the dyad has with that state. When placing the dyad members with respect to each other, each is assumed to have a defense pact with itself. The tau_b measure of shared interests is the tau_b value of the resulting table of the alliance commitments of the states in the dyad with all states in the system. Although tau_b is correlated with the presence of an alliance in the dyad, the existence of an alliance accounts for only about half the variance in the tau_b values in our sample, so *there is significant variation in the* τ_b *values for which the presence of an alliance in the dyad does not account.*[9] Negative tau_b values should also weakly predict the presence of conflict in a dyad, in line with the argument that common interests are a signal of lower political risk. The theoretical range of tau_b values is -1 to 1. We add 1 to these to get the value of TAU in order to avoid the possibility of multiplying the value predicted by the other variables by a negative number.[10] For the relationship between common interests and trade flows (Hypothesis 1b), this coefficient should be positive and statistically significant.

Hypothesis 2 addresses whether *Democratic Dyads* have higher trade flows than other dyads. We consider as democratic those states that score 6 or above on Gurr's 11-point scale (0–10) of institutionalized democracy (Gurr, Jaggers, and Moore 1989, 1991). If both members of a dyad are democracies, the dyad is democratic. We code democratic dyads as 2 and nondemocratic dyads as 1 (DEMD). Hypothesis 2 about democratic dyads and trade predicts this coefficient should be positive and significant.

We draw our list of *Alliances* from the Correlates of War list of mutual formal alliances between the states.[11] We use this list both to calculate the tau_b values and as the indicator of when alliances existed for testing hypotheses 4 and 5. Testing this pair of hypotheses requires two independent variables in order to distinguish the hypothesized positive effects of alliances under *Bipolarity* and their lack of effect under *Multipolarity*. We code the existence of an alliance in a dyad-year as 2 and use 1 for the absence of an alliance (ALLIA). Following Waltz (1979), we classify the international system as multipolar before World War II and bipolar afterward. We construct the interactions of multipolarity and bipolarity with the presence of an alliance, calling the variables MULTIAL and BIAL, respectively. MULTIAL is 2 if an alliance exists in a dyad-year before 1947 and 1 otherwise. BIAL is 2 if an alliance exists in a dyad-year in or after 1947 and 1 otherwise. The combination of these two variables allows us to test hypotheses 3a and 3b. They predict that the coefficient for an alliance under multipolarity should not be statistically significant, but the coefficient for an alliance under bipolarity should be significant and positive.

Finally, the functional form of the gravity model is multiplicative, as follows, where ε_{ijt} is an error term, such that $\ln(\varepsilon_{ijt})$ is normally distributed with mean 0:

$$EXP_{ijt} = \alpha(GNP_{it}^{\beta_1})(GNP_{jt}^{\beta_2})(POP_{it}^{\beta_3})(POP_{jt}^{\beta_4})$$
$$(DISTANC_{ij}^{\beta_5})(TAU_{ijt}^{\beta_6})(DEMD_{ijt}^{\beta_7})(MID_{ijt}^{\beta_8})$$
$$(MULTIAL_{ijt}^{\beta_9})(BIAL_{ijt}^{\beta_{10}})\varepsilon_{ijt}$$

State i denotes the exporter, state j the importer, and t the current year. We transform this equation into a linear relationship by taking the natural logarithm of all the variables. We indicate this transformation by adding the letter L to the end of each variable name and estimate the following linear equation:

[8] We use values of 1 and 2, respectively, for the presence and absence of these variables in place of the conventional 0 and 1 values for dummy variables. Adding one to the conventional values for dummy variables ensures that the absence of the condition does not change the value predicted by all other variables in the gravity model, and the presence of the condition multiplies that value by a fixed amount determined by the coefficient of the dummy variable.

[9] For a discussion of the theoretical and empirical differences between tau_b and bilateral alliances as a measure of common interests, see Bueno de Mesquita (1981, 111–8 and especially Figure 4.1).

[10] The minimum value of the tau_b scores in the data set is $-.475$; adding 1 suffices to avoid infinite negative logarithms. A referee suggested manipulating the minimum value of the tau_b values to ensure that the minimum value of TAU equaled 1. Then the logarithm of the minimum value of TAU would be 0. This transformation could prevent any distortion introduced by taking the logarithms of values of TAU close to 0. We have replicated our analyses using this specification of TAU. The results produce the same pattern of coefficients presented and discussed in this paper but with some minor improvements in significance probabilities.

[11] The alliances we use are drawn exclusively from the COW listing (Small and Singer 1969). Gowa and Mansfield (1993) add a bilateral alliance between the United States and Japan to the COW listing for the bipolar era. It is the only bilateral alliance their data contain for this period. The Japanese-American Security Agreement, on which they base their coding, is widely regarded as a protectorate and not an alliance, since it does not carry reciprocal obligations.

$$EXPL_{ijt} = \alpha + \beta_1(GNPL_{it}) + \beta_2(GNPL_{jt}) + \beta_3(POPL_{it}) + \beta_4(POPL_{jt}) + \beta_5(DISTANCL_{ij}) + \beta_6(TAUL_{ijt}) + \beta_7(DEMDL_{ijt}) + \beta_8(MIDL_{ijt}) + \beta_9(MULTIALL_{ijt}) + \beta_{10}(BIALL_{ijt}) + \ln(\varepsilon_{ijt})$$

ESTIMATION STRATEGY

Time-series cross-sectional analysis presents a variety of statistical problems for estimation. Ordinary least squares (OLS) estimates of the coefficients are unbiased but may not make the most efficient use of the information in the data. The error terms cannot be expected to have the same variance across the cross-section and be independent over the time series and across the cross-section. That is, the error terms may be heteroskedastic, autocorrelated, and contemporaneously correlated. The primary effect of such an error term is that the estimates of the standard errors of the estimated coefficients are inaccurate.

The common response to this problem has been to use a variety of generalized least squares techniques (GLS) to render the error term spherical (Parks 1967; Stimson 1985). Such techniques use residuals of OLS to estimate the error structure and transform the data, using the estimated error structure to run GLS. Asymptotically, these procedures produce efficient, unbiased estimates of the coefficients and their standard errors.

Recent research shows, however, that the cure may be worse than the affliction (Beck and Katz 1995). GLS techniques are efficient only if we know the true error structure. Since we do not know that structure generally, it is usually estimated from the data. These estimates can rely on small effective sample sizes and so be inaccurate in small samples. For example, the effective sample size for estimates of the contemporaneous correlations is about $2T/N$, because $N(N-1)/2$ correlations are estimated using NT observations. When the length of the time series is close to the number of cross-sectional units, as is the case in our analysis, each individual correlation is highly dependent on the data. Because the effective sample size is small, one or a few individual cases may be highly influential. The estimated standard errors of the coefficients are likely to be smaller than the actual variance of the coefficients, and consequently statistical tests using these inaccurate standard errors are likely to lead to overconfidence in the statistical significance of our estimates.

Beck and Katz (1995) suggest a solution to this problem: Use OLS to estimate the coefficients but correct the estimated standard errors using contemporaneous correlations estimated from the residuals. They call these estimated standard errors panel-corrected standard errors (PCSEs). Their Monte Carlo analysis demonstrates that PCSEs are more accurate estimates of the standard errors of the coefficients than those recommended by Parks (1967). They also recommend that any correction for serial correlation be common across the cross-sections, rather than estimating a separate serial correlation for each cross-section. We implement Beck and Katz's technique by estimating and removing a common serial correlation, estimating the coefficients using OLS on the transformed data, and using the residuals of this analysis to estimate the PCSEs.[12]

This technique is an improvement over the estimation procedures used by earlier studies. Both Pollins (1989a, 1989b) and Dixon and Moon (1993) estimate different autocorrelations for each cross-section. Such procedures create the possibility of incorrect standard errors and misleading statistical tests. Of course, Beck and Katz's research had not been done when Pollins and Dixon and Moon did their estimations. In addition, Gowa and Mansfield's (1993) cross-sectional design does not take advantage of the time-series nature of the data. Furthermore, it is difficult to interpret the range of time-specific coefficients Gowa and Mansfield report.

Our data suffer from two problems. First, as noted earlier, we are missing economic or demographic data for some states in a few years, and we delete these observations. Second, the two world wars create two interruptions in our time series, one from 1914–19 and the other from 1939–46. Both the missing data and the wars create interruptions in the time series. We deal with these interruptions when we remove the serial correlation. We use the Prais-Winston transformation, which treats the first term of a time series differently from the following observations (Prais and Winston 1954). We consider each observation that does not have an observation from the prior year as the beginning of a new time series. For example, all observations for 1920 receive the Prais-Winston treatment as the first term in a time series.[13]

STATISTICAL RESULTS

Our primary specification tests the three arguments about the effects of international politics on trade flows. All the hypotheses are tested in this specification. It includes the following exogenous variables, labeled by the corresponding hypothesis: (1a) whether a dispute occurred between the members of the dyad in that year, (1b) the tau_b measure of alliance similarity between the two members of the dyad, (2) whether the dyad was a pair of democracies, (3a) whether an alliance existed between the members of that dyad, and (3b) the interaction of whether an alliance existed with whether the system was bipolar. The coefficient for the tau_b value tests the hypotheses that the similarity of international political relations increases trade. A pos-

[12] PCSEs and all statistical estimations were calculated by a GAUSS program written by James Morrow. The program, which is designed to analyze this data set rather than be a general implementation of the Beck-Katz procedure, can be obtained from Morrow.

[13] The first observation after an interruption in the series is transformed to $y_t = x_t(1-\rho^2)^{1/2}$, where x is the original variable, y the transformed variable, and ρ the estimated serial correlation. The following observations are transformed to $y_t = x_t - \rho x_{t-1}$.

TABLE 1. Effects of Political Variables on Trade Flows

	Estimated Coefficient (Standard Error)
Independent Variables	
Militarized interstate dispute (MIDL)	.016 (.090)
Similarity of relations (TAUL)	1.448*** (.365)
Democratic dyad (DEMDL)	1.221*** (.131)
Alliance under multipolarity (MULTIALL)	−.180 (.157)
Alliance under bipolarity (BIALL)	−.497[a] (.160)
Control Variables	
GNP of the exporter (GNPIL)	.557*** (.076)
Population of the exporter (POPIL)	−.184 (.176)
GNP of the importer (GNPJL)	.308*** (.044)
Population of the importer (POPJL)	−.693*** (.058)
Distance between nations (DISTANCL)	−.277*** (.025)
R^2 = .78.	

Note: ρ = .89 (used in correction for serially correlated errors). ***p < .001.
[a]Indicates significant but incorrectly signed.

itive coefficient supports this hypothesis. A negative coefficient for the occurrence of a dispute supports the hypothesis that conflict lowers trade. The coefficient on whether the dyad is democratic tests the hypotheses that within a pair of democracies trade should be higher than within other pairs of nations. A positive coefficient supports this hypothesis. The pair of alliance variables interacted with the polarity of the system test the hypotheses that alliances under bipolarity increase trade, while alliances under multipolarity have no effect. This argument is supported if the coefficient for alliance is not significant, and the coefficient for the interaction of bipolarity and alliance is positive.

Table 1 presents the results of our primary specification. These results support part of the first argument, all of the second, but the third not at all. Since the coefficient reporting the estimated effect of a militarized interstate dispute on trade is not statistically significant, Hypothesis 1a is not supported. Both democratic dyads and similarity of political relations (i.e., high tau_b) increase trade flows, as hypotheses 1b and 2 assert. An alliance, however, increases trade under multipolarity and decreases it under bipolarity, the exact opposite of hypotheses 3a and 3b. Furthermore, all the control variables of the gravity model have the expected signs, and all but the population of the exporting state are statistically significant.

Because our exogenous variables are correlated and colinearity might lead us to confuse the effect of one variable with another,[14] we have run a variety of specifications to test the robustness of the results beyond those given in Table 1. These analyses are presented in Table 2. In each alternative model, we drop one or more of the independent variables to check the stability of the estimated coefficients. In several runs, we investigate the effect of alliances without respect to the polarity of the system; this variable is listed as ALLIAL in Table 2. Our results are robust to these checks. The same clear pattern of coefficients holds across all models: Similarity of relations and democratic dyads always have higher trade flows, while alliances sometimes increase trade and at other times decrease it. Alliances under multipolarity are more favorable to trade than alliances under bipolarity (except for model 10).[15] The pattern of the control variables is also stable to these variations. Multicolinearity, then, does not threaten the inferences about the hypotheses that we draw from our data.[16]

There are two additional threats to the validity of these results: (1) nonspherical errors even after the correction for a common serial correlation and (2) endogeneity in the independent variables. We checked the residuals of our main analysis for evidence of nonspherical errors that might be caused by, for example, heteroskedasticity. The first ten partial autocorrelations of the residuals are all less than .3, and all but one are less than .1. We also calculated the mean residual of each cross-section. A substantial number of these means differing from 0 implies that the cross-sections exhibit heteroskedasticity. Only four of the 42 cross-sectional means of the residuals were statistically discernible from 0 at the .05 level. Because of the clarity of these patterns, we have confidence that

[14] The correlations between these variables are:

	MIDL	TAUL	DEMDL
MIDL			
TAUL	−0.14		
DEMDL	−0.14	0.45	
ALLIAL	−0.06	0.72	0.26

[15] The reader may wonder why we find such different results on the effects of alliances and polarity from those of Gowa and Mansfield (1993). OLS analyses of our data before correcting for serial correlation produce similar results to Gowa and Mansfield's analysis. The dependent variable, logged imports, and the three main independent variables, similarity of political relations, joint democracy, and the presence of an alliance, all have substantial persistent autocorrelation. For instance, the tenth-order autocorrelation between values of flows of imports in a directed dyad in a given year and ten years later—the approximate separation of the individual cross-sections in Gowa and Mansfield (1993)—is .536.

[16] If we adopt the conservative step of treating our effective sample size as one-half of our number of observations, as discussed in footnote 5, we divide all the t-scores by $\sqrt{2}$. Then alliances under multipolarity are significant at the .05 level instead of the .01 in models 4 and 9. Alliances under bipolarity are significant at the .05 level instead of the .01 level in the main model and model 1. Alliances in general are significant at the .01 level instead of the .001 level in models 6 and 8. The statistical significance of all other coefficients does not change. In short, treating our effective sample size as one-half of our number of observations weakens the results only for alliances. All of our inferences about the three arguments remain the same after this conservative procedure.

TABLE 2. Estimates of Alternative Specifications of the Model

	Model 1: Disputes Omitted	Model 2: Disputes, Polarity Omitted	Model 3: Disputes, Democracy Omitted	Model 4: Disputes, Relations Omitted	Model 5: Disputes, Alliances Omitted
Independent Variables					
Militarized interstate dispute (MIDL)					
Similarity of relations (TAUL)	1.398***	1.410***	1.478***		1.126***
	(.379)	(.373)	(.404)		(.211)
Democratic dyad (DEMDL)	1.213***	1.167***		1.251***	1.179***
	(.129)	(.111)		(.123)	(.115)
Alliance under multipolarity (MULTIALL)	−.163		−.346	.457**	
	(.161)		(.201)	(.157)	
Alliance under bipolarity (BIALL)	−.509**		−.290	.029	
	(.163)		(.151)	(.017)	
Alliance (ALLIAL)		−.274			
		(.153)			
Control Variables					
GNP of the exporter (GNPIL)	.567***	.558***	.675***	.565***	.554***
	(.077)	(.074)	(.071)	(.072)	(.074)
Population of the exporter (POPIL)	−.153	−.138	−.401***	−.102	−.123
	(.123)	(.116)	(.115)	(.123)	(.118)
GNP of the importer (GNPJL)	.298***	.290***	.399***	.297***	.286***
	(.046)	(.042)	(.035)	(.048)	(.041)
Population of the importer (POPJL)	−.680***	−.668***	−.909***	−.614***	−.652***
	(.067)	(.065)	(.056)	(.071)	(.058)
Distance between nations (DISTANCL)	−.272***	−.265***	−.258***	−.276***	−.262***
	(.027)	(.021)	(.010)	(.028)	(.022)
N = 2,631					
R^2	.78	.78	.77	.78	.78
ρ	.92	.92	.92	.92	.92

Note: Estimated coefficients are given with standard errors in parentheses underneath. $^{**}p < .01$, $^{***}p < .001$.

further adjustment of the data is unnecessary to render the errors spherical.

Simultaneity bias would arise if trade causes one or more of our exogenous variables. In particular, trade is often believed to mitigate conflict. Several arguments have been advanced on behalf of this, but they all hinge on the presumed beneficial effects of trade between states. Under theories of interdependence (Keohane and Nye 1977; Nye 1971), trade creates a web of beneficial interdependence between states that dampens conflict by raising its costs. Similarly, Polachek (1980) argues that states will attempt to avoid conflicts with their trading partners in order to preserve the gains from trade. To test for the possibility that trade reduces conflict, we ran several different probit analyses that tried to predict the occurrence of conflict using trade flows, distance, and our other political variables. In our first analysis, we simply used militarized interstate disputes as the dependent variable and trade as the independent variable in a logit. In this bivariate analysis the coefficient for trade is −.002 and statistically significant ($p < .001$), indicating that trade did dampen conflict. Yet, when the political variables for similarity of political relations, joint democracy, and the presence of an alliance and distance are added to the logit, the coefficient for trade falls to −.0002 and is not significant, while the coefficients for similarity of political relations and joint democracy are −3.54 and −3.19, respectively, and both are significant ($p <$ .001). The coefficient for the presence of an alliance is significant, but it is positively signed (1.02, $p < .01$). The coefficient for distance is negative and not significant. Similar results were obtained when we analyzed the same specifications and lagged the independent variables by one period. When controls for joint democracy, foreign policy similarity, alliance, and distance are included, trade does not have a statistically significant effect in predicting the occurrence of a militarized dispute in our data set, whether we tested for trade flows in the year of the dispute or the year prior to it. Consequently, we do not believe that simultaneity bias is a significant threat to our analyses.[17]

[17] For a further analysis of this, see Morrow, Siverson, and Tabares (1998). The empirical literature on this relationship is growing. In recent work, Oneal et al. (1996) and Barbieri (1996) have reported somewhat differing results. The former, drawing upon a broad data set of politically relevant states from the post–World War II era, report that conflict is less likely between states when their trade is significant or they are democratic. Barbieri (1996), using data on international trade and conflict between 1870 and 1938, reports results in which trade can both increase and decrease conflict, with peace being most likely when states have symmetrical trade with each other. In addition, using Granger techniques on a time series of trade and conflict for 16 dyads over the period between 1960 and 1992, Reuveny and Kang (1996) report results showing that increased trade can bring either more or less conflict. Plainly, this research question remains open, but insofar as our results are concerned,

TABLE 2. (continued)

Model 6: Democracy, Alliances Only	Model 7: Polarity, Alliances Only	Model 8: Alliances Only	Model 9: Relations Omitted	Model 10: Democracy Omitted	Model 11: Relations Only	Model 12: Democracy Only
			−.0002	.018		
			(.060)	(.070)		
				1.539***	1.124***	
				(.385)	(.224)	
1.195***			1.262***			1.181***
(.106)			(.126)			(.115)
	.305		.471**	−.370		
	(.173)		(.150)	(.193)		
	.286***		.093***	−.262		
	(.031)		(.019)	(.147)		
.326***		.293***				
(.077)		(.071)				
.555***	.678***	.674***	.578***	.691***	.670***	.566***
(.069)	(.066)	(.060)	(.071)	(.070)	(.067)	(.071)
−.084	−.355**	−.346**	−.134	−.448**	−.381***	−.093
(.115)	(.113)	(.096)	(.126)	(.119)	(.107)	(.121)
.289***	.402***	.399***	.310***	.415***	.394***	.296***
(.044)	(.038)	(.029)	(.046)	(.033)	(.023)	(.045)
−.599***	−.847***	−.814***	−.627***	−.936***	−.890***	−.603***
(.067)	(.046)	(.063)	(.059)	(.059)	(.077)	(.070)
−.268***	−.262***	−.261***	−.282***	−.260***	−.255***	−.275***
(.021)	(.010)	(.013)	(.025)	(.010)	(.014)	(.026)
.78	.77	.77	.77	.77	.77	.78
.92	.92	.92	.89	.89	.92	.92

DISCUSSION

Trade patterns between the major powers during the twentieth century broadly support the first two arguments but not the third. Common interests and mutual democratic institutions increase trade flows between states. The effect of alliances on trade flows is uncertain; they may increase or decrease trade. Trade flows are higher between allies in multipolar system than between allies in bipolar systems. These results are robust, even given the interrelated patterns of the variables. Although democracy, common interests, and alliances are often found together in this period, the results do not change when one or more of these variables is dropped from the analysis. Our sample also contains greater variation on these variables than earlier studies, making it easier to separate the effects of different variables.

If politics does influence trade flows, then how large are the effects? Table 3 gives the estimated effect of changes in trade flows for the specified change in the political variables. Because the gravity model is multiplicative, these effects are given as percentage changes.[18] Holding all other political and economic variables constant, a dyad composed of democratic states has more than double the trade of a nondemocratic dyad. Political relations also have large effects on trade flows, but alliances and disputes have only relatively small effects. For the sake of comparison, we have estimated the effects of all the political variables on trade, even those whose effects were not statistically significant in Table 1.

There is another way of looking at the size of these political effects on trade. We tested the statistical significance of our political variables as a set.[19] Com-

simultaneity does not appear to be a problem with respect to the conclusions we draw.

[18] The percentages in Table 3 are calculated from the estimated coefficients in Table 1 using the multiplicative functional form of the gravity model. As noted earlier, we added 1 to DEMD, ALLIA, MID, and TAU to ensure that their logarithms were positive. An example may clarify how the figures in Table 3 were calculated. A democratic dyad multiplies the effect of the other variables by $2^{1.221} = 2.331$, where 1.221 is the estimated coefficient of the effect of a democratic dyad in Table 1, and 2 is the coded value of a democratic dyad. A nondemocratic dyad multiplies the effect of the other variables by $1^{1.221} = 1$. Moving from a nondemocratic dyad to a democratic dyad increases trade by a factor of 1.331 (=2.331 − 1), or 133.1%, assuming all other variables are fixed.

[19] A joint hypothesis test compares a regression with all the political variables included to a regression without the political variables (Hanushek and Jackson 1977, 128). The test statistic is calculated as the difference in the sums of squares divided by the number of political variables over the sum of squares of the equation including the political variables, divided by the degrees of freedom in that equation. The test statistic has an F-distribution with degrees of freedom of the number of political variables and the degrees of freedom of the equation including the political variables.

TABLE 3. Estimated Effects of Political Variables on Trade Flows

Change in Political Variables	Effect of the Change on Trade Flows
From nondemocratic dyad to democratic dyad	Increases trade 133.1%
Occurrence of a militarized dispute	Increases trade 1.1%[a]
Formation of an alliance	
Under multipolarity	Reduces trade 11.7%[a]
Under bipolarity	Reduces trade 29.1%
Change in similarity of relations	
From negative to neutral (TAU from −.5 to 0)	Increases trade 172.8%
From neutral to positive (TAU from 0 to .5)	Increases trade 79.9%
From negative to positive (TAU from −.5 to .5)	Increases trade 390.1%

[a]These estimates are based on coefficients that were not statistically significant at the .05 level. We include the estimated effect to illustrate the estimated relative effect of these changes in the political variables compared to the changes that had statistically significant effects.

pared with a model of only the economic control variables, the complete set of political variables is significant at the .001 level. If we restrict our attention just to joint democracy and similarity of relations, the two most successful variables in our test, then the political variables add to the economic model at significance levels of .05 or less.[20] Our political variables do account for something the economic model alone misses.

Our evidence challenges neorealist arguments. When we do not control for joint democracy and relations between states, alliances have a positive effect on trade that is statistically discernible. Our evidence clearly shows, however, that domestic institutions and relations have a much larger effect on trade flows than do alliances. Furthermore, trade flows are higher between allies under multipolarity than under bipolarity. According to the neorealist position, the opposite should be true, because neorealists contend that the risk of exit from an alliance is less in bipolarity than multipolarity. The complete neorealist position on trade and alliances—namely, that alliances increase trade between the members and are more reliable under bipolarity than multipolarity—is incompatible with our evidence.[21]

Our evidence does not support the position that security concerns lead states to control their trade flows. Trade flows during the Cold War were primarily within, rather than across, the Eastern and Western blocs. But each of the three arguments—political relations, joint democracy, and security concerns—predict this pattern. Our evidence suggests that common interests and political institutions fostered trade within each bloc. To be sure, some goods were banned for trade between the blocs because of their strategic importance, such as computers (Morrow 1997), but the argument that security concerns affect trade flows conflates the need to monitor trade over *certain goods* into a belief that states must watch *all trade flows*.

Our evidence indicates that other factors are more important than alliances. Prime among these are joint democracy and the similarity of policy interests between the states. The positive effects of joint democracy are in accord with the theoretical position of Pollins. He argues that private economic agents (i.e., individuals and firms) are the central actors; for Gowa the state is the central actor. Liberal democratic states give private economic agents the greatest scope to act on their judgments about what business they should pursue. Those active in foreign commerce are likely to be particularly interested in the various risks attached to trade. One of these risks is that the other party may violate a contract. The risk posed by such violations is very different when the trading partner is a firm in a state with a legal system that recognizes contracts than when the trading partner is a government firm in a state that may not recognize the contract or interprets the contract in accord with the immediate interest of the state rather than a body of law. These are the risks we outlined above, and they seem to make a difference.

Politics affects trade flows because economic actors care about political risks. International conflict disrupts trade, introducing risk. The institutions of limited government reassure actors, reducing risk. Democracy by itself is not a panacea for increasing trade. But modern democracies generally have been limited governments. Democracies are also less likely to engage in violent conflict with one another (Bremer 1992; Maoz and Abdolali 1989). Both of these effects raise trade between democracies. Furthermore, economic actors anticipate these political effects. The anticipation of conflict prevents trade from growing more than the realization of conflict leads to its disruption.

Finally, we end on a note of caution about our results. We have found evidence about the effect of politics on international trade at the level of overall trade flows. We use a broader sample of data with greater variation and more appropriate methods for that data than do existing studies. Further support for these arguments must be found at lower levels of analysis than aggregate trade flows. If limited government does encourage trade, then there should be direct evidence at lower levels, such as the individual trading concern. If economic agents fear the political disruption of trade, then that risk should be reflected in which international trading businesses are established. If security concerns lead governments to manage their aggregate trade flows, then the mechanisms of such management must exist.

[20] The F-scores for each comparison are $F^5_{2621} = 5.67$ for the complete model and $F^2_{2624} = 88.24$ for just similarity of relations and joint democracy.

[21] For further evidence on the lack of an alliance effect on trade in the post–World War II era, see Smith and Eyerman (1996) and Bliss and Russett (1998).

APPENDIX: MEASUREMENT

Exchange rates for 1907 to 1920 for France, Germany, Italy, Japan, and Great Britain were taken from Bidwell (1970) and Liesner (1989); for 1921 to 1929, rates used were from the U.S. Census Bureau (1928, 1931); and from 1930 to 1990, rates were taken from the United Nations (1955, 1966, 1994). Constant dollar values were obtained using the consumer price index (CPI) provided in the U.S. Census Bureau (1947, 1962, 1995) and derivations thereof.

Data on GNP from 1907 to 1965 were taken from Mitchell (1980, 1982, 1983, 1992) and Clarke and Matko (1983). For the period from 1907 to 1922, reliable data on Soviet GNP are difficult to obtain. In this case, estimates were made from data in Bairoch (1976). GNP from 1966 to 1990 were taken from the International Monetary Fund (1995) for all countries except the Soviet Union. Soviet GNP from 1966 to 1988 was taken from Mitchell (1992). Population data for 1907 to 1965 were taken primarily from Liesner (1989). Population figures for France for all years were taken from Maddison (1991), but these were also supplemented. Data for Italy from 1909 to 1950 were from Istituto Centrale di Statistica (1958); for the Soviet Union, from 1907 to 1913, data were from Mitchell (1992), from 1920 to 1939 from Eason (1963), and from 1950 to 1965 population was from Clarke and Matko (1983). Population from 1966 to 1990 was taken from the International Monetary Fund (1995) for all countries except the Soviet Union. Soviet population from 1966 to 1988 was taken from Mitchell (1992); Soviet population figures for 1989 and 1990 are from Karasik (1992). Geographical distance is the shortest distance between capital cities. Prior to 1949, Berlin is used as Germany's capital. Data on geographical distances are from Fitzpatrick and Modlin (1986).

The primary source of export data for 1907 to 1965 was Mitchell (1980, 1982, 1983). Exceptions to this follow. France's exports to Japan and the Soviet Union for all years were taken from Bureau de la Statistique Générale (1900–11, 1932, 1966). Germany's exports to Japan from 1907 to 1954 were calculated from reported Japanese imports from Germany; for 1955 to 1965 data were taken from the United Nations (1959, 1961, 1966). Italy's exports to Japan and the Soviet Union for all years were taken from Istituto Centrale di Statistica (1912, 1914, 1922–25, 1929, 1933, 1936, 1939, 1941, 1952, 1959, 1962, 1966). The Soviet Union's exports to France, Germany, Italy, Great Britain, and the United States from 1909 to 1913 were taken from Miller (1967); exports to Italy, Japan, and the United States from 1913 to 1965 were taken from Clarke and Matko (1983). Great Britain's exports to Italy and Japan for all years came from Great Britain Central Statistical Office (1914, 1928, 1938, 1953, 1961, 1967). U.S. exports to Italy and the Soviet Union for all years were taken from the U.S. Census Bureau (1912, 1919, 1921, 1928, 1931, 1933, 1938, 1940, 1941, 1947). Japan's exports to Italy and the Soviet Union for all years were taken from the Japanese Department of Finance (1916, 1936, 1940). Export data from 1966 to 1990 were taken from the International Monetary Fund (1971, 1978, 1985, 1992).

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Administrative Procedures and Political Control of the Bureaucracy

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Positive theorists have argued that administrative procedures enhance political control of the bureaucracy, in part by predisposing agencies toward policy choices preferred by legislators' favored constituents. Although this "deck-stacking" argument has been both influential and controversial, few scholars have subjected it to empirical examination. This article assesses the operation of a prominent administrative procedure—the notice and comment process—in the context of Medicare physician payment reform, a fundamental restructuring of the way in which the Medicare program pays for physician services. I find, contrary to the deck-stacking thesis, that the Health Care Financing Administration (HCFA) was more responsive to physicians expecting reductions in fees than to the intended beneficiaries of the new payment system. Although these results do not necessarily imply that Congress exerted little influence over HCFA decision making, they suggest that certain administrative procedures do not operate as instruments of political control.

Delegation of policymaking authority from elected officials to unelected bureaucrats is a fact of contemporary American politics. It does not, however, necessarily imply abdication, as officeholders possess instruments that potentially limit bureaucratic discretion. Presidents, for example, fill many high-level agency posts with appointees whose preferences closely resemble those of the chief executive. Members of Congress regularly conduct oversight hearings, which are proceedings designed to monitor and, if necessary, redirect agency activity. The presence of these instruments raises a question of considerable importance for the operation of American democracy: To what extent do different types of instruments facilitate political control of the bureaucracy?

In recent years, numerous studies have addressed this question (e.g., Eisner and Meier 1990; Moe 1985; Wood 1988, 1990; Wood and Waterman 1991, 1993). These studies examine the effect of different instruments on decision making in several public bureaucracies, including the National Labor Relations Board, Equal Employment Opportunity Commission (EEOC), and Environmental Protection Agency (EPA). One finding common to most of this research is that patterns of bureaucratic output frequently change in response to personnel changes at the top of agencies. For example, the litigation activities of EEOC declined substantially after President Reagan appointed an ideological conservative to head the Office of General Counsel (Wood 1990). These studies also indicate that manipulation of agency budgets and close congressional scrutiny can, in certain contexts, enhance political control. These results lead Wood and Waterman (1981, 821) to conclude that "at certain times in the cycle of U.S. politics, responsiveness permeates the U.S. bureaucracy."[1]

The instruments at politicians' disposal are not limited to those examined in these studies. For example, members of Congress can establish administrative procedures (e.g., the public notification and participation requirements codified in the Administrative Procedure Act [APA] of 1946) that impose constraints on bureaucratic decision making. Thus far, scholarly attention to administrative procedures has been quite uneven. On the one hand, positive theorists have argued that administrative procedures are indispensable instruments of political control. On the other hand, students of legislative-executive relations have conducted few empirical tests of propositions derived from this argument. As a result, the extent to which administrative procedures actually limit bureaucratic discretion remains an open question.

The central aim of this article is to take a step toward answering this question. It examines the operation of a prominent administrative procedure—the notice and comment process—in the context of Medicare physician payment reform, a fundamental restructuring of the way in which the Medicare program pays for physician services. More specifically, the article addresses the effect of the notice and comment process on decision making at the Health Care Financing Administration (HCFA), the agency charged with establishing fees for thousands of services covered by the program. The following two sections review the positive theory of administrative procedures and discuss the politics of Medicare physician payments, respectively. The remaining sections present and highlight the implications of the analysis of HCFA decision making,

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I presented a previous version of this article at the 1996 annual meeting of the Midwest Political Science Association, Chicago. I thank John Aldrich, Bill Bianco, John Brehm, Peter Feaver, Joel Kaji, Roger Noll, Chris Snyder, Dave Spence, Rob Sprinkle, Margie Teeters, Don Thompson, Paul Wahlbeck, Jack Wright, Langche Zeng, seminar participants at The College of William and Mary, Duke University, The George Washington University, North Carolina State University, and The Ohio State University, anonymous reviewers, and especially Jay Hamilton for advice and assistance. The National Science Foundation provided research support.

[1] Wood (1988) cautions against stating this conclusion too strongly, as agencies might retain considerable discretion despite the imposition of external controls. Eisner and Meier (1990), for example, find that changes internal to the Department of Justice were more important in explaining changes in antitrust policy than were political events, such as the Reagan revolution. In addition, Brehm and Gates (1996) demonstrate the importance of factors such as organizational culture and professional norms in explaining bureaucratic behavior.

which demonstrates that the notice and comment process did not facilitate congressional control of Medicare physician payment reform.

THE POSITIVE THEORY OF ADMINISTRATIVE PROCEDURES

Three decades ago, Harris (1964, 9) distinguished between two approaches to political control of the bureaucracy: "legislative decision or activities prior to the relevant administrative action . . . [and] inquiries about policies that are or have been in effect." Since then, much of the theoretical and empirical work on political control has focused on the latter approach.[2] McCubbins and Schwartz (1984), for instance, argue that legislators establish rules and procedures enabling interest groups and private citizens to examine agency actions and to alert Congress about noncompliant bureaucratic behavior. Weingast and Moran (1983) provide evidence that penalties imposed on the Federal Trade Commission in the late 1970s were the result of changes in the composition of relevant oversight committees, not a decade of agency intransigence.

Despite these arguments and findings, scholars have asserted that ex post oversight is limited in its ability to enhance political control. Legislators may not be able or willing to sanction recalcitrant agencies. Drafting new legislation and reducing agency budgets, two often-mentioned sanctions, require significant investments of congressional resources, resources that legislators may not possess or may prefer to devote to more electorally salient activities (Fiorina 1989; Moe 1987). Furthermore, these forms of oversight may be of limited utility in redirecting agencies. A coalition within Congress may prefer the policy choices made by bureaucrats to those intended by the enacting coalition (McCubbins, Noll, and Weingast 1989). In addition to this problem of "bureaucratic drift," congressional preferences may change over time, and new coalitions may tamper with carefully negotiated bargains (Horn and Shepsle 1989).

With these shortcomings in mind, McCubbins, Noll, and Weingast (1987, 1989) argue that ex ante administrative procedures are Congress's most effective instruments of political control.[3] Administrative procedures increase bureaucratic accountability in three principal ways. First, administrative procedures enfranchise particular constituents: "Interests that are active participants in the debate over the original legislation should be given representation through the structure and process of the agency so that each will be protected against undesirable policy shift" (McNollgast 1989, 444). Second, administrative procedures increase the likelihood that bureaucrats incorporate the preferences of legislators' favored constituents into agency policies:

> Through these decisions, political actors assure that the influence accorded to different constituents is not random; indeed, by controlling the details of procedures and participation, *political actors stack the deck in favor of constituents who are the intended beneficiaries of the bargain struck by the coalition which created the agency* (McNollgast 1987, 261).

Third, administrative procedures protect the interests of favored constituents over time; as their preferences change, bureaucratic decision making and policy outcomes change as well.

McNollgast's contribution to the study of political control is both important and controversial, as evidenced by the fact that scholars have extended, refined, and critiqued their "deck-stacking" thesis. Macey (1992) emphasizes that legislators can use agency structure, as well as process, to enhance bureaucratic responsiveness. The most straightforward example is the decision to create a single-industry agency; this structure increases the likelihood that agency decisions reflect the preferences of constituents to whom the agency is charged to deliver policy benefits. Formal models have refined the deck-stacking thesis by pointing out the limits of Congress's willingness to circumscribe agency autonomy (Bawn 1995; Epstein and O'Halloran 1994). Although stringent administrative procedures facilitate political control, they also hamper agency flexibility and stifle the provision of potentially valuable information. Legal scholars object to McNollgast's portrayal of administrative procedures as instruments of political control, maintaining that their primary function is to promote regulatory outcomes embodying principles "ranging from 'fairness' to 'efficiency' and utilizing a host of other ideas as well—'openness,' 'accountability,' 'legitimacy,' and 'rationality,' to name a few" (Mashaw 1990, 267; see also Robinson 1990). Finally, the reaction of students of public administration has been decidedly lukewarm. West (1995), for instance, argues that the principal-agent model on which the deck-stacking thesis rests is in some respects fatally flawed and promotes unnecessary oversimplification of a complex political process.

Despite this attention, few scholars have subjected hypotheses drawn from the deck-stacking thesis to empirical examination. A pair of analyses (Hamilton 1996; Hamilton and Schroeder 1994) demonstrate that although legislators established procedures designed to facilitate political control over implementation of the Resource Conservation and Recovery Act (RCRA), EPA retained significant policymaking discretion. Regulators promulgated certain RCRA rules through informal channels, such as interagency memoranda and guidance manuals, thereby circumventing formal procedural requirements. Furthermore, regulators' decisions to pursue informal rulemaking were strategic; the penalties associated with informal rules were higher than those associated with rules promulgated under the notice and comment process, an indication that EPA officials used informality to impose sanctions more

[2] I focus here on positive-theoretic approaches to the study of political control, given that this article assesses the veracity of the positive theory of administrative procedures. Scholars from other intellectual traditions have examined ex post oversight as well (e.g., Aberbach 1990; Dodd and Schott 1979; Ogul 1976; Rourke 1984; Wilson 1989).

[3] Throughout the article, I refer to McCubbins, Noll, and Weingast as "McNollgast."

stringent than those preferred by members of Congress and the interests likely to participate in formal proceedings.

In this article, I address a different facet of bureaucratic response to administrative procedures—the effect on agency decision making of comments submitted in response to proposed rules. According to McNollgast (1987, 258), the notice and comment process facilitates political control in the following manner: "the entire sequence of decisionmaking—notice, comment, deliberation, collection of evidence, and construction of a record in favor of a chosen action—afford[s] numerous opportunities for political principals to respond when an agency seeks to move in a direction that officials do not like." As a result, agencies have an incentive to take actions that are consistent with the preferences of legislators' favored constituents, by, for example, responding to comments submitted by these constituents to a greater extent than to comments submitted by constituents who are not the intended beneficiaries of congressional action. With this in mind, the analysis examines whether HCFA responsiveness varied across constituents expected to "win" and "lose" under Medicare physician payment reform.

POLITICS AND PHYSICIAN PAYMENTS

The politics of physician payments have long been contentious (Marmor 1992; Smith 1992; Starr 1982). Payment issues played a major role in the passage of Medicare, as the American Medical Association (AMA) made its support contingent on a doctor-friendly reimbursement mechanism. The resulting "customary, prevailing, and reasonable" (CPR) system preserved physician autonomy by allowing doctors to set their fees and to bill the program accordingly. The system also produced a pair of negative effects. First, since physicians had an incentive to raise their fees continually, expenditures increased more rapidly than expected. From 1978 to 1987, for instance, payments to doctors grew at an annual rate of 16%, a rate unmatched in other sectors of the health economy (Kay 1990). Second, the CPR system produced an inequitable distribution of physician payments. Specialists such as cardiac surgeons had no incentive to reduce their fees when the costs of providing such services as open heart surgery diminished. Consequently, Medicare reimbursed these types of specialists more generously (relative to costs) than primary care practitioners (Jencks and Dobson 1985; U.S. Department of Health and Human Services 1989).

Elected officials continually took steps to slow the rate of growth in Medicare physician expenditures. President Nixon championed legislation that provided health care providers and beneficiaries with incentives to forsake traditional fee-for-service medicine in favor of prepaid group practices, such as health maintenance organizations. Congress passed an array of legislation, ranging from the creation of peer review organizations (panels of physicians that evaluate the practice patterns of fellow doctors) to the imposition of a fifteen-month freeze on increases in physician fees.

Despite these piecemeal reforms, expenditures continued to rise at an alarmingly rapid rate, prompting Democrats and Republicans alike to press for fundamental restructuring in Medicare financing. In this regard, the Omnibus Budget Reconciliation Act (OBRA) of 1989 was landmark legislation, as it contained provisions mandating the development of a Medicare fee schedule. Under the fee schedule, payments for physician services were to be established in advance and to reflect physicians' resource costs (i.e., the amount of work required to produce services, as well as practice and malpractice expenditures).[4] The central objective of the fee schedule and related provisions was to limit the ability of doctors to set prices and, in the process, facilitate cost containment and cross-specialty redistribution of Medicare resources (U.S. Congress 1991; U.S. Department of Health and Human Services 1989).

Members of Congress delegated the task of establishing fees to HCFA, the agency that administers the Medicare program. This delegation placed enormous policymaking power in the hands of HCFA officials, as they possessed authority to set reimbursement rates for thousands of physician services covered by the program. Legislators sought to manage this delegation in a variety of ways. They created the Physician Payment Review Commission (PPRC), a congressional agency composed of prominent practitioners, academics, and health industry personnel. PPRC advised Congress on physician payment issues and monitored the development of the fee schedule. In addition, several health-oriented subcommittees conducted hearings on the fee schedule, both before and after passage of the enacting legislation (e.g., U.S. Congress 1988, 1991). These hearings assembled representatives from HCFA, organized medicine, and other interested parties and addressed a host of issues relevant to the setting of physician fees. Finally, and most important in the context of this article, legislators required HCFA to report payment levels in the *Federal Register*, in part to provide the physician community with opportunities to monitor and comment on the development of the fee schedule. With this mandate in mind, the agency published three notices: (1) a Model Fee Schedule on September 4, 1990, (2) a Proposed Rule on June 5, 1991, and (3) a Final Rule on November 25, 1991.[5]

[4] A resource-based fee schedule was one of several options weighed during the 1980s. Other options included a charge-based fee schedule, whereby payments would be determined prospectively on the basis of physicians' past charges, and a capitated system, whereby physicians would receive lump-sum payments to provide Medicare beneficiaries with all necessary medical care during a specified period. See Smith (1992, 136–58) for a discussion of why legislators ultimately pursued a fee schedule based on resource costs.

[5] HCFA derived the payment levels reported in these notices from information provided by a variety of governmental and nongovernmental sources, most notably Harvard University's School of Public Health (U.S. Department of Health and Human Services 1990, 1991a, 1991b). Harvard researchers produced three reports under a cooperative agreement with HCFA. The first report (from which the

TABLE 1. Overview of Physician Participation in Medicare Fee Schedule Development

By Type and Period	Period of Comment		
Type of Commenter	Model Fee Schedule	Proposed Rule	Total
Professional association	89	158	247
Individual practice	201	~95,000	~95,201
Total	290	~95,158	~95,448
By Specialty and Period			
Specialty			
Radiology	36	788	824
Anesthesiology	3	555	558
Internal Medicine	12	412	424
Family Practice	13	342	355
Neurology	3	319	322
Ophthalmology	24	258	282
Urology	6	171	177
Orthopaedic Surgery	8	164	172
Dermatology	4	136	140
General Surgery	15	111	126
Cardiology	8	112	120
General Practice	1	94	95
Obstetrics/Gynecology	2	80	82
Thoracic Surgery	4	74	78
Neurosurgery	5	53	58
Otolaryngology	5	52	57
Pulmonary Disease	6	47	53
Nuclear Medicine	0	14	14
Podiatry	1	2	3

Note: The numbers in the cells are counts of comments. I examined 5,471 of the approximately 95,000 comments submitted by individual practices in response to the Proposed Rule. HCFA stores these comments in numerous boxes. I randomly selected several boxes and collected information on all individual practice comments contained in these boxes. The 19 specialties listed here are a sample of prominent specialties affected by physician payment reform, selected to demonstrate cross-specialty variation in participation.

In accordance with APA requirements, HCFA solicited reactions from parties interested in Medicare payment policy. Given their large financial stake, physicians participated extensively during the sixty-day comment periods following publication of the Model Fee Schedule and Proposed Rule. Table 1 provides an overview of this participation. Physicians submitted 290 comments in response to the Model Fee Schedule. Professional associations such as the American Academy of Family Practitioners and American College of Surgeons accounted for 89 of these comments, with individual practices submitting the remainder. Physician participation increased dramatically after publication of the Proposed Rule. Professional associations nearly doubled their output, submitting 158 comments, while individual practices inundated the agency with approximately 95,000 comments. Participation also varied widely across specialties. Specialties such as radiology and internal medicine commented extensively, particularly in response to the Proposed Rule, there was relatively little participation by such specialties as thoracic surgery and pulmonary disease during both public comment periods.

With these patterns in mind, the analysis addresses two central questions: Did physician comments exert influence over the setting of Medicare fees? More specifically, did participation on the part of physicians expected to "win" under the fee schedule (e.g., primary care practitioners) affect HCFA decision making to a greater extent than participation by "losing" physicians (e.g., surgeons and other specialists)? The second question is particularly important in light of McNollgast's argument that administrative procedures serve the interests of legislators' favored constituents. Given that a primary goal of the fee schedule was to redistribute Medicare resources across specialties, a finding that HCFA officials were more responsive to primary care practitioners than to surgeons and other specialists would support the deck-stacking thesis. In contrast, a null finding would indicate that the notice and comment process did not operate as an instrument of political control in the manner posited by McNollgast.

VARIABLES AND METHODS

Dependent Variables

I assess the effect of physician participation by examining the extent to which comments prompted HCFA to change payment levels across *Federal Register* notices. The dependent variables are measures of change in specialty-level Medicare revenues. As Table 2 indicates, specialties fared quite differently during the rulemaking process. Cardiology and radiology, for example, experienced sizable revenue increases from the Proposed to Final Rule. In contrast, podiatry revenues decreased substantially across these notices.[6]

Table 2 presents an opportunity to discuss two important facets of the politics of Medicare reform. First, revenue changes were almost exclusively negative from the Model Fee Schedule to Proposed Rule and almost entirely positive from the Proposed to Final Rule. HCFA argued, on the basis of studies conducted by numerous researchers, that physicians performing services targeted for payment decreases would protect their Medicare revenues by increasing their volume of services (U.S. Department of Health and Human Services 1991a). The agency further argued that it had to incorporate this "behavioral offset" into its calculations in order to comply with a statutory mandate that the new payment system be

Model Fee Schedule was derived) presented extremely tentative values for about 1,000 services that accounted for approximately two-thirds of Medicare's physician expenditures. The second report (which influenced the development of the Proposed Rule) refined existing estimates and included preliminary values for about 3,000 additional services. The third report provided the values necessary to develop the Final Rule, which published binding levels for the full set of approximately 8,000 services.

[6] The unit of analysis is the physician specialty because comments typically addressed specialty-level, as opposed to procedure-level, issues. In addition, HCFA organized its payment-setting activities around specialties. Certain agency personnel, for example, were responsible for determining obstetrics/gynecology payments, while others focused on neurosurgery procedures.

TABLE 2. Changes in Specialty Revenues across the Model Fee Schedule, Proposed Rule, and Final Rule

	Change in Revenues ($1,000)	
Specialty	Model Fee Schedule to Proposed Rule	Proposed Rule to Final Rule
Cardiology	−8,990	477,580
Radiology	359,868	449,789
Ophthalmology	−657,770	258,972
Orthopaedic Surgery	−177,547	177,619
Internal Medicine	−75,191	146,650
General Surgery	−189,787	125,040
Dermatology	−16,651	103,511
Urology	−117,359	100,785
Family Practice	−32,704	47,575
Thoracic Surgery	−80,339	45,035
General Practice	−22,856	39,743
Obstetrics/Gynecology	−17,520	33,378
Anesthesiology	−25,718	28,071
Neurosurgery	−17,887	25,039
Otolaryngology	−16,140	24,769
Pulmonary Disease	−15,185	14,923
Neurology	735	10,225
Nuclear Medicine	5,171	4,506
Podiatry	−13,662	−24,439

Note: I calculated these changes in the following manner. I identified the set of procedures included in both the Model Fee Schedule and Proposed Rule (817), as well as those included in both the Proposed and Final Rules (3,377). For each specialty and *Federal Register* notice, I multiplied the reported payment for each of these procedures by the number of times that practitioners performed the procedure during 1991 and summed this product across procedures. These dollar amounts approximate the revenues that specialties would have derived from the Medicare program under each notice. Finally, I calculated differences in revenues across the Model Fee Schedule, Proposed Rule, and Final Rule.

budget neutral in its first year. As a result, HCFA imposed a substantial reduction in payments for procedures reported in the Proposed Rule (i.e., each payment was reduced by an equivalent percentage). This reduction explains why almost all specialties experienced negative revenue changes from the Model Fee Schedule to Proposed Rule.

The physician community objected vehemently to the behavioral offset. Specialties expected to fare poorly under the fee schedule argued that their payments were subject to two sets of reductions, one bringing payments more into line with resource costs and a second resulting from the offset. Winning specialties opposed the behavioral offset on the grounds that it erased a significant portion of their expected revenue gains (U.S. Congress 1991). Congress ultimately sided with the medical community and ordered HCFA to eliminate the behavioral offset almost completely from its calculations. As a result, payments for all procedures increased by an equivalent percentage from the Proposed to Final Rule. Although the inclusion and subsequent exclusion of the behavioral offset affected Medicare physician expenditures at the aggregate level, they did not affect the extent to which the fee schedule redistributed resources across specialties.

The second feature of Table 2 that deserves mention is the fact that some winning specialties (e.g., family practice) experienced smaller revenue increases across the Proposed and Final Rules than certain expected losers (e.g., general surgery). Table 2 does not provide information about net wins and losses under the fee schedule. Rather, it tracks revenue changes across three stages of the rulemaking process. It is possible that although surgical and other specialties fared relatively well in this process, they nonetheless experienced reductions in Medicare payments under the fee schedule (e.g., the Model Fee Schedule may have reduced their payments by a large amount relative to the status quo, and, despite their success in the notice and comment process, their payments under the fee schedule were lower than under the status quo) and vice versa for primary care specialties. Information about the net effect of the fee schedule is not necessary in the context of this article, given its focus on the effect of the notice and comment process on agency decision making.

Explanatory Variables

To examine the efficacy of specialty participation, it is necessary to measure participation in the rulemaking process. I have chosen two measures, counts of comments submitted by professional associations (*Professional Association Comments*) and individual practices (*Individual Practice Comments*) in response to HCFA's preliminary regulations.[7] Comments typically consisted of arguments and evidence designed to persuade the agency to increase specialty payments. As a result, a reasonable expectation is that participation exerted positive influence over revenue changes. This expectation, however, is tempered by previous empirical work. Magat, Krupnick, and Harrington (1986) find that, in general, industry participation did not influence EPA's setting of effluent standards under federal water pollution legislation.[8] The lone exception occurred in the first of two comment periods, during which industry participation prompted EPA to relax proposed standards for a particular class of effluents. Magat, Krupnick, and Harrington (1986, 157) suggest that these results are consistent with the following account of participation:

[7] The distinction between professional association and individual practice comments is blurred by the fact that practices often participated at the behest of associations. Many practice comments, for instance, were form letters that associations distributed to their members. Nevertheless, HCFA processed association and practice comments differently. It assigned a unique identification number to each association comment and stored them separately from practice comments. It is therefore appropriate to treat association and practice comments as distinct types of participation.

[8] A major difference between my analysis and that of Magat, Krupnick, and Harrington (1986) is that I use information about actual comments, while they rely (of necessity) on EPA's published responses to categories of comments. Their null findings with respect to the efficacy of participation may reflect measurement error rather than EPA nonresponsiveness.

> Comments in the . . . [initial] . . : period consisted of technical discussions and proposals that directly addressed the problem of drafting reasonable regulations, whereas in the period from proposal to final promulgation the commenters appeared to be focusing on legal questions in preparation for eventual litigation.

A finding that comments submitted between the Model Fee Schedule and Proposed Rule, but not the Proposed and Final Rule, influenced HCFA decision making would support the argument that early participation is more substantive and, hence, effective than that occurring later in the rulemaking process.

Given my focus on the deck-stacking thesis, it is essential to measure the extent to which specialties were the intended beneficiaries of the new payment system. Studies conducted prior to passage of OBRA by a variety of parties (e.g., AMA, HCFA, PPRC, Harvard University's School of Public Health) indicated that specialties such as thoracic surgery were likely to experience substantial reductions in Medicare revenues if payments were tied to resource costs.[9] These studies also predicted that specialties such as family and general practice would experience sizable revenue increases under a resource-based fee schedule. Losing specialties typically were among those with the highest average per capita income, while winning specialties generally ranked among those with the lowest. With this in mind, I use *Income* as a measure of the extent to which specialties were the intended beneficiaries of the fee schedule (Pope and Schneider 1991).

Members of Congress were undoubtedly aware of the redistributive effect of the new payment system. At a March 17, 1989, hearing, health economist William C. Hsiao, one of the principal architects of the Harvard reports, testified that a resource-based fee schedule would result in payment increases for services provided by primary care and other low-income specialties and payment decreases for services provided by surgical and other high-income specialties (U.S. Congress 1989). His assessment was echoed at an April 20, 1989, hearing by representatives of specialties expected to fare well under the fee schedule (e.g., the American Academy of Family Practitioners), as well as representatives of losing specialties (e.g., the American Academy of Orthopaedic Surgeons) (U.S. Congress 1989).

Statements by members of Congress confirm that it is appropriate to consider low-income, but not high-income, specialties to be legislators' favored constituents. At a July 5, 1991, hearing, Senator Charles E. Grassley (R-IA), reacting hostilely to the Proposed Rule, made the following statement: "We believed, with everyone else, that the Medicare Physician Payment Reform was going to re-allocate money towards primary care practitioners" (U.S. Congress 1991, 6). Senator John D. Rockefeller (D-WV), a main proponent of reform, provided a similar account:

> From the beginning, Congressional intent regarding this law has been very precise. The law was intended to establish a logical, rational method for reimbursing physicians based on the resources required to carry out that service.
>
> Certain procedures, particularly high-tech, invasive ones [i.e., those performed by high-income specialties] have traditionally been over-valued, while others that require the investment of time and are, perhaps, more cognitive in nature [i.e., those performed by low-income specialties], are under-valued. Congress clearly wanted a new payment system that understood those differences and would correct the distortions (U.S. Congress 1991, 2).[10]

The key explanatory variables are interactions of income and specialty participation (*Professional Association Comments * Income* and *Individual Practice Comments * Income*). These interactions provide insight into the relationship between legislative wins and losses and the efficacy of comments. A negative relationship between these variables and revenue changes would indicate that participation by low-income, or winning, specialties exerted greater influence over HCFA decision making than comments submitted by high-income, or losing, specialties. This result would indicate that the notice and comment process operated in a manner consistent with the deck-stacking thesis.

Factors other than specialty participation may have affected HCFA decision making. For example, I expect that HCFA was more likely to increase payments to specialties with large professional associations. These groups have the resources necessary to activate congressional fire alarms and mount court challenges, two unpleasant scenarios from the perspective of agency officials, who "prefer to maintain a fair degree of internal autonomy" (Ferejohn 1987, 442). I measure size as membership in a specialty's primary professional association (*Professional Association Membership*) (*Encyclopedia of Associations* 1991).[11]

I also expect that HCFA was more likely to consider the preferences of specialties with greater *Financial Stake* in Medicare. Specialties that derive substantial revenue from the program (e.g., internal medicine and ophthalmology) would be more likely to notify Congress and the courts of their dissatisfaction with HCFA

[9] The results of these studies are summarized in U.S. Department of Health and Human Services 1989.

[10] Another reason that income is a valid proxy for legislative wins and losses is because it is distinct from Medicare revenues, which are a function of volume as well as fees. For instance, certain low-income specialties (e.g., general practice and internal medicine) encounter Medicare beneficiaries more frequently than some high-income specialties (e.g., otolaryngology and neurosurgery) and, therefore, derive greater revenue from the program.

[11] I use membership (more specifically, the log of membership) rather than budget because it is more readily available for many professional associations. This functional form implies that a percentage change in membership produces a unit change in revenues and takes into account the fact that specialties incur fixed costs when lobbying Congress and petitioning the courts. They must develop, for example, expertise regarding the resource costs of the services they provide, as well as the types of arguments and evidence that legislators and judges find persuasive. For smaller organizations, an increase of a given size may enable them to meet these costs and, as a result, substantially increase their influence in the notice and comment process. Larger groups may find that an equivalent increase in membership translates into a relatively small rise in influence, due to the fact that they have already met the fixed costs of participation.

decision making than those that generate relatively little revenue (e.g., infectious disease).[12]

Finally, professional associations vary in the extent to which they can credibly claim to represent a wide range of professional interests. In 1990, for example, membership in the American College of Radiology exceeded the population of radiologists by more than 10,000, while membership in the American Urology Association fell well short of the number of urologists. I expect that HCFA was more likely to increase fees for specialties with more representative organizations, as these types of groups have historically wielded greater influence over health policy than those with narrower constituencies (Rayack 1967; Starr 1982). I measure *Representativeness of Professional Association* as professional association membership divided by specialty population (Roback, Randolph, and Seidman 1992; U.S. Department of Health and Human Services 1993; U.S. Department of Labor 1994).

Methods

The most straightforward way to determine the influence of the explanatory variables on the setting of fees is through ordinary least squares (OLS) regression. This approach is potentially problematic, as the decisions of professional associations and individual practices to comment on the Model Fee Schedule and Proposed Rule may have been endogenous. Participation, for example, may have been a function of expected wins and losses, with losing specialties being more likely to comment than those expecting increases in Medicare revenues. This pattern would be consistent with Schattschneider's (1960, 39) observation, echoed by a wide array of scholars, that it is losers in policy battles "*who seek redress from public authority*." One implication of endogeneity is that the estimated effects of participation may be negative and significant when, in fact, comments exerted substantial positive influence over revenues changes (i.e., losing specialties would have experienced larger revenue decreases had they not participated extensively, and winners would have enhanced their gains had they submitted additional comments).

Despite this possibility, I do not model participation as endogenous because of the following methodological complications. First, the participation variables consist of counts of comments. As a result, equations examining the determinants of participation must be estimated through event count regression (e.g., Poisson, negative binomial) (King 1988, 1989). Second, the interactions of participation and income must also be modeled as endogenous. Together these econometric dictates imply the following system of equations: two first-stage equations, one of which is an event count, and a second-stage OLS equation. I am aware of no technique that facilitates estimation of such a system while making the necessary correction in second-stage standard errors (Gujarati 1995). With this in mind, I assume that physician participation is not endogenous, an assumption that increases the uncertainty attached to the standard errors and significance tests. The OLS results must therefore be interpreted with appropriate caution.

An additional methodological issue—heteroskedasticity—requires attention. Heteroskedasticity may arise because the unit of analysis is the physician specialty. Specialties vary widely in both size and stake in the Medicare program, a fact that may be reflected in greater error term variance for larger specialties and those with greater stake. As a result, I use White's (1980) covariance matrix estimator to calculate the standard errors.

RESULTS

Tables 3 and 4 present the results. Each table contains two equations, one in which participation is measured as the number of comments submitted by professional associations and one in which participation is measured by the number of comments submitted by individual practices. Table 3 focuses on the determinants of revenue changes across the Model Fee Schedule and Proposed Rule. None of the explanatory variables exert their expected influence. The fact that comments submitted early in the rulemaking process were ineffectual contradicts Magat, Krupnick, and Harrington's (1986) suggestion that early participation—unlike later, politically motivated participation—provides regulators with substantive information. This null finding, however, may be a reflection not of the nature of physician participation but of the way in which HCFA managed its authority. The Model Fee Schedule did not include the vast majority of Medicare services and, therefore, provided specialties with limited information about the likely effects of the fee schedule. In addition, the agency emphasized that this notice was extremely tentative and potentially subject to dramatic change, in part due to anticipated improvements in data quality (e.g., forthcoming Harvard reports). Finally, HCFA announced that it would not reply to comments submitted prior to the Proposed Rule: "If you wish to have your comments formally considered, they must be submitted to us in response to the proposed rule in accordance with the instructions specified in that rule" (U.S. Department of Health and Human Services 1990, 36178). By manipulating these facets of its procedural environment, the agency may have provided itself with an opportunity to operate relatively autonomously during the early stages of the development of the fee schedule.

Table 3 indicates that HCFA was not more responsive to comments submitted by low-income specialties than to comments submitted by high-income specialties. These results imply that legislators' favored constituents did not benefit disproportionately from the initial comment period. This stage of the notice and comment process, in other words, did not operate as an

[12] When examining change across the Model Fee Schedule and Proposed Rule, I measure stake as the log of Model Fee Schedule revenues. Likewise, I measure stake as the log of Proposed Rule revenues when focusing on Proposed to Final Rule changes. These measures capture specialty stake in the *Federal Register* notice directly preceding the comment period under consideration.

TABLE 3. The Determinants of Changes in Specialty Revenues across the Medicare Model Fee Schedule and Proposed Rule

Variable	Parameter Estimate (Standard Error) Professional Association Participation	Individual Practice Participation
Constant	12,433 (57,014)	128,081* (57,972)
Professional Association Comments	115,156 (72,603)	
Individual Practice Comments		25,774 (20,096)
Professional Association Comments * Income	−855.53 (524.14)	
Individual Practice Comments * Income		−127.83 (128.90)
Professional Association Membership	-6,083 (8,804)	−21,118 (11,134)
Financial Stake	2,348 (4,923)	−8,986* (4,020)
Representativeness of Professional Association	336.83 (679.34)	808.10 (773.55)
R^2	.31	.14
Number of observations	52	52

Note: The dependent variable in these OLS regressions is change in specialty revenues across the Model Fee Schedule and Proposed Rule, in thousands of dollars. I used the *regress* procedure in Stata 5.0 to generate these results. *$p < .05$.

TABLE 4. The Determinants of Changes in Specialty Revenues across the Proposed Rule and Final Rule

Variable	Parameter Estimate (Standard Error) Professional Association Participation	Individual Practice Participation
Constant	−122,633** (49,017)	−118,841* (52,244)
Professional Association Comments	−38,643 (27,970)	
Individual Practice Comments		−469.33 (351.79)
Professional Association Comments * Income	355.74* (164.24)	
Individual Practice Comments * Income		4.24* (2.18)
Professional Association Membership	16,547 (10,699)	15,748 (9,177)
Financial Stake	7,002** (2,361)	8,310* (4,232)
Representativeness of Professional Association	−770.01 (917.55)	−416.70 (638.91)
R^2	.60	.51
Number of observations	57	57

Note: The dependent variable in these OLS regressions is change in specialty revenues across the Proposed and Final Rule, in thousands of dollars. I used the *regress* procedure in Stata 5.0 to generate these results. *$p < .05$; **$p < .01$.

instrument of political control in the manner posited by the deck-stacking thesis.

Table 4 provides evidence that comments submitted after the Proposed Rule exerted mixed influence over HCFA decision making. The null parameter estimates attached to the participation variables mirror the results generated by Magat, Krupnick, and Harrington (1986, 143), who point out that such findings raise "serious questions about the responsiveness of the process." The efficacy of participation, however, varied across specialties. The coefficients for both interaction variables are positive and significant, indicating that comments submitted by high-income specialties exerted greater influence than comments submitted by low-income specialties. In other words, high-income specialists expected to experience revenue decreases under a resource-based fee schedule derived greater benefits from this stage of the notice and comment process than legislators' favored constituents (low-income specialists). This finding, together with the results presented in Table 3, demonstrates that neither comment period facilitated political control of HCFA decision making in the manner posited by the deck-stacking thesis.

As Table 4 indicates, specialty stake in the fee schedule influenced HCFA decision making; agency officials were more responsive to specialties that derive substantial revenue from the Medicare program. In addition, there is modest evidence that HCFA was more responsive to specialties with large professional associations. This variable's estimate is positive and nearly significant ($p = .09$) in the equation measuring participation as the number of comments submitted by individual practices. This result suggests that while professional associations' letter-writing campaigns were of limited utility (i.e., the number of individual practice comments did not affect revenue changes), HCFA officials nevertheless considered organizational resources when setting fees.

CONCLUSION

This article has demonstrated that physician participation in the notice and comment process did not influence Medicare physician payment reform in the manner posited by the deck-stacking thesis. HCFA was not more responsive to comments submitted by low-income specialties after publication of the Model Fee

Schedule than to those submitted by high-income specialties. In addition, the agency was more responsive to comments submitted by high-income specialties after publication of the Proposed Rule than to those submitted by low-income specialties. Given that the latter were the intended beneficiaries of the redistributive component of the fee schedule, this finding implies that the notice and comment process did not facilitate political control by channeling HCFA's decisions "toward the substantive outcomes that are most favored by those who are intended to be benefitted by the policy" (McNollgast 1987, 244).

As McNollgast (1987) point out, legislators can stack agency decks in a variety of ways. One example is burden-of-proof standards. Prior to 1978, the Civil Aeronautics Board (CAB) required airlines proposing price changes to demonstrate the appropriateness of requested rates. After passage of the Airline Deregulation Act, the burden of proof shifted to carriers seeking to block prospective fare hikes or reductions. Not surprisingly, CAB more readily approved airline requests in the aftermath of this procedural change. As this case illustrates, burden of proof operates in a manner that makes it difficult for constituents excluded from congressional bargains to secure policy advantages at the expense of legislators' favored constituents.

More generally, Spence (1997, 205) makes a distinction between procedures that "increase the transaction costs of agency decision making" and those that increase the "costs of making a *particular* decision." The notice and comment process falls into the former category; while it facilitates external participation in rulemaking, it does not, in and of itself, direct agencies toward specific policy choices. Spence (1996) provides empirical evidence for this distinction in the context of hydroelectric licensing. The Federal Energy Regulatory Commission (FERC) did not become more responsive to environmental interests in the aftermath of a mandate to consider the environmental effects of proposed projects. The environmental content of FERC decisions changed only after Congress required the agency "to accept any license conditions proposed by state or federal environmental or resource agencies, or to provide a written explanation of the reasons why the proposed conditions were rejected" (Spence 1996, 8). These findings suggest that administrative procedures vary in the extent to which they predispose agencies toward particular policy choices and, ultimately, serve as instruments of political control.

Why would members of Congress select administrative procedures that limit their ability to stack agency decks? As Hill and Brazier (1991) point out, legislative outcomes vary in the extent to which they resolve differences among competing constituencies. The Clean Air Act Amendments of 1977, for example, did not fully reconcile the divergent preferences of industrial and environmental interests. The act's procedural requirements, therefore, ensured that both parties had access to EPA without stacking the deck in favor of the economy or environment. Hill and Brazier (1991, 398) argue that since legislative bargains are frequently fraught with these types of ambiguities, "the conditions conducive to this ex ante control [i.e., deck-stacking on behalf of legislators' favored constituents] cannot be expected to exist generally, and ex ante controls seem easily undermined by a large variety of commonly found situations."

Despite appearances to the contrary, payment levels promulgated in the Final Rule may have been consistent with congressional preferences. Legislators may have anticipated that HCFA officials would be more responsive to high-income specialties and, as a result, mandated the development of a fee schedule that was more redistributive than that preferred by the median member. Furthermore, members of Congress may have been content with the discretion exercised by HCFA. As Bawn (1995, 63) points out, the "problem is that administrative procedures designed to prevent bureaucratic drift also limit the agency's ability to research policy consequences or to make decisions that reflect its expertise." Given the technical nature of Medicare reform, legislators may have been willing to trade strict control over redistribution for accurate information about physicians' resource costs. Finally, members of Congress, as discussed earlier, monitored HCFA decision making through instruments other than the notice and comment process. These activities (e.g., creating PPRC and conducting oversight hearings) may have effectively protected the bargain struck in Congress.

In sum, this article does not demonstrate that HCFA enjoyed autonomy in the setting of physician fees or, more generally, that Congress exerts little control over the bureaucracy. Rather, it demonstrates that a prominent administrative procedure does not necessarily promote agency policy choices that serve the interests of legislators' favored constituents. This finding is important because of the attention that the deck-stacking thesis has received since the seminal work of McNollgast. It also suggests, in tandem with previous empirical results, that elected officials may most effectively constrain bureaucratic discretion through mechanisms such as personnel decisions, budgetary manipulations, and procedures that predispose agencies toward particular policy choices. Given the widespread use of the notice and comment process in contemporary American politics (Kerwin 1994), legislative bargains may unravel within a wide range of policy areas and agencies.

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BOOK REVIEWS

Political Theory

Freedom and Culture in Western Society. By Hans Blokland. New York: Routledge, 1997. 319p. $74.95.

Brent L. Pickett, *Chadron State College*

Hans Blokland revisits the debate between positive and negative freedom. He seeks to defend a chastened version of positive freedom, one which emphasizes personal autonomy. While he defends the worth of a large scope for negative freedom, Blokland ultimately believes that a positive or exercise-concept of freedom can be made largely compatible with negative liberty. Furthermore, he argues that through an active arts and culture policy the state has a significant role to play in the promotion of autonomy.

Blokland displays a strong grasp of the voluminous literature on the debate over freedom, especially the debate in the Anglo-American tradition. It seems as if virtually no Anglo-American theorist writing about freedom in the last 30 years has escaped his attention (though remarkably enough there is no mention of Richard Flathman's *The Philosophy and Politics of Freedom* [1987]). Indeed, at times his focus is too much upon contemporary works; the only pre-twentieth century thinker to make a real appearance in this work is J. S. Mill. Blokland focuses much of his attention upon Isaiah Berlin, devoting a chapter to a careful explication and criticism of Berlin's "Two Concepts of Liberty" and other writings on freedom. This is appropriate not only because of the preeminence of Berlin's work in this area but also because Blokland's approach closely resembles that of Berlin. Both are ethical pluralists who are willing to admit that at least certain forms of negative and positive freedom are genuinely valuable; they simply disagree about where the tradeoffs actually lie.

Admitting that both negative and positive freedom are real goods opens up what Blokland calls an *emancipation dilemma*, which has two dimensions, one individual and the other social. On the one hand, Blokland favors an active cultural dissemination policy, since he believes this will help develop capacities essential to autonomy, such as the ability to reflect critically upon the society one inhabits. On the other hand, Blokland recognizes that such a policy can be coercive, especially for those adults who lack autonomy and do not see it as a good. Is paternalism toward such individuals justifiable? If contemporary society inculcates undesirable attitudes and modes of life, such as a shallow materialism, then how can society be systematically reorganized to eliminate these tendencies, while still recognizing a large degree of negative freedom? Blokland's goal is to articulate a conception of autonomy which is compelling to a wide range of theorists and "which makes it possible to break out of the dilemma . . . in a manner acceptable to the parties concerned" (p. 6). In his discussions of positive freedom and autonomy, Blokland does much to clarify a rather disparate debate. In effect, he conducts a survey of recent positive freedom theorists, such as Charles Taylor and Benjamin Barber, in order to sort out what is essential to theories of positive freedom. The ideal of autonomy at which Blokland arrives is rather demanding. Autonomy requires "a balanced, integrated personality," along with self-knowledge, will power, some independence of mind, and an "active theoretical rationality" (pp. 81, 82). Yet, because this conception is demanding, it is doubtful whether it will persuade a wide range of theorists, as Blokland uses this conception to defend paternalism, in the hope that a temporary interference in a person's life may, in the long run, greatly expand her number of meaningful choices.

Blokland also thinks paternalism is "sometimes inevitable . . . in order to break up existing structures of socialization or a prevailing inertia" (p. 157). That is, in the social dimension of the emancipation dilemma Blokland again supports active intervention in order to foster the capacities necessary for autonomy. He realizes this opens him to various charges of recommending coercion, such as Berlin made, yet Blokland contends that "it is undesirable that people, out of fear of the accusation of totalitarian thinking, leave cultural formation as a matter of course to the 'free play of social, in this case economic, forces,' and not to democratically accountable institutions such as government, parliament, political parties, educational institutions and the press" (p. 185). Though one can certainly wonder just how democratically accountable these latter institutions are, there are good reasons for not leaving socialization up to market forces. Since participation in culture often has the effect of opening persons up to alternative ways of living, encouraging critical reflection upon one's society and its norms, and aiding the development of the capacities necessary for autonomy, Blokland argues for a greater social commitment to the arts and culture. Western societies have made the mistake of pursuing only a supply-oriented culture policy; that is, subsidizing theaters, libraries, and so on, in the hope that people will attend. He provides evidence, however, that participation in culture is lacking especially among the poor, and that cultural activity is declining in general among all groups. Thus, Blokland believes that governments in particular need to do more to increase the demand side in their cultural policies; for example, through altering school curriculums to give the arts and culture a greater emphasis, and by subsidizing more amateur art performances.

Because Blokland combines an extensive literature review with his own valuable analysis, this book is of great value to those interested in theories of freedom. Yet, it should have an even wider appeal: The manner in which he connects abstract issues about human nature and the nature of freedom with issues of cultural policy is powerful and well argued. This book has a great number of virtues: a wealth of interesting asides about variation between countries in cultural policies; a discussion of the effects of moving from an oral to a written culture, and now to a visual (electronic) one; and too many other topics to mention. Even though the translation is sometimes awkward, this book deserves a large audience.

Skepticism, Belief, and the Modern: Maimonides to Nietzsche. By Aryeh Botwinick. Ithaca, NY, and London: Cornell University Press, 1997. 249p. $39.95.

Fredrick Appel, *Harvard University*

Aryeh Botwinick is nothing if not ambitious. In this provocative and occasionally insightful book he proposes nothing less than a rereading of Western intellectual history as the progressive triumph of skepticism. The general idea is that skepticism, the view that "our theoretical formulations do not necessarily intersect with a reality outside themselves" (p. 18), initially presented itself in an inchoate and unselfconscious form in antiquity and the Middle Ages before gradually emerging in modernity and postmodernity as a more mature (i.e., more self-conscious and self-confident) mode of being in the world.

Although Plato makes a (surprising) cameo appearance as a posterboy for premodern skepticism, Botwinick places the

twelfth-century Jewish philosopher Maimonides center stage as the exemplary premodern thinker who placed skepticism on its road to triumph. In the course of his "negative theology," Maimonides dismisses popular, literal accounts of God as mere linguistic constructions that have no basis in reality. This insight is said to have opened the door for more mature forms of skepticism to expose linguistic constructivism as an exercise that "may proceed to infinity" (p. 30). Botwinick argues that Thomas Hobbes appropriates key Maimonidean motifs, thereby extending the scope and power of skeptical doctrine. Finally, skepticism as a mode of being emerges in its fully developed form in Friedrich Nietzsche, who is portrayed as both the quintessential modern thinker and as an early exemplar of postmodernism, "modernity's most elevated moment of reflectiveness and self-consciousness" (p. 9).

There is much to admire in this learned book, particularly in the sections on Maimonides and Hobbes. Botwinick points to some clear and important parallels between the austere monotheism of Maimonides and the account of God in the second half of Hobbes's *Leviathan*. Even if Maimonides were not a direct formative influence on Hobbes, Botwinick is right that something like the Maimonidean vision informs Hobbes's account. In light of Hobbes's well-known assessment of the dangers of allowing nonstate actors to proclaim themselves religious authorities with privileged knowledge of the Deity, it is understandable why he would be attracted to something like the Maimonidean notion of a wholly inscrutable God.

Unfortunately, Botwinick's efforts to make each of his featured thinkers fit into his metathesis lead to some highly problematic interpretations. While it is true that Maimonides sometimes seems to adopt the perspective of a "field anthropologist" vis-à-vis Jewish ritual practices (p. 133), describing them functionally rather than in terms of their truth value, Botwinick goes too far in asserting that *The Guide of the Perplexed* points ultimately to the "foundationless character of all our knowledge claims" (p. 32; cf. 41). Although Maimonides was at pains to stress the limits of reason, he also insisted upon the possibility of apprehending things outside the mind (*Guide of the Perplexed*, I, chap. 50). Botwinick seems aware of this at times; for example, he notes that for Maimonides "only the solitary thinker has an opportunity to approach the limits of knowledge and of truth" (p. 149). But he then suggests, strangely, that the truths attained by these scholars are strictly "private" (ibid.).

Botwinick is certainly on solid ground in making the case for Hobbes's moral skepticism. Once again, however, he strains credulity by asserting that it is so thorough-going that it leads us to bypass altogether any reference to an external reality (pp. 5, 152). Far from presenting his views as a closed system, Hobbes insists upon the existence of at least a few cross-culturally valid facts (e.g., our striving for self-preservation, our valuing existence as good) that a modern political science can unearth. (Botwinick notes Hobbes's admiration for modern science in passing, without reflecting on the difficulty this creates for his thesis.)

The view that Nietzsche "attempts to evolve a philosophical discourse situated beyond preoccupation with truth" (p. 1) has been standard in Nietzsche scholarship until the last decade. Unfortunately, Botwinick shows no sign of having considered more recent work (by, for example, Maudemarie Clark, John Richardson, and Peter Berkowitz) which marshalls impressive textual evidence that demonstrates (to my mind, at least) Nietzsche's abiding interest in truth. If this recent scholarship has any bite, the central question of Botwinick's Nietzsche chapter—does Nietzsche successfully come up with a strategy for salvaging his extreme skepticism and relativism?—loses its dramatic force.

Botwinick's metathesis might (possibly) be of concern to philosophers interested in epistemology, but why should political theorists care about this book? The author assures us that the story of skepticism's triumph has salutary political consequences: Skepticism, we are informed, provides strong support for both liberalism and participatory democracy. If all arguments about reality are indeed circular, then this fact necessarily "undercuts the claims of particular individuals or groups to superior knowledge" and "justifies the inclusion of as many people as possible in public decision making" (p. 7). Moreover, the "most coherent reading" of liberalism reveals it to be "grounded in skepticism" (p. 6). I am not so sure, however, that the flourishing of liberal democracy is as dependent upon the veracity of Botwinick's metathesis as the author thinks. Although much contemporary political theory suggests otherwise, the idea that epistemology drives politics is not self-evident. It seems to me that a given epistemological stance can lead to a bewildering variety of political stances. The political credentials of skepticism, in other words, remain an open question.

Even if Botwinick's enthusiasm for his metanarrative has led him into such misreadings, this fact alone does not disprove his story. Realist conceptions of truth are taking a beating from all sides in the academy these days: Mainstream analytic philosophers such as Michael Dummet and Crispin Wright are in the antirealist camp, and of course antirealism is a staple of postmodern writing inspired by Foucault, Deleuze, and Derrida. So Botwinick can count on a lot of support. Yet, the possibility of a strongly realistic conception of truth should not be discounted. Contemporary philosophers such as Alasdair MacIntyre, Martha Nussbaum, and Charles Taylor agree that we must discount naïve positivism without following Botwinick to the conclusion that our language games are entirely self-enclosed (p. 83). The debate between realists and antirealists is far from over; as Botwinick himself concedes, "the returns are not yet in" (p. 18). As the debate unfolds, Botwinick's metathesis must be taken seriously as a contender. Hence, for all its flaws, the book is important.

Hannah Arendt and the Meaning of Politics. Edited by Craig Calhoun and John McGowan. Afterword by Martin Jay. Minneapolis and London: University of Minnesota Press, 1997. 376p. $54.95 cloth, $21.95 paper.

Phillip Hansen, *University of Regina*

This fine collection of essays, prepared originally for a conference at the University of North Carolina at Chapel Hill in 1995, makes a valuable contribution to the continuing debate about the meaning and significance of Hannah Arendt's political thought. Largely eschewing exegesis, the authors attempt to think creatively with and against Arendt in the quest for "a political theory that gives a richer meaning to the idea of politics than has seemed common in recent years" (p. 2). In line with current preoccupations in political thinking, the book focuses on what might broadly be called the aesthetic dimension of Arendt's work. It devotes considerable attention to the roles that judgment and public space play in her thought; as a result, the differences between so-called postmodernist and Habermasian interpreters figure prominently throughout. The arguments of the papers are developed against the backdrop of the complex relation of Arendt's ideas to the modern age, the legacy of enlightenment, and especially the consequences of totalitarianism.

Reflecting the view that a conference is itself an action in public space, the book is structured as a dialogue among the participants. The papers written by Susan Bickford, Nancy Fraser, Craig Calhoun, and Stephen T. Leonard are direct responses to those of the other authors, while Martin Jay provides a perspective on the conference as a whole. For a work of this kind, the essays are of a uniformly high quality. Not all are, to this reviewer, equally compelling. Those by Leonard and by Anthony J. Cascardi seem to misconceive important dimensions of Arendt's ideas, while Cascardi and Dana Villa overplay the "agonistic" character of Arendt's notion of public space, forgetting that for Arendt political action ought to be properly bounded by the requirements of power and thus a certain sort of solidarity. But all are well written and thoughtful.

The book offers insights into the phenomenology of political experience, the ways in which politics can be personally meaningful in the face of current predicaments, and it shows how Arendt's ideas contribute to the kind of understanding required. This is the book's strength, but it is also its weakness. There is today a tendency (to which this reviewer has also contributed) to emphasize the micropolitics of experience, to the exclusion of macropolitical questions that involve confronting issues of history and social structure, as Arendt herself often did. For a variety of reasons, not least the assumption that actually existing liberal democracy is the best we can do, much recent political thought has failed to confront the question of how to relate micropolitical to macropolitical concerns.

Not every essay in this collection ignores this admittedly complex problem. Leonard's argument assumes the need to face it, while Lisa Disch provides an imaginative attempt to relate Arendt's perspectival account of judging to the 1970s experience of feminist consciousness raising, with a view to institutionalizing a "cubist" public sphere adequate to the pluralist character of contemporary societies. And both Eli Zaretsky and Nancy Fraser, the one explicitly, the other implicitly, remind us that Arendt's political thought emerged in large part as a dialogue with Marx; Fraser is particularly good in sharply criticizing the proclivity to overburden normatively Arendt's notion of action, to the exclusion of considerations of justice and equality, a proclivity Arendt herself abets. These latter pieces suggest that political theory must become reacquainted with political economy if it is to address meaningfully contemporary political questions.

In light of these considerations, the essay by Richard J. Bernstein on the relations among thinking, judging, and evil stands out. It is not the best essay—that honor falls to Kirstie McClure's splendid exploration of the complexities of Arendtian judgment—but it is in a sense the most representative. After carefully discussing Arendt's account of Adolf Eichmann, and the problem of evil and complicity with it, Bernstein argues that Arendt does not, and perhaps cannot, tell us how to distinguish between those who in the face of moral collapse can still judge and follow their conscience and those who, like Eichmann, either lost this ability or never had it. In Bernstein's view, this reveals a contradiction in Arendt's thought: While "Arendt herself was critical of what she once called 'the optimistic view of human nature', which presupposes 'an independent human faculty. . . . which judges anew and in full spontaneity every deed and intent' . . . ironically it is Arendt herself who seems to *want to believe* that there is such a human faculty . . . the faculty of judgment" (p. 318). As Bernstein sees it, she has no way of reconciling these positions, and this suggests a terrible and unresolvable human dilemma.

This contradiction is posed too sharply, however, because it is too wrapped up with the micropolitics of personal judging. In *The Origins of Totalitarianism* (3d ed., 1973), Arendt argued that totalitarianism and thus the moral collapse it brought with it fed upon loneliness and isolation, themselves the products at least in part of human misery and material deprivation. In other words, there are conditions under which at least the possibilities for judging can be more fully secured, as opposed to those conditions under which it is more likely to be suppressed. Such conditions are alterable and involve changes in social structure—and this in spite of Arendt's well-known (and overly exaggerated) hostility to the "social" as well as her tendency in her late work on judging to overlook her own earlier argument. More decent socioeconomic and political conditions provide, of course, no guarantee against unconscionable behavior; nothing can. But they do call for a more nuanced account of human capabilities. Our encounters with the challenges, problems, and dilemmas of politics are not as starkly "existential" as Bernstein's account, and many of the essays in this book, imply.

One thing clearly emerges from this thought-provoking collection: After twenty years of intensive examination, Hannah Arendt's political ideas continue to challenge and provoke—and, on occasion, to frustrate as well.

Nietzsche's Dangerous Game: Philosophy in the Twilight of the Idols. By Daniel W. Conway. Cambridge: Cambridge University Press, 1997. 267p. $54.95.

Tracy B. Strong, *University of California, San Diego*

Daniel Conway has written an important book focusing on what he calls Nietzsche's "mature political thinking" (p. 1). He identifies this as the work Nietzsche completed after the fourth part of *Thus Spoke Zarathustra* (1885) and the onset of his insanity in early 1889. That Conway can speak of Nietzsche's "mature" political writing is a sign of how far Nietzsche studies have come in the last quarter-century. The question used to be whether he had any political thinking worth paying attention to at all.

The dynamic premise of this book is that Nietzsche knew himself to be implicated in and to exemplify the cultural crisis of modernity. Nietzsche's name for this crisis is "decadence," which Conway reads as a mis- or inappropriate matching of instincts to life—a kind of collective neurosis and/or psychosis whereby a culture consistently makes choices harmful to its health. Conway spends a strong chapter arguing that Nietzsche intended this diagnosis as empirical; I think him right about Nietzsche's intention here (and a good deal of recent work on Nietzsche—by such authors as Keith Ansell-Pearson, Babette Babich, Mark Warren, and David Owen—provides additional support for versions of this claim).

If Nietzsche is indeed writing about existing historical conditions, then his political philosophy will of necessity exemplify at least some of the problems identified. How could any project of cultural revolution not make the situation worse? And this in turn implies, as Conway argues, that as an author Nietzsche will not be able to control the understandings given to and uses made of his work. (This is the situation that precipitates Zarathustra's departure from the marketplace at the beginning of the work that bears his name.)

Zarathustra's failure in the marketplace thus precipitates in Nietzsche a new approach, which Conway holds to be characteristic of the work of the last period of his life. In his attempt to deal with the problem of his own involvement in that of which he is critical, Nietzsche develops a number of rhetorical strategies. Conway analyzes these under the term

parastrategesis, which designates "those rhetorical strategies that [Nietzsche] intentionally crafts to exceed the sphere of his own authorial control.... [It] constitutes an arena of dialectical contest within which the decadent Nietzsche is forced to confront his most independent readers" (pp. 153–4). (I should note that part of the richness of the book is a discussion of Nietzsche's philosophy as a kind satyr playing with its audience).

Nietzsche's project is thus political in two senses. First, it is Nietzsche's intention to found "communities of resistance" (p. 95) to the condition of modernity. Second, such communities will be of a nonliberal politics in that they will be composed of those who would find themselves in resistance in general, rather than in particular, to the society of which they are members. Such politics will not address specific problems but society as a whole (on this last point I may be reading something into Conway, but it is, I think, implied). Conway notes (as have some others, including this reviewer) that by fall 1888 Nietzsche was willing to accept for himself the designation of a political lawgiver. The claim here is that Nietzsche's assumption of a political stance corresponds to his having finally accomplished and embodied the full critique of Christianity. For Nietzsche, Christianity is the source and cause of the political ills of the modern world.

From whence the Anti-Christ, now seen as a necessary *political* figure? Nietzsche, for Conway, wants to turn his readers (his perfect readers) into his agonistic enemies, to embody himself in them as the opponents of Christianity. Parastrategesis is the "dangerous game" of Conway's title. For this strategy to work, however, Nietzsche would have to have as perfect knowledge of himself as he hoped for in his readers. Conway scatters the book with references to Nietzsche's "unconscious prejudices" (p. 16) and "unwitting" self-presentations (p. 131). In the end, Nietzsche "did not possess sufficient self-knowledge to close the circle of his thought and complete his critical project" (p. 248). Conway illustrates this with an extremely interesting reading of Nietzsche's *The Antichrist* (or *The Antichristian*, the German can mean both), a reading all the stronger for showing why most readers have tended to pass over this book as an embarrassment in Nietzsche's corpus.

What, then, is Nietzsche's accomplishment, if we accept that his project fails and must fail? For Conway, Nietzsche provides us in his own example with "a representative type of life that faithfully express the fractured ethos of the age.... Owning to his unique destiny as a representative exemplar of late modernity, he accommodates within his tortured soul the riddles and tensions that define his age" (pp. 233–4). Conway concludes, however, that his "actual readers" are only pale reflections of his would-be exemplars, "nook-swelling creatures of *ressentiment*, versed in the 'effeminate' arts of subterfuge, duplicity and deception,... treacherous followers who will betray him and distort his own teachings to suit their own designs" (p. 255).

I do not know exactly whom Conway has in mind as the membership of this last inadequate group. His footnotes are a model of learning and kindness; he wears them lightly and covers most of the important (and less important) scholars. But beyond naming names, there is an important point here: Nietzsche has the quality of lending himself to promiscuous and self-serving appropriation. I am not as sure as is Conway that this is a bad thing; at least I do not think that the matter can stop there. In my experience, Nietzsche writes not only in such a way that it is inevitable he be misunderstood but also with the knowledge that being misunderstood is a necessary part of being understood. For example, we all know that Nietzsche says "whatever does not kill me makes me stronger." *Conan the Barbarian* and Gold's Gym are not the first appropriations. But Nietzsche is quoted out of context. In *Twilight of the Idols*, a book that is to be a "declaration of war" but also a "wounding" of himself, he writes: "*From the military school of life*: whatever does not kill me makes me stronger." So part of our understanding of this aphorism has to do with why we want to misremember it. And, presumably, understanding why we want to misremember will tell us something about the kinds of things from which we feel the need to protect ourselves. We will have to think about our thinking.

Conway has written an excellent and thoughtful book. The difference between him and me has to do with the possibility of writing political theory in and for the modern age. If Nietzsche cannot be avoided—a claim in itself, but one which I think true—then Conway shows us that political theory is in fact impossible; it will be only ideology or neurosis. If what I say about Nietzsche's texts is true, then they have the quality of not allowing us to spare ourselves from thinking, and that may be more than just a start to political theory.

Ways of War and Peace: Realism, Liberalism, and Socialism. By Michael W. Doyle. New York: W. W. Norton, 1997. 557p. $30.00.

Steven Forde, *University of North Texas*

This lengthy tome undertakes a truly ambitious project, mining the thought of the past two millenia for guidance on how to approach the challenges of international politics today. Michael Doyle structures his account around the three outlooks that have dominated international thought during the twentieth century: realism, liberalism, and Marxism or socialism. Each of these outlooks has older roots, and Doyle explores them at length. Large portions of his book are devoted to treatments of Thucydides, Kant, Marx, Rousseau, Schumpeter, and a host of other writers who contributed to the development of the three paradigms. These writers are often treated perfunctorily in books on international politics, if they are treated at all, but Doyle's discussions are thorough and nuanced. Specialists on each of the authors may quarrel with details of the exposition, but very few will be well enough versed in all the thinkers to play the critic. In that respect, the book is encyclopedic in its range.

The range is impressive, but it creates some problems as well. Some readers may complain that the book lacks a clear thesis or agenda, a criticism that receives some justification from the far-flung and diffuse nature of the work. The same qualities sometimes make the thread of the argument hard to follow and, occasionally, lead the author into redundancies. It is also true that, by dint of giving every thinker and every perspective a sympathetic hearing, Doyle sometimes leaves it unclear where he stands. This I do not regard as a vice, though it does lead to occasional confusion as to what Doyle is saying in his own voice.

The aim of the book is not merely to interpret theorists but to move beyond this and bring the insights of these traditions to bear on some of the hard problems faced by states and statesmen today. Woven in with the theoretical expositions are sometimes extensive discussions of historical and contemporary events. These are examined with a view to the light that each of the three fundamental theories can cast on them—and vice versa. One of the most interesting aspects of this book is its sustained attempt to use historical and contemporary case studies to test the theories. As Doyle points out, the lack of such testing is a shortcoming of much current literature, in which the underlying theories of real-

ism, liberalism, or socialism often appear as mere assumptions. In the separate sections of the work devoted to each of the fundamental theories, Doyle undertakes both to explore the theory in question and to test it. Thus, under "realism," he identifies structuralist, fundamentalist, and constitutionalist variants of the theory and explores the classical and contemporary authors who have shaped them. He then uses the case of the (shifting) balance of power in eighteenth-century Europe to test which of these variants best explains events (the constitutionalist model, emphasizing domestic political arrangements, seems to be the winner: p. 192).

In the course of the book, Doyle makes many arguments that should materially improve the state of the discussion in international theory. He shows how Machiavelli's fundamentalist theory is more consistent (and thus more instructive) than most versions that have followed it. He argues that Hobbes's structuralism is superior to more modern variants in that it is not purely structural: Anarchy leads to conflict only given certain suppositions about human nature. Doyle's discussion of Kant and the liberal paradigm sheds light on the "democratic peace." As scholars have come to realize, this phenomenon would be better labeled the "liberal peace," as Kant would have preferred. Doyle shows that "liberalism" must be even further refined and restricted to vindicate the hypothesis. Thucydides' Athens and Machiavelli's Rome were characterized by "liberty" but were far from pacific. It is only distinctly modern liberty, universalist and nondiscriminatory, that makes liberal regimes peaceful (p. 306). When discussing the socialist tradition, Doyle outlines important differences between Marx and Lenin regarding their theories of international politics and imperialism, and he locates their thought firmly within the historical context that shaped them. In one indirect test, Lenin's theory does not fare well (p. 359), but Doyle exonerates the socialist movement as a whole from the charge that individual parties' support for war in 1914 was a betrayal or refutation of their professed internationalism (pp. 368–77). He argues that the prudential component allowed for in Marxist international thought can explain the parties' behavior.

The final sections of the book bring the three traditions to bear on some of the tough ethical and practical issues of international politics today. Doyle explains how realism, liberalism, and socialism would each approach the issues of intervention and of international (re)distribution of wealth, and how they might see the future of the international system. Policymakers' responses to each of these challenges will depend decisively upon which perspective is taken, so Doyle's effort to trace the practical implications of each of the theories in this matter is a real public service. The centerpiece of his treatment of intervention is a discussion of the "hard case" of U.S. intervention in Grenada. He finds the intervention ethically justifiable despite its violation of international law. Its justification, though, is not the one that (he finds) motivated President Reagan, but rather the principle of Grenadan self-determination. In the matter of the international distribution of wealth, Doyle argues for the position that some redistribution is a duty. This redistribution should be more extensive than allowed by the current U.S. foreign aid budget, but its appropriate level is difficult to peg with exactness (p. 435). According to Doyle, appropriate redistribution will depend upon many factors, such as the moral standing of individual wealthy and poor states (p. 434), whether they are participants in the "liberal peace" (p. 435), the legitimate security concerns of the donor (p. 435), and concern for the moral autonomy of citizens in the wealthy states (they should not be coerced into the role of "moral saints," p. 433).

This highly qualified verdict gives a good sense of the type of theoretical outlook Doyle wishes to forge as a guide to contemporary policy. It is a mixed one. Doyle wants to fashion an outlook on international politics that combines preferred elements from each of the three fundamental perspectives (p. 495). This does not necessarily mean a sacrifice of theoretical coherence, since at least some authors in each of the traditions mix elements in just this way. Perhaps the perspective Doyle prefers is closest to his understanding of Thucydides, who combined fairly hard-headed realism with a genuine moral concern (pp. 82–3). This would not be a bad model to follow. The outlines of Doyle's international ethics, however, are somewhat more difficult to make out. This is in part because of his laudable refusal to oversimplify the complexities of international politics and ethics, and in part because he wishes to provide guideposts for thinking, rather than a full-blown theory of his own. Readers wishing to make a fresh start on their thinking about international politics and ethics will find this book an admirable starting point.

Leo Strauss and the American Right. By Shadia B. Drury. New York: St. Martin's, 1997. 239p. $35.00.

James F. Ward, *University of Massachusetts Boston*

Shadia Drury's book is necessary reading for anyone interested in Leo Strauss's political teaching, its significance, and its influence. Although it is not a perfect book of its kind, it raises questions disciples of Strauss should, but I suspect many will not, address with a seriousness commensurate to their importance. Strauss appears here as a thinker of the radical Right who has had a profound influence on the "political triumph of neoconservatism in America" (p. xi). Drury makes a strong case that Strauss is profoundly hostile to liberal democracy; his alleged defenses of it, so frequently cited by his students or defenders, are both misunderstood and given excessive weight by them. This is an extremely provocative reading of Strauss, who is portrayed by many Straussians as having recovered classical antiquity the better to defend liberal democracy against its own excesses, and by many of his critics as having provided an especially influential statement of political conservatism. In this book, Strauss is no conservative but, rather, a radical Nietzschean who rejects modern liberalism or liberal democracy root and branch because he believes that it erodes and destroys the public and specifically religious orthodoxy necessary for the maintenance, even the very existence, of political society.

According to Drury, Strauss believes that because genuine philosophers recognize that the "truth is dark and sordid," they construct salutary myths, orthodoxies for the many, while teaching their disciples that "all truth is fabrication, that justice is doing good to friends and evil to enemies, and that the only good is one's own pleasure" (pp. 17–8). She further holds that Strauss's antipathy to liberalism is principled; he opposes its fundamental ideals, "secular politics, human rights, equal dignity, and individual freedom" and not simply its faults (p. 10). Drury argues that unlike conservatives, however, Strauss does not truly appeal to tradition, for he knows that "tradition is the conscious creation of philosophers-lawgivers-prophets" or Nietzschean supermen who invent, and must then conceal their invention of, the myths or frauds required for society (pp. 11–2). This book continues the author's interpretation of Strauss as presented in her *The Political Ideas of Leo Strauss* (1988) and in a chapter in a later book, *Alexandre Kojève: The Roots of Postmodern Politics* (1994).

For Strauss and his disciples, Drury argues, liberal democracy is exemplified by the Weimar Republic, whose fate could well be America's future unless liberalism is somehow diminished in or purged from the American regime. She maintains the political actions of Straussians consist of attempts to reinterpret the American founding in order to detach liberalism from it as much as possible and to forge an alliance with the religious Right, or more generally, to attempt to use populism to separate liberalism from democracy.

The present book is concerned with the Straussian filiation of neoconservatism and thus of what Drury regards, without adducing evidence, as the "dominant ideology" of the Republican Party (p. 137). The book straddles somewhat uneasily a boundary between political commentary—here of the dangers Straussian-influenced neoconservatism poses to liberal democracy—and criticism of Strauss as the source of those dangers. The introductory first chapter recounts the visibility of some of Strauss's students and followers in American government and Republican politics. The second and third chapters are the intellectual core of the book, concerned, respectively, with Strauss's complex relationship to his "Jewish heritage" and his significant intellectual debts to Carl Schmitt and Heidegger. The fourth chapter rehearses and criticizes the political teachings of Harry Jaffa, Allan Bloom, and Willmoore Kendall as followers of Strauss who, like their master, exhibit contempt for much of the modern liberal enterprise. The final chapter on neoconservatism is devoted to Irving Kristol as the most politically influential continuator of Strauss's "radical and reactionary" (p. 161) perspective.

For Drury the "key to understanding Strauss rests in comprehending his efforts to reconcile" his legacy from Judaism with his legacy from German philosophy, "a posture of resignation in the face of the injustice and tragedy of life," with "a political romanticism that gave him an aversion to liberal democracy" (p. 33). The author's interpretation of the former topic is, for me, the most intriguing part of the book. Drury maintains that for Strauss the "Jewish problem" is unsolvable, for neither assimilation, nor liberal democracy, nor political Zionism can eliminate the need for Jews who would live as Jews to live with persecution. She goes on to argue that the Jews for Strauss are an essentially religious and thus an essentially antimodern people, which makes the universalism, secularism, and individualism of liberal democracy profoundly antithetical to their identity. She views Strauss as a Jewish nationalist who sees liberalism as a "special threat to the uniqueness of peoples" (p. 42). In the most acute discussion in the book (pp. 54–64), Drury criticizes Strauss's understanding of Judaism as excessively political. Moreover, his notion of the philosopher-prophet as standing beyond good and evil is a secularized version of the teachings of Kabbalistic mysticism. Strauss's philosopher-prophet is a "secular messiah" who brings divine law from the "abyss of nihilism," engendering, for Drury, a terrifying political ideal that "leaves us entirely at the mercy of cunning and ambitious priests" (pp. 63–4).

The chapter on Heidegger and Schmitt is less satisfactory, primarily because Drury's treatment of Heidegger is superficial. While Strauss shares Heidegger's abhorrence of modernity, he regards Nietzsche as superior in his grasp of both the disease of and the cure for modernity. Drury accepts uncritically the opinion that Heidegger's teaching is best understood as individualist and nonconformist, and she relies here (as she does in her discussions of Plato and Nietzsche) on conventional interpretations. She believes that Strauss shares with Heidegger an apocalyptic sense of the crisis of modernity, a belief that philosophers should influence those in power, a radical aspiration to begin anew, and a "self-referential" (p. 77) way of reading the works of the philosophers. Drury simply accepts the received opinion that Strauss and Heidegger distort the texts of the philosophers without examining this issue herself.

According to Drury, Strauss adapts and radicalizes Schmitt's critique of liberalism in his own critique of American liberalism. He accepts and accentuates Schmitt's rejection of liberalism as antipolitical and agrees that the political is the necessary opposition of friend and foe. Strauss embraces the political, celebrating it as superior to a "depoliticized" world (p. 92). Against Schmitt, Strauss proposes to "re-theologize the political" to inspire the kind of loyalty and self-sacrifice essential to politics (p. 93). Finally, Schmitt and Strauss reject universalism and know and celebrate only the particular, the nation, the people; thus, in their own ways, they are eminent postmoderns (p. 95).

The author refers to most, although not all, of Strauss's published writings, but she engages in very little close reading of any of them. She excels at finding passages that shock the sensibilities of liberals, but we are entitled to ask whether Strauss's works are as easily understood and as politically transparent as Drury believes. For her, Strauss's message is "loud and clear to anyone who is patient enough to read carefully and to the end" (p. 49). Yet, there remain a number of mutually incompatible readings of Strauss, produced by some of his best students. Drury's reading is relentlessly political. Evidently, she finds nothing in Strauss's books that surprises, puzzles, astonishes, or delights, and she has no use for the claim made by students of Strauss that he is a *zetetic* or authentic skeptic. She reads Strauss narrowly, like critics of Heidegger who read his work exclusively for Nazi resonances and are oblivious to any thought for which this correlation cannot be readily established. I am inclined to think that Strauss's works, to borrow a remark Derrida made concerning Heidegger, have immense reserves, that they do not, contrary to Drury, foreclose inquiry.

Drury is a liberal who is not unsympathetic to conservatism. She recognizes, to her credit, that liberalism is vulnerable because it opposes the "human proclivity" (p. 175) for homogeneous social identity. It is ultimately fragile, a "heroic achievement against nearly insurmountable odds" (p. 176). Liberalism is exposed as well to the difficulty that it is not a glamorous political position. It regards politics as a "practical and prosaic affair—not the stuff of poetry and martyrdom," for it "is not the business of politics to intensify life, to give it meaning, mystery, and magic" (p. 43). Defenses of liberalism will seldom satisfy political romantics of the Left and Right. Drury's reading of Strauss has merit because it allows us to recognize more fully the gulf both wide and deep between liberalism and political romanticism.

The New Golden Rule: Community and Morality in a Democratic Society. By Amitai Etzioni. New York: Basic Books, 1996. 314p. $27.50.

Jeff Spinner-Halev, *University of Nebraska-Lincoln*

Amitai Etzioni has written this book to clarify his version of communitarianism. He was partly prodded by critics of his earlier books to write this new one, but he explicitly avoids lengthy scholarly disputes, even as he drops names left and right. Indeed, this book often reads as a manifesto (though a rather long one). Keep your seat belt on if you read this book: Etzioni often moves from topic to topic with lightning speed. There is a new heading or subheading every couple of pages, sometimes every page (and occasionally every paragraph or

two). The moral problem with lawyers defending the indefensible is discussed in one and one-half pages; seat belt, speed limit, and helmet laws are discussed in three paragraphs, while Habermas gets two.

Those familiar with the more popular communitarian arguments will not find much new here. Many of his policy prescriptions and moral urgings are familiar and usually sensible. Those in the dark, however, and who are curious to know how communitarians want to change society will profit from this book. (Unless they want to look up the communitarian platform on the World Wide Web; Etzioni conveniently lists in the preface the Web address of the Communitarian Network, which he founded. Or you can call: Eztioni lists the toll-free number of the network in an endnote so you can order their position papers.)

The core of Etzioni's argument is that our society should establish a new golden rule: "A good society requires an order that is aligned with the moral commitments of the members" (p. 12). Figuring out the meaning of these moral commitments is tricky business, of course, and Etzioni sometimes avoids it. Indeed, he drops the idea of moral commitments when he later defines the new golden rule as one in which order "is balanced with autonomy" (p. 165). Much of the first half of the book reads as a defense of the later version of the golden rule: We must learn to balance autonomy with order. Some societies, like China, emphasize order too much and crush autonomy when they do so. Other societies, like the United States, emphasize autonomy too much at the expense of order.

Etzioni claims that American society is now characterized by excessive individualism. The accusations are familiar. Individualists want rights, not responsibilities; they criticize metal detectors in airports, drug testing of school bus drivers, and roadblocks to catch drunken drivers. Individualists' highest value, and sometimes their only value, is the right to choose. Etzioni, however, thinks that this position ignores the social attachments that are crucial to a good society. Yet, he thinks that social conservatives who have a bad case of nostalgia and want to revert to the supposed utopia of the 1950s are trying to resurrect an oppressive social and political structure. Etzioni calls for a resurrection of communities, communities that support both autonomy and order and that respect the dignity of all their members. He wants a community of communities, in which people have multiple memberships, so no one membership is so stifling as to stamp out one's autonomy. If you dislike the direction of a community, and you cannot change it, then you can simply devote your energies to another community.

While many liberals may be suspicious of a call for more order in society, what Etzioni often means by order is more familiar and palatable. He wants people to curb their desire to act selfishly. He contends that people are born self-interested, but over time a set of values that recognize our social nature and the communities in which we live can be encouraged and instilled within us. There is a tension, Etzioni claims, between the "pleasure principle and the moral voice" that is within us all (p. 170).

Etzioni argues that we should not rely mostly or only on laws to ensure order. Rather, the good society must be guided by shared moral values that condone certain behaviors and condemn others. Etzioni does not dismiss the importance of laws but argues that values must be shared for a moral and orderly society. These moral values can be revealed and established through a "megalogue"—a moral dialogue that takes place in local and national forums. That these dialogues not be only national is important for Etzioni. He argues that local communities are important places to nurture and hear the "moral voice" needed in megalogues.

Etzioni admits that megalogues are a bit fuzzy, and he tries to deflect the criticism that he is actually long on moral hand-wringing and short on policy prescriptions by arguing for the importance of moral dialogue. This argument is unnecessary, though, for the second half of the book has many policy prescriptions. He argues for government funding for community organizations, the protection of public spaces, community courts and community policing; he insists that schools should be character-building agents, and he calls for a variety of ways to strengthen marriage, among other proposals.

Etzioni wants to drive a middle ground between social conservatives and liberal individualists. By leaving aside scholarly debates, however, Etzioni does not note how many liberals have responded to the communitarian critique in the last fifteen years or so. Many liberals today commonly argue that the state cannot be neutral on the good life, and that good democratic citizens have certain virtues which are best inculcated in schools, the family, and all kinds of local communities. By agreeing with these arguments and by holding on firmly to the idea of autonomy, Etzioni makes arguments that are distinctive from many liberal positions only when it comes to the details. It is hard to know exactly where these differences arise, since Etzioni rarely points them out or says how his arguments differ from liberals in any substantive way. Those who want to know how the theoretical differences between liberals and communitarians lead to policy differences will want to look elsewhere for guidance. Those who are looking for a primer on communitarian politics and policies will find this book useful.

Corrupting Youth: Political Education, Democratic Culture, and Political Theory. By J. Peter Euben. Princeton, NJ: Princeton University Press, 1997. 272p. $55.00 cloth, $18.95 paper.

Susan D. Collins, *Southern Illinois University at Carbondale*

In this rich and often moving study of the political and intellectual life of classical Athens, Peter Euben makes an impressive case that there exists a fundamental kinship between democratic politics and the philosophic activity of that most famous corrupter of youth, Socrates. Euben is aware that he defies a certain orthodoxy connected with Platonic political philosophy: the view that there exists an ineradicable tension between politics and philosophy in general, and between democracy and philosophy in particular. Euben's redefinition of these relations is more than an academic exercise. Moving gracefully between America at the millennium and classical Athens at its zenith and nadir, he seeks to shed light upon the debate over education in the contemporary "culture wars." He enlists the aid of such classics as Thucydides' history, Aristophanes' *Clouds*, Sophocles' *Antigone* and *Oedipus Tyrannus*, and Plato's *Apology*, *Gorgias*, and *Protagoras*. In illustrating the relevance of these great works of the Western tradition to our times, Euben sides not with the canon's usual defenders, whom he calls "conservative canonists," but with those whom he identifies as these canonists' "multicultural critics." Although he is not simply a partisan of the latter or a foe of the former, he nevertheless condemns the "didactic cooptation" and one-dimensional readings of the Greek texts by conservatives, and he proposes that multiculturalists, in their quest to challenge "the dominant voices in our culture," take inspiration from the "deeply radical impulse" of the very tradition they

challenge (p. xiv). In his own sometimes admittedly idiosyncratic reading of the works—he speaks, for example, of "my" Socrates of the *Apology*, and of his "against the grain" and "democratic" readings of Plato's dialogues (pp. 54, 246, 204, 226–7)—Euben offers much evidence for his proposition. He draws links between Athenian political practices and Socratic critique, discovers common ground between Aristophanes and Socrates in the concern to preserve the best traditions of Athenian democracy, and brings to life a multivocal and paradoxical world of politics to which only democracy and Socratic dialectic, properly understood, can do justice. Euben's case for the relation between democracy and Socratic political philosophy forms the serious core of his book and raises compelling questions, especially about the possibility of a "philosophic politics" and about the grounds of the Socratic way of life.

In its essence, Euben argues, Athenian democratic culture constituted a form of political independence and self-consciousness, as well as theoretical self-awareness. The "daring" typical of the Athenians, and celebrated in the "legitimating story" of Athenian democracy, the naval battle at Salamis, had its source in a desire for liberation from all forms of authority, political and theoretical. Even as they sought empire over others, the Athenians undertook to bind power within their domestic sphere through such political practices as scrutiny and critique (see especially Euben's discussion of *dokimasia* and *euthunai* in chapter 4), and in contrast to the Spartans, who were deferential in the face of what they saw to be "natural or divinely ordained necessities," the Athenians conceived of the world as "moments of opportunity for the exercise of political and theoretical power" (pp. 80–1). Yet, "the cultural rhythms" initiated by Salamis, which were the basis of Athens's freedom, proved also to be the source of its decline into tyranny; more generally, the Athenian rejection of all authority—of that which is to be revered without question—leads, if left unchecked, to the political and linguistic chaos described by Thucydides in his account of the Corcyrean Revolution. Euben seeks to show that the problem of maintaining political as well as epistemic order while preserving the freedom to criticize that order is the stuff of Aristophanic comedy and Sophoclean tragedy and lies at the heart of what he calls the "Socratic project" (p. 88). Socrates' philosophic activity, he maintains, involves both the elaboration of "an already existent theoretical impulse" in Athenian democratic culture and the "grounding [of] that impulse in the traditions it threatens to destroy" (p. 88). On Euben's reading of the *Apology*, *Gorgias*, and *Protagoras*, therefore, the deepest purpose of Socrates' dialectical confrontations is not, finally, the refutation of the Oracle at Delphi but the promotion of a "philosophical citizenship" and "politicized philosophy" in accord with the finest traditions of Athenian democracy (p. 107).

Central to Euben's argument is a theme familiar from his earlier work: the importance of what he calls "the tragic sensibility" to clarity about political life and human existence (see especially *The Tragedy of Political Theory, The Road Not Taken*, 1990). In his present study, Euben turns to Sophocles' *Antigone* and *Oedipus Tyrannus* to lay out most fully the tragic view undergirding his position: The world into which we are thrown, he suggests, is one of multiple voices, meanings, differences, and oppositions we can never fully resolve or overcome; this is a world which ultimately eludes human comprehension and which, lacking a legitimating myth, will naturally devolve into chaos. Euben thus counsels that we create a political and philosophic discourse that is as inclusive as possible of the world's many voices, differences, and oppositions, at the same time as we guard against that same discourse becoming "hegemonic." The promotion of such a democratic order is the proper end of political education, and Socratic dialectic is the political-philosophic vehicle upon which we may reach this end. In the midst of our rather precarious situation in the world, then, Euben and his Socrates, as a "paradigmatic educator of a democratic polity," offer hope (pp. xi–xii; cf. chaps. 2 and 9). Or do they? Euben himself concludes on a tragic note, calling the "idea" of a "philosophical democracy" "a noble legacy *and* a seduction, the only aspiration worthy of political educators and sheer vanity" (p. 265). The tragic sensibility he has recommended as the true view and as a necessary "antidote" to misconceived and typically modern attempts to master nature and rationalize human affairs is also revelatory of the ultimate futility of even the best efforts to find a stable home amid the fundamental chaos. Our hopes, it seems, may be dashed at the very moment they are raised, the nobility of our aspiration having been undone by our realization of its vanity.

By both its critics and sympathizers, Euben's *Corrupting Youth* will be admired for its mastery of the secondary literature, facility with old and new discourses, and lively readings of old works. The book's most important contribution within and without the scholarly community, however, concerns human education. The first part of its subtitle notwithstanding, Euben's return to classical Athens is about more than *political* education and *democratic* culture, strictly speaking. The larger frame of the book, to which the subtitle's reference to political theory points, is the rebirth of efforts to understand the whole of human life, its moral and political dimensions, longings and hopes, evils and goods. As Euben's study attests, these efforts cannot find direction absent a quest to understand the whole within which we exist. The sheer scope and quality of his work should raise the level of contemporary discourse and inspire especially the young to undertake the inquires that an understanding of human life seems to demand. Yet, if that whole and that quest are such that they will always elude human comprehension—if, therefore, we can know about the world only that it is fundamentally tragic—then the possibility of a truly philosophic politics remains all the more unlikely, and the grounds of Socrates' own activity and way of life are all the more mysterious than even Euben would seem to admit.

Tradition and Modernity: Philosophic Reflections on the African Experience. By Kwame Gyekye. New York: Oxford University Press, 1997. 338p. $45.00 cloth, $19.95 paper.

Crawford Young, *University of Wisconsin-Madison*

Kwame Gyekye is an ambitious scholar. A philosopher sometime of the University of Ghana, he undertakes a far-reaching excursion into the philosophic dimensions of the core dilemmas confronting the contemporary African state. In this endeavor, he seeks to marry current debates in moral philosophy with the empirical literatures relating to the African condition.

The resulting study, largely completed during a fellowship year at the Woodrow Wilson International Center for Scholars, covers a vast terrain. His acknowledgments of those who provided critical comments on the manuscript list an unusually distinguished roster of luminaries: from Amitai Etzioni and Will Kymlicka to Seymour Lipset and Ernest Gellner. Interestingly, none of them are Africanists, an intellectual domain in which the author understandably felt solidly implanted.

Gyekye, after locating himself in the broader realm of

philosophic discourse as a "moderate communitarian," explores the issue of whether there is an "African philosophy." Although he has no sympathy for Afrocentric framings of this issue, his analysis does presume a broadly similar sub-Saharan cultural heritage that permits and validates the continental perspective he adopts. He examines in turn the relative saliency of individual and community in African political thought, the (mis)uses of a presumption of ontological primacy of community in the African heritage as justification for socialist orientation, the problematic of ethnos and nation in the African state system, the elusive quest for legitimacy, the stain of corruption, and the master binary of "tradition" and "modernity" in understanding Africa.

Gyekye is always interesting, frequently combative, and sometimes unpersuasive in his analysis. One can only admire the systematic invocation of normative inquiry into such a broad range of questions. The scope of the ambition carries the inevitable corollary of the unevenness of the result.

The vertebral hypothesis of a general African culture and heritage encounters the contradictions of the evidence. The author, perhaps necessarily, relies heavily upon evidence drawn from his own Akan background; throughout, there is an inarticulate major premise that Akan patterns are paradigmatic for the continent. The many Akan proverbs he cites are indeed redolent of a practical but profound wisdom. But can they stand for Africa writ large?

Intriguingly, two other major contributors to the idea of an African philosophy drew essentially from the same cultural resource, though reaching different conclusions: Willie Abraham (*The Mind of Africa*, 1962), and K. Anthony Appiah (*In My Father's House: Africa in the Philosophy of Culture*, 1992). Indeed, the original contributor to the notion of a distinctive African cosmology, the Franciscan missionary Placide Tempels (*Bentu Philosophy*, 1948, not cited), likewise projected a Luba-Katanga template upon the continent.

An example of the problems arising is the argument that democratic elements are embedded in the cultural heritage. One can indeed identify important elements of consensus and consent in Akan and many other precolonial African political forms: the practice of destooling the autocratic ruler, in the case at hand. But the germs of democracy are difficult to detect in military conquest states, such as the Chaka Zulu kingdom, in the slave-raiding states, such as those of Rabeh or Msiri, or in the theocracies issuing from the Fulani Islamic revolutionary states in inner West Africa in the 18th and 19th century.

The use of the Akan category also bears reflection. Initially a purely linguistic classification of closely related languages, the more operative political identities were located one level down (Asante, Fante, Nzima, Akuapem, and others in Ghana, Baoule and some other Ivory Coast ethnic communities). The Akan category, however, has become something more than a purely external classificatory label in recent times. Here a larger point needs underlining: Identity politics in postcolonial Africa operates in complex ways; analysis must incorporate the multiplicity of levels, contingency, permeability, situational shifts, and evolving dynamics of identity construction and transaction.

The effort to capture in a reductive notational schema the multifaceted nature of ethnicity in politics, and its interaction with state institutions in quest of legitimacy and evolving discourses of "nation," is bound to fall short. Gyekye postulates the existence of a number of genuinely homogeneous nation-states (actually on close inspection relatively rare), labelled N1. Juxtaposed to these are the African norm of multinational states (N2), wherein state affective legitimacy rests upon a weakly articulated sense of "nationhood." The normative path (nation-building) is toward N3, where a fully constructed nation is in place, and finally N4 (the "mentanation"), where the "toxic effects" (p. 103) of ethnicity are finally purged from the system. This rather evolutionary construct sits uneasily with the actual dynamics of social construction on the ground.

Gyekye takes a critical look at the argument—first advanced by the founding fathers of African socialism—Julius Nyerere, Kwame Nkrumah, and Leopole Senghor, among others that socialist doctrine could draw upon the powerful heritage of an essentially communal cultural heritage. The author persuasively demonstrates that the African tradition contained individual as well as communal elements, and that the communitarian reading was an obfuscation. This critique, though not entirely novel, does help explain the shabby, motheaten nature of "really existing socialism" in Africa by the 1980s. But to be fair, one need also acknowledge that similar pathologies of bloated and predatory states, oversized and underperforming, characterized much of Africa in the 1970s and 1980s, with or without benefit of socialist orientation.

An author of such large ambitions can hardly expect all readers to nod amen at every page. But the book deserves many readers, who are guaranteed stimulation in the arguments advanced, and admiration for the wide-ranging erudition of Gyekye. He makes a valuable contribution to joining philosophic reflection to political analysis.

Bodies of Law. By Alan Hyde. Princeton, NJ: Princeton University Press, 1997. 266p. $49.95 cloth, $16.95 paper.

Thomas F. Tierney, *Concord College*

Alan Hyde, a law professor at Rutgers, has read widely in the literature of body politics and has brought this scholarship to bear upon legal discourse. His intended audience is primarily students and professors of law, as well as attorneys and judges, and in regard to his audience his goal is to challenge the "cold metaphors" through which the law approaches the body, and which "proliferate *so many* discursive constructions of the body that none may ever seem 'natural' again—especially not a body's gender or race" (p. ix). Toward this end Hyde employs the writings of Michel Foucault, Judith Butler, Julia Kristeva, and many others, but he admittedly engages these writers in an "extremely light" manner (p. vii). Hyde's objective is not to use case law to support particular readings of these theorists but, rather, to use these theorists to open the law to multiple readings.

As for the larger audience of social and political theorists with whom Hyde would also like to "advance a dialogue" (p. vii), those who are hostile to constructivist claims may become more amenable to these upon exposure to the myriad legal constructions of the body that Hyde examines, especially since the powerful consequences of these constructions are not easily dismissed (p. 262). Theorists already indebted to writers like Foucault, Butler, and Kristeva are presumably not in need of any further persuasion about the discursively inscribed nature of the body, and given Hyde's allusive treatment of such writers, these theorists will not find much to argue with in this book. Nevertheless, *Bodies of Law* is a valuable resource for those already attuned to the discursiveness of the body, in that it provides many examples of body constructions that could bear further theoretical elucidation.

The book is divided into fifteen chapters, each of which examines a particular legal construction of the body, and these chapters are grouped into three divisions according to

the type of cases involved. For the purposes of this review, the divisions can be distinguished by the dominant theoretical influences that Hyde employs in interpreting the cases. The first part, "Regulation," relies primarily on Foucault to interpret cases involving the sale and donation of bodily fluids and parts, abortion rights, and impaired reproductive capacity (both as a condition of employment and as the result of accident). Part 2, "Desire," examines cases of compelled and suppressed bodily displays, body searches of prisoners and suspects, the measurement of penile arousal as a condition of reemployment, and the forced medication of prisoners. Many of the cases in this section turn on gender distinctions made by the courts, and Butler's writings figure prominently here. The third part, "Abjection," uses Kristeva's writings to interpret cases involving urine testing, contested racial identities, mandatory vaccination policies, and the exclusion of offensive bodies from public settings as instances of public purification rituals.

Through this encyclopedic examination of case law, Hyde convincingly demonstrates that any strategy for protecting bodies from economic or public intrusion which relies upon constructions of the body as property, a privacy interest, or an inviolable realm of individual autonomy can do so only by further exposing bodies to intrusion by these same forces. To claim property rights in the body, for instance, is to "inscribe them into the normal regulatory apparatus of consumer and market society, a world of contracts and compensation, domination mystified as economic exchange" (p. 76; also see p. 93). Similarly, the recognition of privacy interests in the body, as in regard to abortion rights (pp. 82–6) or the Fourth Amendment right against unreasonable searches (pp. 154–86), only places those interests in a balance with the public interest that threatens to outweigh them. "The paradox" of the dominant legal strategies, Hyde claims, "is that the body is protected in contemporary law only by means guaranteed to subjugate it" (p. 200).

Although his task is primarily deconstructive, Hyde mentions, but does not develop in detail, an alternative "prototype for the body of law that should be constructed much more frequently" (p. 97). This preferred construction is a narrative that presents the body not as an abstraction that is common to all individuals but as a particular body in specific circumstances with which others can empathize. Hyde is not excessively sanguine, however, about the prospects of even this strategy in the existing legal environment. He applauds one case in which a woman was able to win compensation for loss of consortium through the use of such a personal narrative (pp. 100–3), and he decries another in which a woman was forced to submit to a vaginal search for illegal drugs because of her relation to her husband, a suspected drug smuggler (p. 170). In order to foster the deployment of such body narratives in "the service of human coexistence, understanding, and empowerment" (p. 235), Hyde concludes the book with a brief discussion of an alternative, utopian legal perspective that he calls "a body fantasia." This perspective would "develop a discourse of bodily presence not absence, pleasure not pain, wholeness not fetishism, relations not isolation, connection not display, fusion not difference, incorporation not abjection, play not control" (p. 263).

Hyde is clearly aware of the obstacles that such an alternative legal discourse faces in liberal judicial systems. Indeed, throughout his text he inadvertently indicates just how difficult such a transformation would be to accomplish. Although he repeatedly acknowledges that there is no physical, material body available beyond or underneath discourse (e.g., pp. 6, 187, 240), when Hyde aggressively challenges a judicial decision regarding a specific bodily intrusion, he often comes close to condemning the court for ignoring just such a "real" body (e.g., pp. 85, 157, 243). This is not to criticize Hyde for inconsistency but to point out how effectively the deep-seated adversarial nature of American juridical discourse constrains the introduction of playfulness and contingency into the law. Even those lawyers who have learned Hyde's lesson—that any claim they make about the body, even a narrative of the sort that he favors, is an arbitrary and contestable construction they are employing for instrumental purposes (pp. 6, 235)—will be reluctant to acknowledge this "provisional and artificial quality" (p. 4) in the heat of a legal contest.

In this attempt to provide legal practitioners with a wide array of body constructions from which to choose, Hyde has employed an impressive range of contemporary theory that extends far beyond the primary theorists mentioned in this review. Yet, *Bodies of Law* should not be approached as a law professor's attempt to do theory, which would only subsume Hyde's project in quarrels about the finer points of interpretation of the many theorists he employs, or mire it in debates about the ontological status of the body. Rather, *Bodies of Law* should be approached as a poignant reminder of the various ways that political theory is inscribed in bodies through the law and as an invitation to theorists to join in challenging and expanding the prevailing parameters of the juridical construction of the body.

Creatures of Prometheus: Gender and the Politics of Technology. By Timothy V. Kaufman-Osborn. Lanham, MD: Rowman & Littlefield, 1997. 292p. $57.50 cloth, $22.95 paper.

Ruth Abbey, *University of Western Australia*

Finding a title for this book cannot have been easy. One of Kaufman-Osborn's major concerns is to challenge ways of thinking about the relationships between human identity and the things we make. He is suspicious of the term *technology*, finding it too gross and essentialist to make much sense of the many different sorts of things humans make. He also detects in it traces of the Cartesian approach to identity that he criticizes relentlessly throughout the book for positing an autonomous, bounded self that works on and controls a mindless, inherently meaningless, and endlessly malleable world of objects. The author's insistence that humans are both made by and makers of the things we create means, however, that the eponymous Prometheus is himself both creature and creator.

To threaten any firm separation between subject and object, Kaufman-Osborn's preferred term for humans is "artifact" and for objects it is "artisanal artifacts." He stresses that the arrows of influence run between both sorts of artifact. Adapting Elaine Scarry's dialectic of projection and reciprocation, he offers an account of the ways in which embodied humans engage with and are shaped by the world of artifacts which precedes and survives us. When humans project themselves onto the world, we create things that relieve or reduce the effects of our physical and intellectual limitations. Knitting a pair of gloves for winter is an example of projection. Projection removes or mitigates the effects of physical vulnerability and thereby frees humans, releasing potential for more and different action. With warm hands I can build a shelter to protect my whole body from the cold, which in turn will enable me to engage in other activities. Through the work of projection and reciprocation we both acknowledge and transcend our needs as embodied creatures.

The author's second major concern is with how gender is

implicated in the production of artisanal artifacts. Kaufman-Osborn portrays gender as an artifact, as something produced partly through the different things that men and women typically make and the different tools used in these processes. The different roles of men and women in reproduction is an important, but not unique, example of the way working produces gender. His reciprocal proposition that artisanal artifacts are also gendered requires a more sustained argument to be convincing. The claim, for example, that men's tools are thought of as technology but women's are not is unpersuasive to this reader.

The third important artifact Kaufman-Osborn identifies is language. Language classifies and identifies things but is not unconstrained in this; certain artifacts stubbornly resist some classifications. As this suggests, he resists the deification of language he sees in some strains of postmodern thought. Moreover, he wants to rescue the idea of extradiscursive experience from linguistic reductionism by insisting that things can be known outside of language, that the body can feel things either without or prior to their being articulated.

These three sorts of artifact—human, artisanal, and linguistic—can be disintricated only in thought; in reality they are always connected in some way or other, and any one of three modes of agency can alter these relations. Underlying all this is Kaufman-Osborn's concern with the presence and power of the unreflected background of our daily practices. He strives to remind us how influential the things we routinely take for granted are; we usually fail to notice their significance until they break down or confound our expectations in some way. He also wants to draw attention to how much work is congealed in these everyday artisanal artifacts.

There is obviously a good deal of Marxian influence in the author's approach and concerns; more in fact than is signalled by the book's explicit references to Marx. Nietzschean holism is also invoked to defend the premise that meaning and identity are always relational. The pragmatism of Dewey and James further informs many of this book's arguments. Inspired by these traditional sources, Kaufman-Osborn takes issue with contemporary theorists, such as Donna Haraway, Judith Butler, Iris Marion Young, and Richard Rorty.

While the first and second parts of the book expound Kaufman-Osborn's theoretical approaches, the third explores these through stories: a section from the *Iliad*; Mary Shelley's *Frankenstein*; and two stories about Chernobyl; one told by a Soviet nuclear physicist and the other by the novelist Christa Wolf. Given the book's theoretical dedication to daily life, it might have been more consistent, as well as more useful and interesting, to devote more of these pages to showing how the author's preferred way of talking about things sheds new light on contemporary issues. He claims, for example, that the gun lobby's observation that "guns don't kill people, people do" presupposes the Cartesian paradigm of use, and the development of this type of engaged argument would have been welcome.

When I was reading this book, two pubescent boys in Arkansas murdered four of their female peers and a female teacher. This provided graphic illustration of Kaufman-Osborn's insistence upon the mutual constitution of identity, gender, and objects. These boys grew into a world saturated with guns; being shooters had been part of their identity from a remarkably young age. Conversely, the media have made no mention of what skill with guns the five dead women had, from which it might be inferred that proficiency with guns was not an integral part of their development. Shortly after, however, a young girl shot herself at school in Chapel Hill. Interpreting events like these, along with others less tragic from ordinary life, would have improved and enlivened this third section of the book.

John Locke and the Origins of Private Property: Philosophical Explorations of Individualism, Community, and Equality. By Matthew H. Kramer. New York: Cambridge University Press, 1997. 347p. $59.95.

Eduardo A. Velásquez, *Washington and Lee University*

John Locke's vision of the prepolitical and political condition of human beings, of the inherent powers by which so-called individuals appropriate and master the gifts of nature through labor, and of the use of those powers to create governments for the protection of their persons and property stands at the fountainhead of liberal political philosophy. This vision is no mere abstraction. Locke's doctrine of rights, chief among them the right to the fruits of our labors, is arguably the very soul of the American republic, if not of liberal republicanism generally. In a study that is bound to startle most students of Locke, Matthew H. Kramer proposes a reinterpretation of the central pillars of the Lockean and liberal edifice. The arguments of *John Locke and the Origins of Private Property* supposedly demonstrate that the "apparent individualism of Locke's labor theory of property is rooted in a thoroughgoing communitarianism" (pp. xi, 3). But in coming to terms with Locke's communitarianism, the reader should not think that Kramer's study "tries to blend or junction . . . communitarian elements and individualistic elements in Locke's thought." Instead of "merely being coupled with individualism," Kramer argues that "the communitarianism in the Lockean state of nature would engulf individualism and would employ it for communitarian purposes. Individualism was never absent, but it was starkly epiphenomenal" (p. x). Moreover, Kramer does not side with the likes of Charles Taylor, Jean Bethke Elshtain, Michael Sandel, Alasdair MacIntyre, and other communitarian critics of liberalism (p. 3). Kramer engages Locke on his own terms in order to show that his "atomism was itself utterly collectivistic" (p. 4). The "rights of the individual in the state of nature were but facets or offshoots of humankind's collective ideals and purposes" (p. 3).

Locke, however, is not a self-conscious communitarian. "Having taken his axioms as givens," Kramer writes, "we proceed to observe that he committed some downright blunders in the argumentative uses to which he put those axioms" (p. 12). By reasoning through Locke's "blunders," as Kramer purports to do, interpreters can now appreciate the extent to which the cherished individualism bequeathed to us by the author of the *Second Treatise* is viable only by taking seriously Locke's unconscious collectivist or communitarian "ideals and purposes." Among the most serious mistakes Kramer notes is Locke's attempt to "derive the norm of political equality from a thesis about physical and mental equality" (p. 38). To demonstrate Locke's faulty reasoning, Kramer turns to a familiar passage in the *Second Treatise* (II, p. 4), where Locke argues that all power and jurisdiction is reciprocal, "there being nothing more evident, than that Creatures of the same species and rank promiscuously born to all the same advantages of Nature, and to the use of the same faculties, would be equal to one amongst another without Subordination and Subjection." Locke here is interpreted to mean that "human beings patently all belonged to the same species, with roughly the same endowments; hence, Locke insisted, they were bound to refrain from wielding any dominion over one another" (p. 38). In a lengthy chapter devoted to "Equality Unlocked" (pp. 37–89), Kramer then

proceeds to show that the "fact" of human equality does not issue in the "norm" that all human beings should treat one another as equals, at least not without having those "norms" buttressed by communitarian presuppositions. Locke's blunder is further accentuated when we consider that his devotion to equality is intimately bound to his defense of freedom (pp. 46–8).

Having demonstrated how Locke's premises about human equality and freedom do not necessarily issue in a respect for natural rights, much less than in a community of mutually respecting, tolerant, and civil human beings, Kramer turns to Locke's theory of property. "With his recountal of the paths by which human beings qua autonomous agents could procure things as owned possessions," Kramer concludes, Locke "revealed quite inadvertently that autonomous agents would strip themselves of autonomy by dint of exercising it" (p. 91). Although "the maximizing of overall welfare could sometimes occur through the maximizing of every individual's welfare," a position we ascribe to Locke and to liberal capitalism generally, Kramer argues that the public good is not necessarily or consistently served by the emancipation of individual acquisitiveness. Arguments which "pose their theses as theses of overall production, must refer to the good of humanity rather than the good of all persons qua individuals" (p. 125). Locke's implicit communitarianism becomes explicit by our noticing that "efficiency-centered line of justification for private ownership must advance its refrain in a collectivist key, by pointing to the implication of private ownership for the common weal and by having to disregard the implications of private ownership for the standing of each person considered separately" (p. 126). Locke's supposed claim on behalf of nature's bounty does not rescue his individualism (pp. 102–6).

Kramer's thesis invites scholars to consider important questions and problems that abound throughout the *Second Treatise*. But it is likely to strike the reader that those questions are not necessarily the ones Kramer himself entertains, to his detriment and ours. Consider only one. Conspicuously absent throughout Kramer's work is a necessary appreciation of the rhetorical art, of the differences between the demands of persuasion and logical consistency, of the manner in which Locke fashioned arguments in the context of the contentious political climate of his time. Now, to say that Locke was a politick writer makes one neither a disciple nor a dupe of Leo Strauss and other exegetes of esoteric writing (p. xi). But a short experience in the world can teach the importance of tact. And tactful Locke certainly was. Not every premise advanced at, say, the beginning of the *Second Treatise* is sustained by conclusions that emerge later, and of this Locke is more than aware. For example, the argument in chapter II, section 6, of the *Second Treatise*, according to which human beings are the "workmanship of one omnipotent, and infinitely wise maker" and thus obliged not to destroy themselves or any other person, is very difficult to reconcile with the argument in chapter V, section 27, according to which "man has a *property* in his own person."

It is well beyond my scope here to explore what these apparent contradictions mean. I only point out that no reading of *Second Treatise* will illumine Locke's purposes without attention to tensions Locke *himself* weaves throughout his work. Locke is aware that civil society does not eradicate the "inconveniences" of the state of nature. As to the benefits of commercial society that are the fruit of emancipated acquisitiveness, there is good reason to suspect that the state of nature serves as an apt metaphor for the transactions of the "marketplace." At the center of Locke's teaching one discovers less than edifying premises about the motives that propel humans to action. That our examination of these motives uncovers a necessary admixture of virtues and vices, that the provisions of the Deity are less than adequate to the exigencies of our individual and collective well-being, that the wants of individuals are not in harmony with those of the community but must be made so, are only a few of Lockes' teachings obscured or ignored by the failure to engage Locke's rhetoric. Consequently, *John Locke and the Origins of Private Property* turns out to be an intriguing collection of Kramer's own "philosophical explorations of individualism, community, and equality"; much less is it an interpretation that sheds light on the methods, aims, and character of Locke's political philosophy.

Piety and Humanity: Essays on Religion and Early Modern Political Philosophy. Edited by Douglas Kries. Lanham, MD: Rowman & Littlefield, 1997. 286p. $68.00 cloth, $24.95 paper.

Peter Augustine Lawler, *Berry College*

This excellent book is a most impressive and thorough defense of the relationship between "early modern political philosophy" and "the theologico-political problem" first put forward by Leo Strauss. Its fundamental premise concerns the power of ideas, especially political ideas, in shaping history. The modern world is largely the creation of political philosophers who viewed revealed religion as their chief adversary. Christianity and Judaism, owing to human weakness, could not be completely destroyed, but they could be refashioned in a way compatible with demands of liberal, democratic government. The rule of God and priests could be replaced by self-rule. The modern goal, finally, is autonomy, to which even prosperity is subordinated.

For most of this book's authors at least, it is clear that Machiavelli, Bacon, Hobbes, Spinoza, Locke, Montesquieu, and Rousseau were all freed by reason from being moved personally by the challenge of revelation. They are best understood as using reason to free themselves and the world from the thrall of the Bible. But the dominant scholarly view remains that these philosophers were really believers or "sincere theists," unable to transcend through thought the religious prejudices of their time and place. Because Strauss himself did not convince scholars, more evidence, the authors believe, is required.

Andrea Ciliotta-Rubery's opening chapter is the book's most modest. In light of Strauss's recovery of Machiavelli's intention to manipulate Christianity for political purposes, she argues, Machiavelli's *Exhortation to Penance* can be read as clever subversion of Christian doctrine. On its own terms, she admits, all that can be said for certain is that Machiavelli's interpretation of the faith is strange, but not necessarily fraudulent.

David C. Innis captures the chilling hopelessness that lurks beneath the surface of Bacon's secularization of Christian hope in the *New Atlantis*. This chapter is the one most clearly critical of the elusiveness of the true goal of modern rationalism, and so it is especially to be recommended. Paul D. Cooke does a perfectly competent job of showing that Hobbes's Christianity is counterfeit, but he accepts too readily the thought that Hobbes's true standard is somehow both human reason and human sovereignty.

Martin D. Yaffe presents a most careful and brilliant account of Spinoza's *Theologico-Political Treatise*. He finds something good in Spinoza's destruction of the Bible's authority as the bond that holds political community together: We are now in a position to consider more freely the Bible's claim to be true. But doing so requires that we be more naive and more philosophical than the polemical and methodical Spinoza.

The three chapters on Locke actually disagree. Peter Myers argues that Locke was a rationalist who saw the limits of human reason, and thus, reason's incapacity to refute the claims of revelation. His Locke seems more Tocquevillian than characteristically modern in orientation. Dale S. Kuehne, the book's heretic, presents an anti-Calvinist but ultimately pro-Christian Locke. David Foster, with the preponderance of evidence on his side, counters that Locke's view of human freedom is actually radically antibiblical. His Locke is the most secular and materialistic of the three, but Foster can only muster a few limp words to question this modern extremism

The last two chapters are by editor Douglas Kries. The first exposes Montesquieu's veiled attack on Christian historiography. The veil is not out of respect for Christian faith but for religion's political power. Kries also contributes a fine account of Rousseau's attempt to replace revealed religion with civil religion. He notes that the philosophic discrediting of the former has not led to the reemergence of the latter. Perhaps that is because the psychological criticism by the Christians of civil theology remains true, whether or not the Creator-God of the Bible actually exists.

The authors generally limit themselves to doing little more than showing the hostility of the early modern political philosophers to the claims for truth and political power of revealed religion. The authors are to be praised for doing what they set out to do. But because it had already been done to my satisfaction by Strauss and other Strauss-inspired writers, I wish they had turned more to criticism of the emptiness of modern rationalism. The authors' tendency is to let the modern philosophers off the hook too easily by saying too often that they were primarily defending liberated rationalism as such from piety. The Christians were surely right, after all, to call the pursuit of autonomy sin or prideful self-deception, or not really rational at all.

Harvy Mansfield, in the book's judicious Foreword, says that the modern political philosophers saw religion as a sign of human weakness. To the extent that their efforts replace weakness or dependence with self-sufficient strength, religion can wither away. But at the end of the great success and deeper failure of the modern world, we are more likely to admit that the fundamental human experience is one of limitation, and that modern efforts to deny or destroy that indestructible fact are themselves forms of weakness. If we are not nihilists, then we add that human life as it is actually experienced is nonetheless good.

Religion has not, in fact, withered away. The modern political philosophers said too little or almost nothing about affirming our limitations as the indispensable conditions of human goodness and human happiness. They have too little to say, as Allan Bloom, for one, complained, about the twinship of love and death.

Beyond the Persecuting Society: Religious Toleration before the Enlightenment. Edited by John Christian Laursen and Cary J. Nederman. Philadelphia: University of Pennsylvania Press, 1998. 288p. $39.95 cloth, $19.95 paper.

Difference and Dissent: Theories of Tolerance in Medieval and Early Modern Europe. Edited by Cary J. Nederman and John Christian Laursen. Lanham, MD: Rowman & Allanheld, 1996. 240p. $62.50 cloth, $23.95 paper.

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The idea of toleration (of difference in religious lifestyles as well as in ethnic culture, general definition, or sexual orientation) has become a major theme of political theory research in the 1980s and 1990s. These two collections of new scholarship expand knowledge of the multiple versions of religious toleration from the twelfth through the seventeenth century. *Difference and Dissent* considers the theorists of John of Salisbury, Marsiglio of Padua, John Wyclif, Christine de Piszan, Hans Denck, Sebastian Frank, Francisco de Vitoria, Bartolomé de Las Casas, Jean Bodin, Thomas Hobbes, Samuel Pufendorf, Benedict de Spinoza, and John Locke. *Beyond the Persecuting Society* not only considers practices of toleration in eastern Switzerland, 1525–1615, and in seventeenth-century Massachusetts, but also analyzes the thought of Peter Abelard, John of Salisbury, Menahem ben Solomon Ha-Ne'ira, the circle of Cardinal Pole, Jean Bodin, Samuel von Pufendorf, Pierre Bayle, Aphra Behn, and seventeenth-century religious skeptics and millenarian dogmatists. The editors are to be commended for inviting political theory colleagues in history, literature, philosophy, and religious studies to join with those more numerous in political science to broaden the issues upon which toleration is studied. Each volume has an index, notes following each article, and a statement about contributors.

In *Difference and Dissent*, Nederman and Laursen respond to contemporary critics of toleration by disengaging the identification of toleration with the line of thought extending from Locke through the Enlightenment to Mill. A communitarian functionalist theory of toleration appears in Nederman's discussion of the rights theory in John of Salisbury and Marsiglio of Padua and in Kate Forhan's analysis of Christine de Pisan's philosophy of the active interdependence of the different social classes and the tolerance of one part of the body politic to another part, including women and foreigners. Laursen's article on Spinoza shows that his more universalist, modern theory predated and possibly influenced Locke; the inclusion of articles on Spinoza and Bodin, who both exhibit toleration of Jews, Muslims, deists, skeptics, and other freethinkers, shows by contrast Locke's limited focus on the toleration of Protestant dissidents by the established Church of England. The book concludes with William Walker's analysis of Locke's contention that persuasion is a kind of force; Locke's "ideal" commonwealth emerges as a place where all, including the ruler, have freedom and duty to persuade others concerning salvation.

Augustinian theology, according to Stephen Lahey, is the source of John Wyclif's belief in fair government rule over those destined for Hell. A more positive attitude is found in the Radical Protestant views of Hans Denck and Sebastian Franck, who, according to E. J. Furcha, value the divine spark in Jews, Muslims, and heathen; I would attribute that confidence to the Stoic thread in Erasmian humanism and to such medieval theorists of discourse among Christians, Jews, and Muslims as Raymond Lull. Such a view of the necessary premillenial forbearance of religious difference the editors appropriately label "concordance." In that category also is the toleration of Amerindian cultural difference in Vitoria and Las Casas, whose writings are viewed by Paul J. Cornish, as evidence that a Thomist framework allows protest against extreme injustice within general toleration of cultural difference.

Gary Remer interprets Jean Bodin as a theorist of pluralism who defends freedom to find one's own truth in a world where truth is knowable; the *Colloquium* he appropriately interprets not as a failed debate or a colloquy for conversion but as a successful model of how diverse religionists and doubters today might openly listen peaceably to one another's viewpoints. Glenn Burgess appreciates Hobbes's desire to distinguish religion from philosophy (truth claims) and

ecclesiastical polity from polity, and he views Hobbes not as a theorist of liberty of conscience but as a natural law theorist who contends that persecuting members of society for personal beliefs hurts political stability. Simone Zurbuchen interprets Samuel Pufendorf's rejection of the Revocation of the Edict of Nantes as an inconsistent blending of a republican argument for peace-producing balance in domestic and international politics and a natural law argument that religion is based on a natural freedom not delegated to the sovereign.

Beyond the Persecuting Society encourages contingent, local studies of toleration policies. The editors' introduction localizes and limits Locke and disputes the myths of relentless persecution in medieval and early modern times, of secularization encouraging toleration, of unilateral Christian intolerance of non-Christians. If this deconstruction is necessary, then obviously there has been a cultural lag between the work of numerous historians and the mainstream teaching in political theory.

The medieval section stresses openminded thinkers. Constant J. Mews's study of Peter Abelard's *Dialogue of a Philosopher with a Jew and a Christian* emphasizes Abelard's focus on rational ethics and his finding of a compatibility of such ethics with Jewish law and with Christianity. Mews shows Abelard's further development of dialogue in letters to Heloise and in his poem to his son. Nederman analyzes John of Salisbury's moderate Academic skepticism and his conviction that wise individuals might disagree.

The Jewish question appears from some important new angles. Gary Remer explores the very important issue of Jewish toleration of non-Jews, pointing out that Jewish law does not deny salvation to non-Jews, does not aim to convert all, and respects righteous gentiles; yet, the Provençal Talmudist Ha-Me'iri expanded Jewish toleration by clarifying that Christians are not idolatrous and that Talmudic regulations concerning trade with the idolatrous are not applicable to Jewish relations with Christians. Marion Leathers Kuntz emphasizes the Renaissance Catholic Venetian setting (where there were districts for Jews, Muslims, and Protestants) of Bodin's *Colloquium Heptaplomeres* and emphasizes that the open sharing of religious viewpoint without aim of conversion is musical harmony in that each voice is needed to complete the whole. Richard Popkin suggests that seventeenth-century millenarians who proposed temporary toleration until the near conversion of all to pure Christianity (such as Isaac La Peyrère or Anna Maria van Schurman) encouraged toleration as much as did the very different deists and nonreligious (such as Spinoza or Bayle).

Randolph Head's concise historical introduction to the sixteenth century focuses on the loci of limited toleration, and his article on eastern Swiss villages of Catholic majority and Protestant minority traces the shift from coexistence with Protestants worshipping in a neighboring village to sharing the same church for services of the two different religions. The account is a case study in conflict management and of variations in religious coexistence, where change is justified by custom rather than by principles of toleration. Likewise, Thomas F. Mayer's close study of Reginald Pole's possible influences on Thomas Starkey, Andras Dudic, and Gianbattista Binarid indicates the practical decisions made by the lenient, concordant Pole during the Marian Reformation.

Laursen's introduction to the seventeenth century focuses on the diverse motives for toleration amid the contingencies of civil discord in the variety of European states. Detlef Döring treats Samuel Pufendorf as a Lutheran natural law theorist who is not as liberal as Leibniz, except in the notion that the sciences should be free of influences of theology. Laursen highlights the turning points in Bayle's development of his views on rights of conscience and toleration, which included preventative measures against those who would persecute others. Aphra Behn's novel *Oroonoko* exhibits a respect for the African hero's virtue and for deism that makes this female royalist poet and playwright an early advocate of toleration of diverse ethnic lifestyles. The concluding article on Massachusetts by H. Frank Way shows the colony shift from exiling Roger Williams and Anne Hutchinson, to a gentler policy during the English Restoration, and by 1708 to a grudging toleration of Quakers.

The overall contribution of *Difference and Dissent* is to recognize communitarian and natural law foundations of toleration theory and to encourage us to ask precisely whom a theorist is tolerating, whether s/he intends temporary concordance or permanent toleration, and to whom the theorist is not extending toleration. *Beyond the Persecuting Society* gives evidence of pockets of religious coexistence and of developments in toleration theory in Christian, Talmudic, literary, and philosophical authors of medieval and early modern Europe. Either book would be appropriate for an undergraduate or graduate course on political theory.

Politics and Truth: Political Theory and the Postmodern Challenge. By Theresa Man Ling Lee. Albany: State University of New York Press, 1997. 243p. $17.95.

Louis Herman, *University of Hawaii at West Oahu*

Ever since the "polis killed Socrates," those who follow the truth quest have kept a sharp eye on those who wield power. Theresa Lee's exploration of politics and truth begins by noting that twentieth century politics is distinctive by way of how political ideas dominate the conduct of politics. The Cold War of political ideas has given way to the postmodern politicization of knowledge in the liberal democracies and the equally important depoliticization of knowledge in the Leninist socialist camp. This has given political theorists a unique opportunity, states Lee, to reconsider the fundamental principles of liberal democracy without any palpable threat.

Ironically, *Truth and Power*, a highly intelligent work of professional scholarship, exemplifies the sort of conventional theoretical writing that is quite irrelevant to those who wield the sword or those who are engineering the final stages of corporate globalization (or, for that matter, the lay citizen). The book is complex and makes much of small distinctions, which, elegant though they may be, are laboriously elaborated. Consequently, the book falls far short of taking us beyond the technical deconstructions of postmodernism; it fails to provide what it suggests is needed—a Socratic philosophical discourse which engages what Eric Voegelin called "the full amplitude of human experience" and still has some moral force. Yet, the book does provide some clearly defined points of departure and a few hints for a bolder, more creative revisioning that the discipline so badly needs.

Lee aims to evaluate postmodernism as an alternative to the duality of liberal democracy and Leninist socialism. She begins with Plato, the obvious paradigmatic thinker on power and truth, proceeds to Hobbes, then Weber, Foucault, and finally Arendt. Lee illustrates some of the book's main points by briefly considering the epistemological debate in post-Mao China, and the identity politics of liberal democracies, associated with postmodern attempts to reconstruct university curricula.

Lee's reading of Plato follows a popular, well-established reductionist interpretation via Nietzsche, Popper, Foucault, and Arendt (and many others). She presents Plato's notion of

philosophy as essentialist, providing absolutely transcendent standards, embodied in the aristocratic *Republic*; a defense against the "opinion" of the polis that killed the philosopher. Lee mentions Strauss and briefly considers Gadamer's more reflective interpretation, which credits Plato with a subtle, dialectical notion of philosophy. For Gadamer, the Socratic-Platonic dialogues embody the quest for meaning in the openness of authentic discussion; meaning is found in the "in-between" (Plato's *metaxy*); the movement of thought between opposing positions. The *Republic* is a moment in an ongoing dialogue, not to be understood literally, and certainly not as the final word. Politics and philosophy only have meaning in a creative tension with each other. After this sympathetic treatment Lee dismisses Gadamer, preferring to resolve Plato's paradox (between the aristocratic republic and a democratic truth quest) in favor of a tyranny of a philosophical elite. She marshalls support by citing the familiar aristocratic passages from the *Republic*, emphasizing Plato's dogmatic insistence that "the dialectic is the only way to truth." In doing so Lee effectively reduces the dialectic to a technique and seems to miss the point: The dialectic is indispensable because knowledge only becomes politically significant "truth" in the process of being mediated by the logic of discussion; that is, by being brought into relation with the different experiences and perspectives of a community of philosophers. A dialectical understanding of philosophy and politics would put the two in a creative dynamic tension; philosophers would rule to the extent that they eschew the desire to rule, and those who rule would prefer the joys of the truth quest.

Lee carefully elucidates "postmodern" tendencies in Hobbes and Weber, usefully clarifying what remains thoroughly modern in some "postmodern" thinkers, especially Foucault. She notes that Foucault himself identified with the question and questioning strain in Enlightenment thought. Foucault's rejection of the principle of universality contained in all metanarratives results in a postmodernism and a politics "shaped by the Liberal sentiment that there is always 'too much governing' " (p. 110).

It is against this problematic that Lee turns to Arendt. Arendt, however, wants to defend politics from the totalitarian dangers of ideology by reproducing and elaborating a host of neo-Kantian distinctions, resulting in some convoluted if not absurd conclusions. Arendt distinguishes between, on the one hand, the rational truth of axioms and theories in "mathematics, science and philosophy (sic)" and, on the other hand, the factual truth of "facts and events . . . in the ever changing affairs of men." Factual truth is thus by definition political, intimately related to speech and Socratic praxis. Arendt and Lee then further distinguish this Socratic philosophy from an absolute Platonic truth and end up by insisting that "truth" and philosophy should have nothing to do with politics. It would make a lot more sense to reserve the term "ideology" for the reifications of thought that support totalitarian regimes, and preserve the terms "philosophy" and "truth" for the Socratic speech dialectic that Arendt and apparently Lee think appropriate for influencing political action. This would save Arendt, and a number of postmodernist philosophers, from the embarrassment of having to say things like "totalitarianism is a profound indictment of the role of philosophy in politics."

The conclusion, dealing with post-Mao China and identity politics, is by far the most interesting and creative section of the book and should have been expanded. Lee examines the reforms in post-Mao China, which took seriously the issue of epistemological standpoint. She claims that this analysis, contra Connolly's notion of agonistic democracy, shows that "there is nothing intrinsically democratic in a political practice that recognizes that truth ought to be rooted in the reality of experience" (p. 153). Lee cites, among others, Deng's "emancipation of the mind," but I am not persuaded that these utterances have any more than a superficial resemblance to Connolly's radical critical approach. I think Lee is correct, however, in suggesting that the antifoundationalist, contexualist approach of postmodern "legitimation discourse" is "not necessarily . . . conducive to democratic practice" (p. 157).

Lee examines identity politics in the West as the other example of politics in the spirit of antifoundationalism. She points to the obvious insufficiency: Politics based on a single identity is divisive and corrupts any notion of a good of the whole. She seems to end up endorsing the standpoint perspective of Hartsock, with its unifying notion of individual "experience" integrating multiple identities (nested identities?). Lee implies that this is similar to Arendt's principled defense of politics from totalizing systems. Her final sentence notes inconclusively and apologetically that this is "fundamentally an essentialist postulation." Lee's own position on this and other important issues remains unclarified.

Lee's predicament is the predicament of much of postmodern political theory. On the one hand, there is revulsion against the absolute "isms" of modernity and their murderous regimes. On the other hand, there is a growing recognition (which is thoroughly Socratic-Platonic) that without some sort of principled shared understanding of the human condition and a collective "good," politics becomes capricious, vulnerable to demagogues and tyrants. Unfortunately, a rigid dualistic (Cartesian) habit of thought blocks most Western philosophers from thinking Socratically, that is, in terms of the meaning generated by the tension between opposites, and thus moving through the paradox. None of Lee's sources show much stomach for this sort of Socratic-dialectical (as apposed to the Marxist-Hegelian variety) thinking, with the exception perhaps of Gadamer; and Lee ignores those philosophers who do: on the Left, the early Frankfurt School thinkers, Horkheimer, Adorno, and Marcuse; and on the Right Eric Voegelin, who is one of the few modern philosophers to explore the dialectically formulated "truth quest" in world civilizations.

It is a pity Lee does not take this opportunity to respond with gusto to the postmodern challenge to engage "the other." Her interest in China would seem to position her well to go beyond jargon-filled epistemological tracts "celebrating difference," where all opinions and all cultures are indifferently equated. Many non-Western, premodern traditions, from Native American shamanism to Chinese Taosim, are quite comfortable with such paradoxical, Socratic-dialectical thinking and could help considerably in formulating a normatively compelling "foundationless" foundation; neither absolute nor arbitrary; secure enough to guide meaningful action yet constantly shifting.

Ecocritique: Contesting the Politics of Nature, Economy, and Culture. By Timothy W. Luke. Minneapolis: University of Minnesota Press, 1997. 253 p. $49.95 cloth, $19.95 paper.

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One can estimate the maturity of a social movement by the caliber of its internal critics. A mature movement generates thoughtful criticism within its own ranks. Arguably, this internal dissent stimulates the movement's capacity for adaptive change. *Ecocritique* confirms that the environmental movement has come of age. In its nine chapters, each of

which has appeared in whole or part in other publications, Timothy Luke offers insightful assessments of selected environmental organizations, theorists, and orientations.

Luke is not the first internal critic of environmentalism. Indeed, for the last number of years a new book has appeared on the shelves every few months that takes (some segment of) the environmental movement to task for its shortfalls. Luke suggests that his book is unique because it addresses environmental thinkers and groups that hitherto have been ignored. Yet, his investigations of deep ecology, Earth First!, green consumerism, and social ecology, to identify the subjects of four chapters, rework well-tilled ground. That is not to say that fresh plowing is without merit. Luke's chapter on Earth First!, for instance, is balanced and informative. On the whole, however, the value of *Ecocritique* rests not so much with the originality of its subject matter as with the originality of its insights.

Luke does venture into less charted waters on a few occasions, such as his chapters on Biosphere 2 and Arcosanti. Biosphere 2 was an experiment in building an ecologically self-sufficient set of biomes within a self-contained architectural structure. It was meant to yield ecological knowledge by mimicking the workings of the Earth as a whole. Located outside Tucson, Arizona, Biosphere 2 became operational in September 1991. For the next two years, a crew of eight was sequestered in this largely self-sustaining microplanet, which from day one was plagued by malfunctions. Luke acknowledges the intellectual and technological resources that were marshaled to get Biosphere 2 off the ground, but he worries that in design and in fact it stayed there. Rather than better integrating human communities into the natural world, the experiment put a technocratic green disguise on a corporate capitalistic venture.

Arcosanti, located in central Arizona, was the brainchild of Italian architect Paolo Soleri. It was designed as an environmentally benign, automobile free, urban landscape. Grounded in a Teilhardian philosophy, Arcosanti was meant to accommodate many thousands of residents and serve as a model for future cities. Currently it is inhabited by less than 100 people and serves largely as a tourist attraction and convention center. Still, Soleri's visionary city provides an alternative to the abuse of ecological space, commonly known as "sprawl," that largely defines the contemporary American landscape. Luke is enthusiastic about the possibilities of ecological urban design. Yet, he worries that Arcosanti, like Biosphere II, prefigures a "global systematization in green governmentality" (p. 114). Soleri's arcology, Luke observes, "smacks of a puritan desert despotism" (p. 171).

The rise of green totalitarianism is a central theme of *Ecocritique*. Luke shares the concern of Charles Rubin (*The Green Crusade*, 1994) that environmentalists are enamored with centralized, intrusive governance of potentially planetary proportions. A chapter assessing the efforts of Lester Brown's Worldwatch Institute, known chiefly for its annual publication, *State of the World*, reworks this theme. According to Luke, the institute stands in alliance with corporate capitalism in developing ways to control people through global strategies of sustainable development. Applying the thought of Michel Foucault to environmental issues, Luke argues that Worldwatchers are chiefly in the business of "environmentalizing discipline" (p. 81), and their goal is "to police the planet" (p. 84). This perspective is provocative, but at times Luke's railings come close to the paranoia of radical antienvironmentalists, who fear the rise of the United Nations because it designates World Heritage Sites.

For Luke, nature has been denatured through the commodification of life that accompanies corporate hegemony. Instead of directly taking on the institutions of translational capitalism, however, he criticizes environmentalists for being insufficiently radical in their own struggles against these institutions. There is, of course, a long tradition of armchair philosophers haranguing social activists for their compromises. Still, the irony is pungent. It is heightened by the fact that Luke's critique of too-timid, business-friendly environmentalism has been accompanied by the standard academic practice of producing a resource-consuming literary commodity of dubious recyclable value.

The connections to corporate capitalism of many of the mainstream environmental organizations certainly deserves criticism. Yet, Luke might have given due to those environmental groups that assail—in word and deed—the ecological irresponsibility of corporate power, such as the Center for Health, Environment and Justice (formerly Citizens Clearinghouse for Hazardous Waste), Earth Island Institute, Friends of the Earth, and Greenpeace, to name but a few. Luke picks easier targets. He focuses on The Nature Conservancy (TNC), a wealthy, business-like organization that purchases ecologically rich land and sets it aside for preservation. Luke questions whether these efforts to preserve biologically diverse tracts of land actually undermines the "cause of Nature preservation" because it reduces "Nature to real estate" (p. 58). This reduction should never be accepted, Luke insists, though he does not address whether all forms of land ownership are equally illegitimate.

To acquire its land, TNC needs lots of money. Like many other environmental organizations, it engages in endless fundraising. For good reason, this focus on revenue worries Luke. For many, environmentalism has primarily become a business enterprise. But Luke is wrong to assume that TNC does not also solicit from its members and patrons the "more messy contributions [of] time, labor, or moral witness" (p. 61). Like most environmental organizations in America, it relies heavily on volunteer efforts. While paid staff at TNC number a few hundred, about 25,000 of its members contribute 300,000 hours yearly doing the messy volunteer work of ecological preservation and restoration.

Luke also mischaracterizes TNC's mission of safeguarding biodiversity. It is not a matter of protecting threatened species as isolated specimens of a former, biologically diverse, time. For Luke, TNC is nature's mortician, a "burial society dedicated to giving perpetual maintenance and loving care at a variety of Nature cemeteries" (p. 71). (Luke believes that Nature ended sometime in the not-too-distant past for some pretty obscure reasons, such as the penetration of national capitalism into Russia and China during the Nixon years and the oil crisis of the early 1970s.) In fact, TNC's goal, like that of most preservationist groups, is to safeguards the dynamic relationships among species and the capacity of species to evolve over time. Preserving many large, and preferably interconnected, chunks of natural habitat facilitates both species preservation and evolutionary adaptation.

In another chapter, Luke aptly questions whether the voluntary simplicity and frugality of green consumers will suffice to preserve nature in the face of a global economy structured by the logic of corporate capitalism. Luke's depiction of the omnipotence of corporate capitalism, however, has the effect of freeing the individual from all environmental responsibility and undermining the incentive for individuals to organize against institutionalized environmental degradation.

Luke disparages the efforts of those who espouse individual responsibility—organizations such as the Earth Works Group, which wrote the best-selling *50 Simple Things You Can Do to Save the Earth*. The naiveté of such do-good

approaches is troubling to be sure. Though political action and community organizing is recommended, it is not emphasized. For Luke, however, the problem is simply that these approaches are not radical enough. "Rather than pushing for waste elimination," Luke writes, "the Earth Works Group stands for waste reduction. Instead of advocating total economic transformation, it accepts weak bureaucratic regulation of present-day polluting processes. Unable to support the total reconstitution of today's productive forces, it advocates piecemeal reforms to lessen, but never end, their most environmentally destructive activities" (p. 123). Luke rejects "weak reformist strategy" (p. 122). He calls instead for a radical social transformation that would yield bioregionally decentralized communities operating in "equal partnership with Nature" (p. 208). The ultimate goal is "survivable communitarian ecologies within which people dominate neither other human beings nor their fellow nonhuman beings" (p. xiii). Presumably these biocentric, egalitarian, protoanarchistic communities would produce neither waste nor pollution.

Luke rejects "ecosocialist millenarianism." He also dismisses the "utopian ecologism" of deep ecologists because it fails to outline the practicable means for realizing its moral vision and, like most other revolutionary programs, lacks "a theory of the transition" (pp. 24–5). This is clearly a case of the pot calling the kettle black. Notwithstanding a fine chapter on Murray Bookchin's environmental philosophy and Luke's insistence that all environmental advocacy must be "workable and realistic" (p. 203), *Ecocritique* remains divorced from practicality and is a stranger to transitional strategy.

The "ecological populism" promoted by Luke would entail "rebuilding the contemporary city and society from the ground up around new aesthetic sensibilities." It would require "equal participation by every community member in shared action to make collective decisions," such that, in the end, everyone will hold "equitable shares of property, power, and privilege in the community commonwealth" (pp. 200–1). Luke's unwillingness to suggest any viable means of achieving these ends is troubling. It is particularly suspect owing to his disparagement of those who take the small steps that might slowly move individuals and societies down the road to ecological responsibility. Luke insists that "only the actions of a very small handful of the humans that are now living, namely those in significant positions of decisive managerial power in business or central executive authority in government, can truly do something to determine the future" (p. 126). That conviction is what most separates environmental activists from those who write books about environmental activists.

Reflecting on Plato's idealism, Aristotle observed that the best often becomes the enemy of the good. One senses that Luke's diatribe against environmental reformism gains its fire from a vision of the ecological and sociopolitical best. This alluring vision will never come closer to realization, however, if yearnings for what is best undermine all concrete efforts to achieve what is good. Luke's ideal of a global confederation of ecologically benign and fully egalitarian communities is infatuating. Yet, in the end, Luke's unwillingness to address the practical means of its achievement leaves the reader forlorn. One is reminded of the traveler who asked a resident for directions to a small town in the region. "Ah, yes, that's quite a few turns in the road. If I were you, I wouldn't start from here!"

That is a frustrating response, and *Ecocritique* is a frustrating book. But it is also lively, provocative, and insightful. With the understanding that frustration is often the mother of invention, I would recommend *Ecocritique* to all those whose environmental concerns will be piqued rather than palsied by the dearth of easy solutions.

The Racial Contract. By Charles W. Mills. Ithaca, NY, and London: Cornell University Press, 1997. 171p. $19.95.

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This is an ambitious little book, as it seeks to place race at the very center of political theory. Mills attempts to show that race has been an essential, though often unspoken, concept in modern Western political thought and that its importance must be acknowledged and analyzed if we are to overcome the legacies of our racist past (and present). Mills develops "a global theoretical framework for situating discussions of race and white racism" (p. 2) that can serve as "a conceptual bridge between . . . mainstream (i.e., white) ethics and political thought [and] . . . Native American, African American, and Third and Fourth World political thought" (p. 4). The framework is a contractarian one, derived from mainstream Western thought and used to engage and critique that tradition from the perspective of those who traditionally have been left out of the social contract and subjugated by it. The main thesis of the book, then, is that there has been, in the modern era, a global racial contract, the parties to which have been whites, and the substance of which has been the denial of nonwhites' status as fully human, "the exploitation of their bodies, land, and resources, and the denial of equal socioeconomic opportunities to them" (p. 11).

For those who agree that issues of race and racial justice demand far more attention from political theorists than they are currently receiving, the book is a welcome contribution. By showing the systematic and deeply embedded nature of racism in modern Western political theory and practice, Mills demonstrates that racist policies and ideas are not unfortunate divergences from the general rule of race neutrality but are themselves the rule in Western culture. In the process, Mills provides an analytical framework that connects claims for domestic racial justice and those for international justice. In all these respects, the book in an important contribution to current discussions about justice in both realms.

The book is marred by some shortcomings, the greatest of which is that the arguments it presents are often too truncated to be fully persuasive. This may be due to the author's desire to produce "a short, punchy book" (p. x) that must sacrifice some precision and detail for accessibility. Still, this reader often wished Mills had sacrificed some of the book's punchiness for greater philosophical depth. One example is the discussion of the major figures in the social contract tradition (pp. 64–72). Mills's general point is that the racial contract "underwrites" the social contract, and he examines the thought of Hobbes, Locke, Rousseau, and Kant to demonstrate the racist underpinnings of their theories. In each case, however, the situation is far too complex to be adequately captured in the few short paragraphs Mills devotes to each.

Mills suggests that, for Hobbes, the state of nature is a real possibility only for nonwhites and is purely hypothetical for whites, due to the latter's greater rationality and "superior grasp of natural law" (p. 66). But since this contradicts everything we know about Hobbes, Mills must quickly retract the claim, and his position on Hobbes is therefore left unclear. Regarding Locke, Mills notes the contradiction between his just-war theory of slavery and his involvement in the slave trade and resolves it through "the supposition that Locke saw blacks as not fully human" (p. 68). This is pure

speculation for which Mills can provide no textual support and which is largely uninformed by the sizable literature on Locke's relation to the slave trade and its bearing on his political thought. Mills then offers the contentious suggestion that Rousseau sees Europeans as "intrinsically civilized" (p. 68) and that his "praise for nonwhite savages is a limited paternalistic praise, tantamount to admiration for healthy animals, in no way to be taken to imply their equality, let alone superiority" (p. 69). One might have thought that Rousseau's position is far more ambivalent than this suggests, and Mills simply does not say enough to establish the plausibility of this assertion. Regarding Kant, Mills argues that "The Different Races of Mankind" sheds important light on his moral and political theory, because it makes clear that, for Kant, full personhood depends on race and is reserved for whites. Here Mills is more persuasive, but he overstates his case by trying to connect Kant, without much discussion, to Nazi ideology (p. 72).

In all these cases, then, the discussion is inadequate. Though there may be racist elements in the thought of all these writers, Mills fails to show how the racism is *essential* to the overall theory and, therefore, does not establish that the racial contract *underwrites* the social contract. A much more extended and nuanced treatment is called for, and it might support Mills's overall position. As it stands, however, the discussion is at best suggestive, at worst misleading.

There is perhaps a more fundamental difficulty with the book, one related to its contractarian character. Those skeptical of contractarianism's justificatory power may be skeptical of its force as a critique as well. As with contractarianism in its positive, constructive mode, so, too, in its critical guise one must ask what is gained by speaking in terms of agreement and contract. Of course, the atrocities and racist policies that Mills cites occurred, and no doubt their legacies continue to shape our society and the world. But what is added to the account by the contractarian framework? It seems to raise more questions than it answers. For instance, there is some ambiguity as to who exactly are the parties to the contract. Mills suggests that the parties are whites (though not all whites [pp. 11, 107]), but sometimes he implies that nonwhites can be thought of as parties as well (pp. 25–6, 118). The content of the contract also is not clear, since it is not really one contract but several, and according to Mills it is constantly being rewritten and "has now written itself out of existence" (p. 130; but see p. 133). And Mills argues that the racial contract is an "historical actuality" (pp. 19–31), but of course it is not. Like all contracts posited by contractarian theories, it is a metaphor. All this seems to obscure rather than clarify the important normative issues raised by the racist history of our society and, therefore, undermines rather than advances Mills's project.

As a result, the book provides little guidance as to what is to be done. Mills provides only a few remarks about what he sees as the practical upshot of his analysis. He "does not see the ideals of contractarianism themselves as necessarily problematic" (p. 129; but whose contractarianism? Hobbes's?) and even calls the Kantian idea of the infinite value of all human beings "a noble and inspiring ideal" (p. 55). Yet, he rejects the idea of extending the social contract to nonwhites, at least not without first acknowledging the existence of the racial contract (p. 77). But beyond this acknowledgment, what is necessary to do justice? Mills offers, for example, no sustained argument for, say, reparations, either at the domestic or international level (but see pp. 37–40). And he is entirely silent on issues of culture. By focusing exclusively on the "material" dimension of white domination (p. 33), Mills has nothing to say about cultural imperialism. Since many of the most salient racial and ethnic issues today are more cultural than material, the book is not as helpful as it might have been. (Perhaps the author's "antipostmodernism" [p. 129] contributes to a neglect of these issues.)

Despite these shortcomings, the book is highly suggestive and succeeds at being engaging and accessible. Though almost none of the issues it raises are treated with the depth they deserve, that this slim volume raises so many issues is itself an achievement. It is hoped the book will provoke much discussion which will further develop the arguments presented by Mills. If it does, Mills will have succeeded, even if the discussion is unlikely to take place within the framework he provides.

Liberalism and Its Discontents. By Patrick Neal. New York: New York University Press, 1997. 209p. $40.00.

Emily R. Gill, *Bradley University*

Although Neal's book comprises a series of essays that have each appeared independently over the last dozen years, they fit together admirably and constitute a trajectory that raises important questions for liberals today. The liberal desideratum over recent years has been "to give an account of liberal order which shows it to be both inclusive of and fair toward all ways of life which themselves are willing to abide by the terms of fairness" (p. 163). Neal's project demonstrates, however, that theorists whose liberalism is full-bodied and vigorous maintain this vigor at the price of exclusiveness, whereas those whose liberalism is inclusive maintain the latter quality at the price of substance and vigor. Neal eventually opts for a more Hobbesian "vulgar liberalism," in which a rough equality of power among competing interests is a more necessary precondition for a stable liberal order than is an agreement among political philosophers on the terms of an elusive or even nonexistent overlapping consensus.

Neal begins by maintaining that governments, as part of the social fabric, cannot be neutral on the question of the good life for human beings. Therefore, even liberalism cannot maintain a privileged position above the fray of competing values but must be positively defended in terms of the preferences it produces. Liberalism in fact possesses a meta-theory of the good on an epistemological level. Because it is individuals who bear conceptions of the good and because these can only be contingently shared, liberalism displays a particular *conceptualization* of conceptions of the good, one that excludes those who, for example, espouse conceptions of the good which require its collective pursuit on the basis of essentially shared ends, as Sandel or MacIntyre advocate.

Rawls, of course, implies that his conception of justice as fairness, expressed in an overlapping consensus, threads its way between the horns of the dilemma of substance versus inclusiveness. It avoids controversial, comprehensive conceptions of the good which would exclude those who cannot subscribe to these, while it simultaneously eschews a modus vivendi which is "merely" prudential and without moral substance. But for Neal, "the theoretical ground which Rawls wishes to claim and occupy does not, in my view, exist" (p. 8). First, the farther we progress from the Kantian ethical conception of self-willed principle toward its actual political expression, the more closely we approach Rousseau's general will and the existence of communally determined constraints on this expression. Second, the farther we move from universalistic and metaphysical interpretations of justice as fairness toward empirical propositions about the self-understandings

of democratic citizens, the more closely we approach political interpretations that resemble Hobbesian prudence by imposing moral beliefs without moral justification on those who may not share them. Finally, the more "a-foundational" Rawls becomes in order to avoid consideration of the truth claims of comprehensive doctrines of the good, the more his bracketing of controversial issues as nonpolitical, and therefore off the political agenda, serves to settle the identity of political actors and interests in advance, thereby ironically excluding and freezing out persons and points of view from participating in defining the terms of the overlapping consensus.

To stem the bleeding of ethical substance, as it were, from liberal theory, we might turn, as Neal does, to the perfectionist liberalism of Raz. Although Raz views personal autonomy as a necessary part of a good life, he believes that autonomy must be directed toward the good to be valuable, and that public or state action must support perfectionist ideals if valuable options are to be preserved and if their validity is to be signified. Neal is disturbed, however, by Raz's apparent endorsement of reason over will, which effectively freezes the perspective of authority as "non-contingent and non-contestable" (p. 156) and valorizes social consensus over inclusiveness. And although Dworkin's later work purports to achieve both more vigor than Rawls and more inclusiveness than Raz, his conceptual innovations do not convince Neal that liberals can escape a choice between "Rawlsian minimalism . . . and Razian perfectionism" (p. 181). Thus, Neal concludes that "Hobbesian liberalism is defensible in its own right; liberals ought to let him out of the closet and affirm him as their own" (p. 186). It is unclear to Neal that the mere existence of a link between individuals' conceptions of the good and elements of the public conception of justice alone provides any particular motivation, moral *or* prudential, for the individual to adhere to the latter. A rough equality of power among groups, brokered by the state, and checks on the state's own potential domination seem more effective.

Neal's demolition of attempts in recent liberal theory to combine robustness with inclusiveness is clearly, skillfully, and even elegantly argued. He is not unsympathetic to those he critiques, and he admires specific facets of all these theories. I shall pose only one query of his project. Neal notes that retreat from the liberal desideratum is manifested by recent tendencies to see the value of autonomy as a particular or substantive conception of the good life rather than as a broader "point of access for choice amongst the competing substantive views" (p. 168), or what Will Kymlicka would call a context of choice. Later he classifies Kymlicka's project itself as an ideal-based model of liberalism, because it defends liberalism on the basis of an essential human interest in leading a good life. Although I would not suggest that Kymlicka's is a neutrality model of liberalism, to the extent that it is ideal-based, it is rooted in a particular *type* of ideal. The capacity for personal autonomy does provide a point of access or context of choice enabling us to examine and reaffirm or revise our current projects and goals, but it does not tell us what substantively constitutes the good life for us individually. Therefore, it functions as a meta-ideal, as it were. Because we will hold differing views on the good life and on the implications of these views for public policy, the evolution of a modus vivendi is still necessary. Moreover, the neutrality model, if viable, would also require adjudication among differing views of what constitutes neutrality.

Neal's modus vivendi model, then, may not be a separate model of liberalism, but it may instead represent a particular way of negotiating the conflicts arising from the instantiation of any *other* model. Just as Neal questions settling the identity of political actors and interests in advance of political activity, I question establishing the content of ideals prior to the political process. Overall, Neal's book is provocative, raising crucial issues for all who are interested in liberal theory and practice.

The Art of Being Free: Taking Liberties with Tocqueville, Marx, and Arendt. By Mark Reinhardt. Ithaca, NY, and London: Cornell University Press, 1997. 234p. $39.95 cloth, $16.95 paper.

Michael Janover, *Monash University*

This impassioned and lucid book proposes to "take liberties" with the reading of three modern political thinkers in order to gauge degrees of fit and misfit between works of the tradition of political theory and a world in which traditional assumptions of congruence between politics and state collapse, and in which ideals of harmony between politics and moral community are found wanting. Indeed, if Reinhardt is right, political theory cannot evoke or presume a single, whole political world at all. In a condition of displacements and fractures, in which global aggregations of power are accompanied by disintegration of old identities, classic texts of political thought must be read in strange ways if we are still to learn from their depictions of the past and present and their imaginings of the future. Reinhardt argues that, if they are to mean anything to us, Tocqueville, Marx, and Arendt must be "read in the gaps" between the coherent worlds they project and the fragmented multiple worlds we now inhabit. Only then, through ironic interpretative acts of piracy and errancy, might we arrive at images and concepts of freedom and democracy informed by the past and formative of future possibilities.

For all its sense that the lamps of tradition need reorienting, or even rewiring, if they are to illuminate late-modern conditions, this book is at one with the theorists it reappropriates in believing that the activity of political thinking can transform the practice of politics. The theorists analyzed here (two chapters on Tocqueville, one each on Marx and Arendt) are powerful imaginative thinkers who weave conceptual forms and historical stories into nets of practical significance. In his fine, close readings of these "strong poets of the political" (p. 7), Reinhardt focuses on the forms, and deformations, of political space that their writings portray, suggesting that contemporary political thought and action can learn from their overlapping, though by no means identical, emphases on democratic practice and on freedom. In Reinhardt's view, the encompassing and world-mapping political theory that all three of these thinkers exemplify is always at risk of narrowly confining or evading the problems and potentialities of politics by virtue of the limits and lights of its own vision of what "the political" comprises and makes possible or impossible. This view, that the world mapped is the world made by figures and motifs of the theory, owes evident debt to the deconstructive strategies of literary theorists and philosophers of the last three decades, and it also echoes the yet older writings of Sheldon Wolin on the epic vocation of the political theorist.

In attending to the ways in which the theorists he reads contain and evade the possibilities of politics, Reinhardt plays a dangerous game. He wishes neither simply to represent their epic vistas nor to discard or dismiss them altogether. More fruitful understanding will flow from what Reinhardt calls "reading the gaps" (p. 23) of their treatments of public and private, freedom and despotism, democracy and plurality. The normative premise of this kind of reading,

and its criterion of the fruitfulness of understanding, is that it "issues a challenge to our own conditions of unfreedom and the imperfections in our democratic theories and practices" (p. 29). This is a work of zestful and bold critical theory. Reading the gaps in political theories and between those theories and ourselves means reading them resistantly and against the grain, but it does not mean reading them tendentiously so as to use reading as a weapon against them. In all three cases, Reinhardt's readings are close, sympathetic, rich but for all that, because of that, critical in the best sense. His are readings that work from inside Tocqueville, Marx, and Arendt to reveal tensions and frictions within their own concepts, idioms, rhetorics, and visions.

In Tocqueville's case (the most extensively treated and the one from which the marvelous title phrase, "the art of being free," is drawn) reading against the grain means criticizing Tocqueville's agrarian (grainy!), localist, racially, and sexually sequestered vision of the homogeneous (Puritan) cultural basis of American democracy, while affirming Tocqueville's enthusiasm for the enactment of freedom through democratic association. This means taking from Tocqueville the idea of the energy of public political action as the key resistance to the growth of tutelary powers (or what Foucauldian readers will call disciplinary apparatuses) but resisting Tocqueville's sense that such action can exist only where class differentials are low or absent and where the actors share the same civic and cultural (white, male, American) identity. The difficulty with this exciting and exacting reading is that once the Tocquevillean prism of a homogeneous civic public is shattered to make way for multiple publics/identities and for the "radical democratization of cultural power" (p. 60), it is hard to see what the basis of mutual dialogue and collective action will be, short of sheer interest or an implied commonality beneath the multiplicity, which simply reposes the problem of Tocqueville's "(con)founding" of democracy (pp. 59–63).

A similarly polarizing and resistant reading is brought to bear upon Marx, who is seen to provide, sometimes in the same text, two contending conceptions of "freedom as transcendence" (of politics) and "freedom as a political practice" (pp. 95–6). Drawing upon readings of Marx (especially the *Manifesto* and the *Eighteenth Brumaire*) that develop the literary tropes and rhetorical strategies through which Marx invents and discovers the actors, the spaces, and the stakes of the politics of class conflict, Reinhardt shows how Marx's theory and dramaturgy of history are undermined by his own accounts of the ironies and uncertainties of political events, personae, and masks. This surely helps to reclaim Marx as a political theorist, but Marx himself does little to illuminate or substantiate any sense of "freedom as a political practice." Whenever he adumbrates shapes of politics as theater or gesture, as unexpected forms and transformations of class representation, he recollects these moments into the philosophically (over)wrought and politically undertheorized (depoliticized) container of History.

The reading of gaps, tensions, ambiguities in the thought of Arendt proceeds once more by locating a bifurcation between differing conceptions of politics in her work. On one side are the pure but empty politics of agonistic action, and on the other side the less pure, more substantial, politics of claims, interests, and solidarities which emerge from the cases of worker-councils and revolutionary moments and which serve Arendt as ideal types of the politics of action. Reinhardt's key move is to bring Arendt's own image of action (as disclosure of identity and as speech-deed of innovation) to life within spaces that she consigned to the depoliticized register of "society." This transposition of the forms of politics onto the contents of spheres formerly conceived as unpolitical matches Reinhardt's earlier suggestion that Tocqueville's democratic association be transposed to the register of culture. Both moves open up possibilities of thinking toward a cultural politics of identities. Such a politics remains outside the boundaries of "the political" for both Tocqueville and Arendt, and it is subordinated to the politics (and identity) of class by Marx. One great merit of this thought-provoking book is that it opens new possibilities for political thinking without abandoning the rich conceptual and imaginative resources of these three thinkers of politics as democratic association, as unique but public action, and as social transformation.

The Political Consequences of Thinking: Gender and Judaism in the Work of Hannah Arendt. By Jennifer Ring. Albany: State University of New York Press, 1997. 358p. $34.50 cloth, $22.95 paper.

Lisa Disch, *University of Minnesota*

Jennifer Ring's fine new study of Hannah Arendt deserves a broad audience for, among other reasons, its astonishing thesis: Hannah Arendt thought as a Jew and was received as a woman. Why should this be astonishing? The simple reason is that many of Arendt's more recent readers have meekly obeyed her instruction not to interpret her public writings in light of what she took to be the private matter of her identity. Ring's book defies Arendt, and that alone makes it interesting. In addition, in Ring's words, it is a "compassionate" work that offers an intellectually and politically engaging portrait of Arendt as "a conflicted Jewish intellectual" (p. 237).

In Part One, Ring takes a wholly original approach to Arendt's *Eichmann in Jerusalem* by situating the text in the politics of the two communities that criticized it: New York intellectuals and the Israeli leadership. Whereas members of the *Partisan Review* crowd attacked Arendt's detachment to atone for their own "cosmopolitanism," in Israel the report played into a crisis of legitimacy. Ben Gurion needed the trial to redeem himself from a double indictment: that he had done too little for Holocaust victims during the war and that he had been " 'soft on the Germans' " afterward (p. 84). Ring maintains that an "unacknowledged gender factor" also fueled the conflict over the Eichmann report (p. 170). Leaders in both communities "felt not only guilty and powerless about their responses to the Holocaust, but 'unmanned' by the power of the Nazis and what appeared to be the collusion of most of the Western world in the face of the slaughter of the European Jews" (p. 111).

To defend this controversial statement, Ring draws on George Mosse's analysis of the gender logic of modern nationalism, which allows her to explain Jewish male anxiety as a response to being cast as nationalism's effeminate outside. Drawing, in turn, on the work of Daniel Boyarin, Ring argues that this anxiety also has a psychic source: circumcision. An "emasculating wound" that reminds the Jewish male he cannot measure up to the (gentile) phallus, circumcision produces the compensatory response of aggressive assertion of gender difference and national difference (p. 122). (Note that Ring does not explain why circumcision would provoke fears of Jewish inadequacy in postwar America, where gentiles were routinely circumcised). In this context, Zionism provided for the assertion of national difference; Talmudic scholarship, with its "inherent and pervasive male bias," reaffirmed the immutability of gender difference (p. 252). By her dispassionate criticism, Ring contends that Arendt destabilized the nationalist and gender binaries that

these two communities used to shore up their anxious Jewish masculinity. Her criticism of the Eichmann trial was "tantamount to cross-dressing": "For the Israelis she was the 'strong Jewish woman' from the Diaspora, a combination they had not believed possible. For the New Yorkers, she was a Jewish woman who presented herself as a universal scholar" (p. 155).

With these arguments, Ring pioneers a way of thinking about the Eichmann text that moves it beyond well-worn debates about the nature of evil and into the politics of the current impasse over the Oslo agreement. The work is original, drawing on a body of scholarship that is both surprisingly (the material on Israeli politics in the postwar period) and predictably (the cultural critique of nationalism, Judaism, and sexuality) beyond the typical purview of Arendt scholarship. By her use of the cross-dressing metaphor, however, Ring brings to light an ambiguity in her account of Arendt's crime. Was it that Arendt "usurped" the masculine position (p. 170)? Or was it her "dismantling" of the very oppositional structure on which categories such as masculine identity and national sovereignty depend (p. 155)?

Although Ring wants to credit Arendt with "the deconstruction of accepted categories," she has represented the " 'problem' of Jewish masculinity" in a way that is at odds with this broader objective (pp. 170, 139). Briefly, Ring's attribution of Jewish male anxiety to a transhistorical imperative to "escape the stigma of femininity" presupposes that the boundaries between masculinity and femininity are static and that the challenge is to measure up to the phallus, thereby earning a place on the privileged side of the divide (p. 128). Feminist critics of psychoanalytic theory (such as Jacqueline Rose, Judith Butler, and Joan Scott) have observed that to equate "the phallus" with male power—as Ring does, apparently following Daniel Boyarin—is to reconfirm a stereotyped understanding of gender from the start. To these scholars, the phallus represents not an intimidating ideal of sovereign manhood but the impossibility of certainty regarding who one is in a linguistic and social system in which all meaning is relational and eludes closure. By contrast to those who persist in assigning a gender to the phallus, this view deconstructs the phallus as the paradigmatic instance of meaning-in-opposition: It is the precondition of the binary conception of gender that underwrites our stereotypes. Recourse to a deconstructed phallus might put Ring's argument about Arendt on firmer ground, so to speak. That she relies on a more literal understanding in no way diminishes her important contribution to Arendt scholarship, however. This is the first single-authored book that even opens up the possibility for this kind of argumentation by defying the proscription against studying gender as a factor in Arendt's life and writing.

Ring's point of departure in Part Two is the widely noted contradiction in Arendt's *Thinking*, the project she undertook to elucidate the theoretical commitments implicit in her widely misunderstood catchphrase, the "banality" of evil. It was Arendt's intuition that political evil occurred when people stopped thinking; consequently, thinking is and must be a worldly activity. Ring argues that Arendt's last work is "conflicted"; although she attempted to conceive of a kind of thinking that would enrich and be enriched by public life, Arendt could not relinquish the rarefied model of thinking as a solitary, pure activity that is an inheritance from Plato (p. 237). Ring advances another controversial thesis to explain this conflict: She speculates that it "may have reflected a conflict within Arendt, between the political Jew and the intellectual German" (p. 215).

Using Thorlief Bowman's treatise, *Hebrew Thought Compared to Greek* (1960), Ring schematizes the competing models of thought that she claims are at odds in Arendt's last work. She demonstrates that although there are affinities to dynamic and dialogical Jewish thinking in such works as *The Human Condition* and *Thinking* (which have no explicitly Jewish subject matter), Arendt's inability to give the Jewish aspects of her thought freer play doomed her quest for a worldly philosophy (p. 215). Ring contends that the reason for this inability was twofold. As a woman, Arendt could not have had access to "the exclusively male culture of the Talmud and Torah" (p. 36). Moreover, as a Jewish scholar trained in the German academy, she was "perhaps seeking 'parvenu legitimacy' in ways in which she was unaware" (p. 237).

Ring's argument in Part Two is well conceived, illuminating, and delightfully risky. Her portrait of Arendt as "a conflicted Jewish intellectual" gives contemporary Jewish scholars a point of identification with Arendt, affording them a more sympathetic way into her work than was previously available (and anyone who doubts the need for such a project should read the entry on Arendt at the Holocaust Museum in Washington, DC). Although the book is probably a calculated appeal to this foreclosed Jewish audience, I nonetheless wish that Ring had provided some discussion of the rationale behind a formulation that occurs repeatedly throughout Part Two. She chooses to characterize the tensions in Arendt's final work as testimony to "a divided loyalty between Arendt's Jewish soul and her German education" (p. 229; similar formulations at pp. 9, 215, 234, 259). By assigning priority to the fact that Arendt was Jewish, Ring's formulation discounts the fact that Arendt was also German, thereby erasing the fact that Arendt grew up in a Germany where it was possible to be both German and a Jew. After the war, Arendt sometimes acknowledged what an oddity it was to be something—a German Jew—that Nazism had practically rendered a contradiction in terms, but she allowed the puzzle that this identity posed to persist undecided. Although it may not be a flaw of Ring's study that she settles the question, the dialogic style that Ring prizes and exemplifies would be enhanced by some elucidation of her rationale.

Far from being—as Ring puts it—"another book about Arendt," this is a genuinely new study that Arendt specialists should read. Ring's writing makes it so lively as to appeal to newcomers as well.

Spinoza, Liberalism, and the Question of Jewish Identity. By Steven B. Smith. New Haven, CT, and London: Yale University Press, 1997. 288p. $30.00.

George M. Gross, *Temple University*

Benedict de Spinoza (1632–77) was a major figure in the formation of liberal political thought and in the emergence of the historico-critical study of the Bible, but the eclipse of liberal republicanism in continental philosophy under the influence of Romanticism caused his contribution to appear as a theory of pantheism. Spinoza scholarship has yet to recover from this disfigurement. Now Steven B. Smith seeks "to establish [Spinoza's] stature as a political theorist first and foremost" (p. xv), observing that "Spinoza's [*Theologico-Political*] *Treatise* is perhaps the least read work of political theory by a philosopher of the first rank, no doubt because it is overshadowed by the *Ethics*" (p. 13). One part of Smith's intended audience is the philosophy professors who stake a proprietary claim to Spinoza, but Smith directs his book mainly to political theorists in order to supply a pivotal, missing chapter in the history of liberal thought.

Smith wants to show that "Spinoza was passionately political" (p. xv; cf. p. 32), but not too political. Neglecting Spinoza's other mature work, the *Political Treatise*, Smith pronounces that Spinoza's theory of sovereignty "seems radically opposed to the Lockean notion of limited government or the *Federalist*'s scheme of constitutional checks and balances" (pp. 197–8); the view that "Spinoza favored a separation-of-powers doctrine with a considerable degree of pluralism . . . seems false to me" (p. 249, n97). Yirmiyahu Yovel and Harvey C. Mansfield, Jr., offered versions of this reading in the 1980s. Lewis Feuer and Robert McShea sketched contrary views in the 1950s and 1960s, but incompletely. The Spinoza controversy today in part concerns each of these issues.

Anyone who relies on the Smith-Mansfield-Yovel reading would be surprised to learn that Spinoza defines in constitutional terms what it is to execute (*exsequi*). Spinoza also contends that if two share political power, the polity is likely to fracture into two polar factions. But, Spinoza says, "the factions will be weaker, the more there are to whom the dominion [i]s delegated" (Spinoza, *Political Treatise*, chap. 8, sec. 1, Elwes tr.). A more limpid statement of the reason for political pluralism under constitutional government is hard to find. Smith recognizes that the constitutional encouragement of a proliferation of sects or factions is the hallmark of "the pluralist strategy" (p. 3), but he misses Spinoza's articulation of this strategy in the *Political Treatise*. In the *Ethics*, Spinoza employs "what was to become a standard stratagem of liberal political theory . . . by playing off one passion against another" (p. 129). Smith does not explore the linkages between the "standard stratagem" and "the pluralist strategy." Because philosophically he is predisposed to find new evidence of nuclear individualism in liberal theory, he misses the opportunity to establish Spinoza's pertinence to American political theory in the most direct way, maintaining instead that "Spinoza's form of liberalism is individualist but not pluralist" (p. 154).

How, then, is Spinoza political? He is significant for "his theology of the liberated individual" (p. xiii) and as a supporter of "the democratic republic because it is the regime most consistent with the autonomous individual or the liberated self" (p. 122). The liberated individual merges in Smith's narrative with the liberal citizen of the liberal polity (cf. p. 20), who is unmindful of the gap between the few and the many and, indeed, forgets the many, along with their fears and hopes. The premise of the book is thus that contemporary religion "caught liberals unawares" (p. 1). Smith turns to Spinoza as a progenitor of these bland, autonomous liberals. After characterizing liberalism's conception of liberty and civil peace as a Faustian bargain, because it is soulless, and as "something like an offer to buy the Brooklyn Bridge" (p. 22), because its promise of peace is chimerical, Smith concludes that it is "the worst solution except for all the alternatives" (p. 205).

Smith's second thesis, a subsidiary argument on account of how he develops it, is that "Spinoza provides us with an alternative route to liberalism by way of Judaism" (p. 23). That is, "the Jewish Question," famously associated with Karl Marx, already had received a full delineation in Spinoza's *Theologico-Political Treatise*; Enlightenment and nineteenth-century thinkers understood that not only Spinoza's writing but also his life epitomized this question. Thus, liberalism has its origins not just in religious wars or in the rivalries of Protestant sects, but in the issue of Jewish emancipation, an intrinsic concern of liberal political doctrine from the beginning. Smith rightly observes that Spinoza's excommunication by the Jewish community of Amsterdam in 1656 continues to be controversial and "can be seen as a virtual litmus test" for dilemmas posed by emancipation (p. 167). Unfortunately, Smith's tendency is to vindicate the Jewish community of Amsterdam. Spinoza's position that the hereditary priesthood is not intrinsic to Jewish law (cf. pp. 149–50) is a radicalization of republican tendencies within Judaism, and while it may ultimately be heterodox (cf. Exodus 19:6), this reviewer thinks that the verdict on Spinoza is closer than Smith pronounces.

As an inquiry into the origins of phenomena that Smith wants to understand, this reviewer would recommend Oakeshott's analysis of the "autonomous individual" (and of the "anti-individual"), which is more textured ("The Masses in Representative Democracy," in Michael Oakeshott, *Rationalism in Politics and Other Essays*, 1991). Smith's idiom is often Nietzschean. While Oakeshott speaks of "conditions so preeminently favorable to a very high degree of individuality" and which favored choices by Hobbes and Spinoza as to which human dispositions to cultivate and encourage, the idiom of Smith's reading is that Spinoza is a kind of alchemist (e.g., pp. xiv, 51), or a sorceror's apprentice. Some time in the remote past, Oakeshott's thesis on the "autonomous individual" may have been a motivation for Smith's study, but that past is forgotten now.

Smith's reconstruction of the multifacetedness of Spinoza's readership in chapter 2 is good, but it is unnecessary for him to invoke Leo Strauss's esoteric writing thesis. Strauss is germane in the succeeding chapters on the religious sociology of Spinoza's Scriptural hermeneutics. Smith shows that Spinoza advocates disarming clericalism and taming religion from the perspective of the civil state, but his emphasis on Spinoza's rhetoric leads him to be overly cynical about Spinoza's Biblical exegesis, some of which is fertile and interesting.

Liberal political theorists may find Smith's volume useful as an introduction to Spinoza. There are disappointments, however. Smith's neglect of Spinoza's constitutional theory is partly due to his following the lead of scholars whose aim was not, after all, to interpret Spinoza's political thought. Smith repeats the possibly true (but too easily asserted) dictum that "the liberal paradigm . . . begins with Locke," while showing that elements of liberal thought are present in Spinoza's thought (p. 164); when one adds elements of liberal thought that are present in Spinoza's thought and that Smith overlooks, as well as the signature concept of the *homo liber* himself, one is outside the compass of Smith's discussion.

In sum, this reviewer finds that the lapses in Smith's account of "Spinoza's form of liberalism" are not incidental. Smith does for the "friends of liberalism" what J.G.A. Pocock and Eco Mulier (writing in the 1980s) did for the "civic republicans": He shapes Spinoza according to preconceived ideas. In writing a sequel (pp. xvi, 252, n7), some autonomy could be helpful.

Machiavelli's Three Romes: Religion, Liberty, and Politics Reformed. By Vickie B. Sullivan. DeKalb: Northern Illinois University Press, 1996. 249p. $30.00.

Benedetto Fontana, *Baruch College, CUNY*

Machiavelli's treatment of religion has sparked numerous controversies and debates among his interpreters. Some, like Hulliung and Parel, see Machiavelli as a pagan, while others, like de Grazia, regard him as a Christian. Still others, like Germino, see ambiguous and ambivalent nuances in a religious view that is still fundamentally Christian. At the same time, whether or not Machiavelli sees religion as an instru-

ment of rule, and thus subordinated to the needs and demands of politics, is yet another contentious issue.

Vickie Sullivan's book is an important and useful contribution to this literature. It combines meticulous scholarship with provocative and insightful analysis. While issuing directly from this literature, and thus in a sense the intellectual heir of these debates, it attempts to overcome their dualistic character by presenting an alternative and original interpretation. In a manner reminiscent of Machiavelli himself, Sullivan uses the arguments of her forerunners precisely in order to subvert them. She argues, with Skinner, Pocock, and others, that a central strand of Machiavellian thought is liberty and its embodiment in political institutions. Yet, she also maintains, with Strauss and Mansfield, that Machiavelli's attack on Christianity is a fraudulent pretense devised to veil his use of Christian methods of rule. Sullivan shows how the use of pagan religion not only undermined liberty in republican Rome but also provided the necessary political and social elements for the emergence and triumph of Christianity (pp. 66–77, 147–8). Although pagan religion led to the tyranny of the caesars (pp. 81–2, 114–7), biblical religion produced a far more formidable tyranny in the rule of Christianity and its political and institutional embodiment in the papacy and clergy (pp. 49–55).

In the process Sullivan provides a novel and intriguing perspective to both the Straussian and civic republican interpretations of Machiavelli. Running throughout Sullivan's reading, just below the surface, yet emerging at various points in her argument, are the traditional Machiavellian themes of political order and political liberty, the relative utility of fraud and force, the autonomy of politics, and, most important, the strife and competition for power between the few (the "great") and the many (the "people"). It is the desire of the people for the unseen and imaginary goods promised by religion (generated by ambitious "potential tyrants") that destroys constitutional order and republican liberty. Conversely, order and liberty are attained and maintained by transforming the "transcendent" hopes and fears inspired by religion (whether Christian or pagan) into those of a worldly and political kind.

In effect, two major themes carry the argument forward. First, since Machiavelli's thought is neither pagan nor Christian, but instead incorporates in his project to reform the world a vision of politics informed by elements taken from the pagan and Christian worlds, Sullivan stresses the temporal and earthly nature of this enterprise. Second, against the "almost universal view" that religion is a useful and necessary instrument of rule, she maintains that to Machiavelli it is both "pernicious" and "wholly superfluous" (p. 7). Religion is dangerous precisely because tyrants and "potential tyrants" will use it to destroy liberty; and it is superfluous because a more useful and more reliable remedy is at hand. Religion invokes fear of the gods, politics fear of human beings. Like the Epicureans, Machiavelli "intends to free" humankind from the fear of the gods; but unlike these philosophers, who retreat from the problems and cares of the city into the private life of contemplation, Machiavelli "shuns philosophy in the name of politics" (p. 11). Fear of the gods is replaced by fear of the prince. The nature and role of fear is central to Sullivan's argument: She moves from fear of the gods, to fear of the prince, to the mutual fear of ambitious and contending leaders ("potential tyrants"). The foundation of liberty is thus a balance of mutual and reciprocal fear. The relation between fear and virtù is important, and an extended discussion of its historical and intellectual lineage, in Sallust and Augustine, for example, would have been fruitful (especially since the author underlines the similarities and parallels in method between pagan and Christian Rome).

Sullivan deftly weaves together the question regarding religion as a political instrument used by the rulers (the few) to dominate and control the people (religion as *religio*) with the problem regarding religion as belief or transcendent faith (*fides*). In so doing, she argues that Machiavelli's "new modes and orders" are the outcome of a pagan critique of Christian Rome and of a Christian critique of pagan Rome. Using the *Prince*, the *Discourses*, and the *Florentine Histories* as her main texts, Sullivan discerns in Machiavelli three cities: pagan Rome (discussed in Part II), Christian Rome (Part I), and Machiavelli's Rome (Part III). Unlike the first two Romes, the third exists entirely in Machiavelli's political and historical imagination, which is itself the product of a selective and critical reading of pagan and Christian thought and practice. Machiavelli's third city is a synthesis of antagonistic yet parallel elements drawn from the two cities of pagan and Christian Rome. But to say that this third Rome is imaginary is to recall Machiavelli's famous critique of those who have "fancied for themselves republics and principalities that have never been seen or known to exist in reality" (*Prince*, p. 15), which is thus a subtle and tacit use of Machiavelli's method against him. Machiavelli, like prophets and religious reformers, also seeks to entice the many with things unseen and lands unknown.

Religion and Contemporary Liberalism. Edited by Paul J. Weithman. Notre Dame, IN: Notre Dame University Press, 1997. 315p. $48.00 cloth, $23.00 paper.

Robert Lowry Clinton, *Southern Illinois University at Carbondale*

These 11 papers were presented at an interdisciplinary conference at the University of Notre Dame in February 1995. As Paul J. Weithman notes in his introductory essay, the central theme of the collection is the effect of what has been called the liberalism of reasoned respect upon religious argument and religious activity in the public forum. Found in the works of John Rawls, Robert Audi, and others, this variant of liberalism asserts that establishment and maintenance of a just and well-ordered society demand that constitutional arrangements and public policies, broadly defined, be based only on "public reasons." Public reasons are those reasons to which all citizens may be expected reasonably to subscribe, at least in the pluralistic Western democracies that have accepted the "settlement" ending the wars of religion in the wake of the Protestant Reformation. This settlement featured the transfer of religion from the public to the private sphere, emphasizing individual freedom of conscience and church-state separation. The resulting religious pluralism means that public reasons cannot be religious reasons because, by definition, such reasons are "plural" under religious pluralism. These considerations have profound implications for the conduct of public political discourse today.

Some of these implications are spelled out by Audi in the essay that follows Weithman's introduction. Audi argues that the liberalism of reasoned respect requires separation not only "between religious institutions and the state" but also "between religious and secular considerations" (p. 38), and he sets forth principles of "secular rationale" (p. 55), "secular motivation" (p. 56), and "ecclesiastical political neutrality" (p. 64). The first two principles, read together, assert that one is obligated "not to advocate or support any law or public policy that restricts human conduct" unless one has, is willing to offer, and is motivated by "adequate secular reason" (pp.

55–6). The third principle holds that churches in a free society are under an obligation not to support candidates or policies that restrict human conduct.

Audi's "gag rule" is both applied and qualified in the essays by Sanford Levinson and Martha Nussbaum. Levinson argues that judicial nominees may appropriately be disqualified unless they satisfy the appropriate confirmation committee that they will resist "the temptation to treat religious sources as containing any privileged statements of moral truth" (pp. 91–2), but he stops short of extending the gag rule to nonjudicial public officials, politicians running for office, or citizens generally. Nussbaum, emphasizing the rights of women in an international context, suggests that something like the liberalism of reasoned respect should be extended globally in order to ensure fundamental human rights for all persons. To this end, Nussbaum argues that systems of religious law like those in force in Iran, Pakistan, or the Sudan should not be permitted (p. 121), that "any form of religious discourse that constitutes a threat of violence against an individual or group" should be criminalized (p. 131), and that it should be regarded as immoral "to speak in ways that contradict or undermine" fundamental rights (p. 133).

The remaining essays in the collection challenge, to one degree or another, the ideas presented by Audi, Levinson, and Nussbaum. Philip Quinn denies that there are "moral duties to refrain from appealing to religious reasons" in political contexts (p. 139), because, on such important issues as that of abortion, secular reasons are simply not strong enough to establish a fair consensus (p. 150). Nicholas Wolterstorff suggests that the strictures placed on public argument by proponents of secular reason are self-contradictory because they show disrespect for persons who do not choose to divide their lives "into secular and religious components" (p. 176). For Timothy P. Jackson, the liberalism of reasoned respect, even in its best formulations, may pose a larger threat to "genuine pluralism" than religion poses (p. 184).

The last four essays in the collection travel beyond the argument about the liberal silencing of religion in the public square to broader concerns. J.L.A. Garcia argues that the extensions of liberal freedom in the final third of the present century, particularly sexual freedom, demonstrate the moral bankruptcy of a liberalism devoid of metaphysical or religious foundations (p. 221). Jean Elshtain argues that religious faith is an essentially public, not private, activity (p. 253), and "much of the ongoing debate that poses religion against liberalism is a distraction from, rather than a contribution to, our grappling with the crisis of authority in late modernity" (p. 263). The theme of a "public church" is echoed by John A. Coleman, who, on the basis of extensive field research of his own and the work of such scholars as Robert Putnam, concludes that active, public religious activity is needed to strengthen civil society. According to Coleman's analysis, the main problem with a liberalism that excludes religion from the public square is that "discipleship" is a stronger motivator than "citizenship" (pp. 290–1). Thus, if liberals really want to strengthen civil society, feed the hungry, clothe the poor, and heal the sick, they will need to make common cause with religious institutions. Finally, David Hollenbach closes the volume with a plea for liberals to stop focusing on the conservative Christian Right and pay more attention to the Christian Left, which he claims has been slogging away in the trenches all along.

There is much that is valuable in this collection. Readers who are interested empirically in the role of religion in public affairs will find the essays by Coleman and Hollenbach especially rewarding. Readers interested in more general theoretical discussions of religious involvement in civil society will enjoy the essays by Jackson and Elshtain. Those who want to access the ongoing debate between Rawlsians and religionists over the exclusion of religion-based arguments from public political discourse will find no better introduction than the essays by Audi, Quinn, and Wolterstorff. Those who want to explore the implications of liberalism for legal policy, global human rights, or social mores will learn from the essays by Levinson, Nussbaum, and Garcia.

It is characteristic of collections of this kind that they tend to suffer from the defects of their virtues and make for difficult reviewing. Promising initially to center upon the relation between religious and political discourse, the volume loses this focus about two-thirds of the way through, and the essays take off in various directions, each shaped by the diverse proclivities of the individual contributors. So while much ground is covered, the reader is left to wonder whether real progress has been made in resolving the issue that spurred the conference in the first place. As Weithman says in his introductory essay, the discussions in the collection seem to show that the relationship between religion and liberalism is far more complicated than usually imagined, and that the view of many contemporary liberal thinkers—that an "unbridgeable gulf" exists between the two world views—is seriously mistaken. But that depends on what "liberalism" really means; and the collection of essays presented here does not suggest that this question is likely to be answered anytime soon.

American Politics

Making Crime Pay. By Katherine Beckett. New York: Oxford University Press, 1997. 158p. $27.50.

William E. Kelly, *Auburn University*

The effects of crime on American society are quite interesting for the serious student of our criminal justice system. They cause a government to react in a variety of ways that are both praised and criticized by a public which experiences the negative aspects of crime. This is understandable, because no solution to the problem is acceptable to all of us. In fact, the many attempts made to combat crime have not been successful in eliminating it, and perhaps that is a major reason its role and effects are of so much interest. In *Making Crime Pay*, Katherine Beckett contributes a valuable analysis.

A main point of this work is that the public's perception of crime is more a result of the rhetoric emanating from politicians than of crime itself. This interesting view is well documented by an abundance of sources. It leads to a number of important and interesting contributions, such as the capacity of the media and politicians to set the political agenda of this country. Once that agenda is established, it is easy to ascertain why public policy reflects a particular value system. Beckett suggests that the political agenda is established by politicians who see much more personal value in alluding to the existence of crime in our society.

This book makes a number of substantial contributions to

the study of American crime. It begins with historical background on how certain types of politicians and law enforcement personnel used the civil rights movement to justify an increase in law-and-order programs. The author also identifies the effects of the increased political rhetoric advocating the need for more criminal justice sanctions. Specifically, she draws attention to the reallocation of public funding from welfare services to social control goals, such as increased incarceration. She also examines the growing trend among the general public to support more crime control policies, such as increased authority of the police, prison construction, and harsher sentences in terms of both length of imprisonment and types of punishment.

This book is valuable for a number of reasons. It shows that crime is very important to an aspiring politician in gathering public support. Advocating a strong law-and-order position often is quite successful because the public has been led to accept a need for it. The book also explains some of the factors behind the huge increase in public funding for the criminal justice system in the hope that this will somehow reduce crime. There has not been much resistance from taxpayers, who have been exposed regularly to political rhetoric about the danger from criminals among us.

Beckett also calls attention to the fact that certain individuals associated with the criminal justice system benefit from its expansion and increased public support. Aside from jobs for law enforcement and correctional personnel, she notes benefits to the private sector, such as privatization of prison management. She believes the penal-industrial complex will continue to support expansion of public funding on crime control for its own financial gain. Beckett could have given more emphasis to the fact that even higher education is a beneficiary of the increased spending, since criminal justice personnel are encouraged to upgrade their credentials.

This work does an excellent job of demonstrating the huge influence of the media in our society. As Beckett suggests, the media may not tell people how to think, but they are very important in giving them something to think about. Another important lesson to be learned from this book is that when politicians use the media to call our attention to crime, we can expect more resources to be devoted to it. This also implies that certain segments of our society will benefit, and others will probably suffer. As noted earlier, when more public resources are shifted to crime control, the social welfare sector pays part of the cost.

This book should be of interest to several different groups. It will give those who work in the criminal justice system a better idea of what to expect in the near future about the views of the general public regarding crime. It also will be of value to students of the criminal justice system and the role of crime in our society, because it will help them understand how and why our government is responding to this serious social problem. Finally, for students of the political system, it shows how politicians exploit an issue of public concern and use the media to further their own objectives.

Fortress America: Gated Communities in the United States. By Edward J. Blakely and Mary Gail Snyder. Washington, DC: Brookings Institution Press; Cambridge, MA: Lincoln Institute of Land Policy, 1997. 209p. $24.95.

John McGeady, *Purdue University*

America's social fabric has begun to rip. Our sense of public community and citizenship, or *civitas*, is fading. As we fragment as a nation, we have become more privatized, atomized, and localized, and as a result we are losing our ability to envision, let alone secure, the common good. With these statements, Blakely and Snyder join a host of scholars, journalists, and politicians who have sounded the alarm of civic decay in recent years. What establishes *Fortress America* as an original and valuable work, however, is the authors' focus on what they claim to be a dramatic and important manifestation of this civic decline: the rise of the gated community.

Blakely and Snyder argue that gated communities reflect the social fragmentation and civic atrophy currently plaguing our nation. Their argument has two general thrusts. First, gated communities greatly limit social contact among members and groups of society. Lower classes are excluded not only from living in these residences (through prohibitive housing costs) but also from even casually passing by. And, the authors note, such class separation is often de facto racial segregation. This loss of social contact and interaction between different classes and races is "narrowing the bonds of mutual responsibility and the social contract" (p. 139). Such social distance can lead to stereotyping and misunderstanding between societal groups, which in turn lead to mistrust, fear, and a further weakening of the civic bonds among Americans.

Second, gated communities are characterized by a form of "private" government: homeowner associations. Because the normally public space within the community has been rendered private by gates and walls, these associations assume responsibility for such services as policing and maintenance of streets, parks, and recreation facilities, and they have the power to levy fees to pay for them. This privatization of local government, according to Blakely and Snyder, is leading to what Robert Reich has called the "secession of the successful." The affluent members of gated communities are questioning whether they should pay taxes for public programs and services which no longer benefit them. For many in gated communities, social connection and sense of common interest end at the walls of their residence; thus, they abdicate any responsibility for less fortunate members of society. Such secession, Blakely and Snyder argue, is poisonous to a society and democracy based on mutuality and collective citizenship (see Robert B. Reich, *The Work of Nations*, 1991, or Evan McKenzie, *Privatopia*, 1994).

This social fragmentation and secession need not be the future of American communities, according to the authors. There are ways to assuage the fear of urban and social decay that drive some into gated enclaves and to reinforce the inclusive and broad systems of community that are the bedrock of society. In the final chapter, the authors detail what they feel are three promising approaches: a set of security measures called Crime Prevention through Environmental Design (CPTED), neotraditional urban design, and more comprehensive and integrated urban development planning on the regional level.

Fortress America has much to recommend it. It is the first book devoted entirely to the study of the gated community phenomenon, the social and political ramifications of which should not be taken lightly. Blakely and Snyder estimate that three million households are already living in such developments (p. 2), and according to one expert, eight out of every ten new urban developments are gated (p. 7). The authors develop a helpful typology of gated communities (lifestyle, prestige, and security zone), and they exhaustively explore these types, and the motivations of their residents, in three respective chapters.

Blakely and Snyder also do well to characterize the phenomenon of gated developments as not a desperate and dramatic revolution in community design but, rather, as a

logical, evolutionary step in the suburban ideology of territorial control. Features of community design widely accepted as standard around much of the country, such as single-use zoning, cul-de-sacs, inaccessibility to public transit, and housing market segmentation, all reflect this ideology of privatization and exclusion. Yet, the authors note, gated communities are unique and warrant special attention: For example, they provide a very visible and powerful symbol of exclusion to people on both sides of the walls and gates.

One weakness of the book is its data. Although Blakely and Snyder collect an impressive array of ethno-descriptive and statistical evidence, including site visits, focus groups, interviews, and surveys, ultimately little of it is very illuminating. For example, in a survey of gated community board members, the authors found that "only 6 percent reported that residents were 'very active' in governance" (pp. 132–3). How does one judge this percentage without the equivalent rate for nongated communities? Furthermore, what is considered "very active" may well differ greatly from one board member in one community to another. These and other research flaws detract from the authors' otherwise persuasive arguments.

Elsewhere, Blakely and Snyder strangely back off from their central argument. Repeatedly, they state that gated communities are not a cause of the social fragmentation that is weakening our sense of public community but simply a manifestation of it (pp. 3, 156). This is an odd statement, since the authors also argue that gated communities "allow some citizens to secede" (p. 3), "create . . . barrier[s] to interaction" (p. 153), may "lead to stereotyping and misunderstanding" (p. 138), and "may be reinforcing people's fear" (p. 150). These statements imply some degree of causality. Indeed, implicit throughout the book is the idea that, though prior fragmentation and civic disconnection prompted the gated community trend, these developments through their ecological effects have become a contributing cause to the perpetuation and exacerbation of this social breakdown.

Finally, Blakely and Snyder raise some fascinating questions that deserve attention. To what extent is the growth in gated communities being driven not by consumer demand but by housing developers? Do gated residences represent a growing commodification of housing and community itself? Do they, and their homeowner associations, embody a societal trend of ceding forms of social responsibility to professionals to the detriment of community? And is our need for others in society lessening?

Fortress America is intended for a wide audience, and so a political scientist will be slightly disappointed in its superficial treatment of these theoretical issues. This aside, Blakely and Snyder have produced a thought-provoking first look at the civic dimensions of the gated community phenomenon. Though they provide few answers, the authors ask the right questions and lay out the terrain for future inquiry.

The Other Elites: Women, Politics, and Power in the Executive Branch. Edited by MaryAnne Borrelli and Janet M. Martin. Boulder, CO: Lynne Rienner, 1997. 265p. $55.00.

Susan J. Carroll, *Rutgers University*

As scholars of women and politics have gained a stronger foothold in the discipline of political science, most aspects of American politics have been examined through a gendered lens. The presidency is one of the few major areas of research to have largely escaped such scrutiny, in part because a woman has never held the nation's highest office and in part because relatively few feminist scholars have focused their research on the executive branch.

The Other Elites is the first comprehensive attempt to examine executive branch politics from a gendered perspective. This pathbreaking anthology, which bridges the fields of presidency studies and women and politics, illustrates the relevance and importance of gender as an analytic category for examining presidential politics. The contributors include several established scholars in the field of women and politics. They examine the increasing presence of women in positions of influence in the executive branch, analyze the politics surrounding their appointments and the context in which they serve, and assess the ways in which gender has influenced relations between the executive and other branches of government as well as public perceptions related to the presidency. Most of the analysis in this volume draws upon and blends together the insights offered by both traditional studies of the executive branch and the growing literature on women and politics.

The Other Elites evidences both the strengths and the weaknesses one might expect in a first attempt to expand research into a new area. On the one hand, it covers many different aspects of presidential politics, presents stimulating new information and analyses, and offers original and memorable insights. On the other hand, the chapters are uneven in quality, some essays fit the volume less well than others, and a few critical aspects of presidential politics receive little or no attention.

The book is divided into four parts. The first section places the study of women in the executive branch in both a theoretical and comparative context. Georgia Duerst-Lahti's essay provides an overall framework for the volume, showing the many ways in which masculinism as an ideology undergirds power relations in the executive branch. Her analysis helps to illuminate why women are considered the "other elites" and suggests several important hypotheses regarding the appointments women are most likely to hold and the obstacles they are likely to confront in the executive branch. Piper A. Hodson's interesting analysis of women prime ministers in South Asian countries functions less well in establishing a context for the essays that follow. Despite Hodson's commendable effort to make her analysis appropriate for a volume focused on U.S. politics, her examples of women whose powerful family connections figured in their rise during times of great national instability in Third World countries have only limited relevance for the United States.

The second section, which is the most clearly focused and arguably the strongest, examines women as political appointees. Janet M. Martin presents empirical data on the background and career paths of women appointees and provides a very useful overview of the rhetoric and records of recent presidents regarding their appointment of women. Through a detailed examination of the early Clinton administration, MaryAnne Borrelli analyzes the politics surrounding the appointment of presidential cabinets in a climate in which women and people of color are making increasing claims for representation. Kathryn Dunn Tenpas examines the presence and influence of women on the White House staff since the Roosevelt administration, documenting the persistent glass ceiling that excludes them from the president's inner circle. Finally, Nancy E. McGlen and Meredith Reid Sarkees examine the inclusion of women in the foreign policy establishment during the Reagan, Bush, and Clinton administrations. They argue that presidential direction, appointment practices, and policy management style all influence the role that women play in shaping foreign policy.

The essays in the third section focus on two of the many relationships between the presidency and the other branches of government: the confirmation process in the Senate and

appointments to the federal judiciary. Jean Reith Schroedel, Sharon Spray, and Bruce D. Snyder present a case study of the Senate confirmation of Roberta Achtenberg, the first open lesbian to be appointed to a top federal position, as assistant secretary of fair housing and equal opportunity in the Department of Housing and Urban Development. Richard L. Pacelle, Jr., whose essay has much in common with those in the second section, analyzes the appointment of women to federal judgeships and highlights the contributions of Jimmy Carter and every subsequent president in increasing the presence of women in the federal judiciary.

The final section examines women as executive activists and as citizens. It includes an excellent essay by Barbara C. Burrell on the role of the First Lady as an adviser to the president and as a policymaker in both foreign and domestic contexts. Burrell explores the contributions of First Ladies and the questions surrounding their accountability. In his second contribution to the volume, Pacelle assesses the role Office of Solicitor General has played in litigation involving affirmative action and abortion. Finally, in a broad-ranging essay on the gender gap in public opinion, partisanship, and voting behavior, Clyde Wilcox reviews relevant survey data and research findings on the electorate's willingness to vote for a woman for president and voter perceptions of women candidates.

The individual essays in *The Other Elites* are not as interrelated and clearly focused on central themes as one might like. For example, despite the fact that Duerst-Lahti provides a fine theoretical framework for thinking about the gendered nature of executive branch politics, few of the essays refer to her concept of masculinism even when it is clearly relevant. In addition, the reader will find many subjects missing from this book. For example, no chapter focuses on women campaign staff or the way that gender influences decision making in presidential campaigns. Similarly, there is little attention to the actual influence on public policy or decision making processes of women civil servants or appointees in the executive branch. Nevertheless, in an area of so little research, Borrelli and Martin have provided a strong foundation on which future scholars can build.

The Other Elites would be a very useful supplemental text in courses on the presidency and women and politics. Consequently, the decision by the publisher to issue this book only in a cloth edition is puzzling. It is hoped that an affordable paperback version will be published for classroom use.

Overall, this volume represents an excellent initial effort to show the relevance of gender for analyzing various aspects of executive branch politics. Borrelli and Martin are to be commended for recognizing and responding to the need for a comprehensive examination of the presidency from a gendered perspective. It is hoped that many scholars will follow their example and devote increasing attention to analyzing the multifaceted ways in which gender is important for understanding presidential politics and the executive branch.

Justice Antonin Scalia and the Conservative Revival. By Richard A. Brisbin, Jr. Baltimore, MD: Johns Hopkins University Press, 1997. 474p. $39.95.

Sue Davis, *University of Delaware*

Books on sitting justices of the Supreme Court have grown more common in the last ten years with the publication of works examining the judicial decisions of William H. Rehnquist, Sandra Day O'Connor (e.g., Nancy Maveety, *Justice Sandra Day O'Connor: Strategist on the Supreme Court*, 1996), and Antonin Scalia (e.g., David A. Schultz and Christopher E. Smith, *The Jurisprudential Vision of Justice Antonin Scalia*, 1996). As Richard Brisbin has conceded, publishing a book on a sitting justice "is a dangerous proposition" because the justice may veer in a new direction at any time (*Law and Politics* book review, May 1997). Nevertheless, there is a high level of interest in current justices. Studies of individual justices, whether present or past, are important insofar as they provide information about the work of a single justice. The best of those studies, however, go farther by providing a framework that promotes an increased understanding of judicial decision making. As I shall explain, Richard Brisbin's book is just such a study. Brisbin's analysis of Scalia's voting behavior and his interpretive analysis of the justice's opinions is comprehensive. It includes the cases in which Scalia has participated in the areas of separation of powers, federalism, equal protection, the First Amendment, the rights of the accused, reproductive rights, and property. If the author merely provided an empirical analysis of Scalia's votes and reviewed a large number of his major opinions, he would make a solid contribution to the literature on sitting justices. The analytical framework that Brisbin has developed enables him to go considerably farther, however.

Brisbin sets forth his framework by offering three propositions about Scalia's behavior on the Court. First, his votes advance his conservative political ideology. Second, in his opinions Scalia employs a mode of jurisprudential discourse, reasoned elaboration (briefly, judges should follow the rules of law devised through a lawmaking procedure defined by the democratic majority and should use traditional modes of judicial decision making established by precedent), to buttress his conservative attitudes and provide legal doctrines for the future. Third, Scalia's constitutive discourse evidences his assumption of the legitimacy of the rule of law.

Brisbin assesses the first proposition with an analysis of Scalia's voting behavior, which reveals that he is the second most conservative member of the Court overall, although he is less conservative on federalism and taxation issues. Proceeding with a thorough examination of the justice's opinions, Brisbin demonstrates that Scalia has usually adhered to the principles of reasoned elaboration. He has departed from the mandate of judicial passivity in the face of majoritarian decision making, however, in order to prevent the expansion of interest group participation and moral discord. Or as Brisbin explains, Scalia has rebuilt reasoned elaboration to serve contemporary conservative political objectives. Thus, Brisbin manages to resolve several familiar inconsistencies in Scalia's decisions. For example, although he is committed to judicial deference to legislative decisions, Scalia has voted to strike down legislation that protects disadvantaged minorities (see, e.g., *RAV* v. *St. Paul* and *Austin* v. *Michigan Chamber of Commerce*). After examining Scalia's opinions in the area of equality, Brisbin speculates that the justice has "simply expressed a political vision hostile to governmental action against racial and gender subordination" (p. 180). More specifically, as Brisbin points out, Scalia has transvalued the strict scrutiny doctrine, using it to defend powerful groups rather than the disadvantaged specified in the *Carolene Products* footnote. Similarly, the positions that Scalia has taken in First Amendment cases illustrate how he has attempted to reduce the legitimacy of discussion and actions that promote difference and alternative visions of the political future of the regime. Indeed, Brisbin argues that Scalia's opinions call into question the value of dissent against decisions of the state.

Brisbin is at his most creative and critical in chapters 9 and

10, which are devoted to demonstrating his third proposition. He explains how Scalia's opinions reflect his commitment to the rule of law—a predictable guide for social and political behavior—and to the politics of modernity (particularly individualism and reason). But it is all an artifice, Brisbin claims, for Scalia's decision making endorses the vision of American politics favored by the conservative revival: "executive policy leadership and a reinforcement of the status of interests that are already powerful" (p. 325). Although his framework and analysis provide plausible explanation of Scalia's decision making, Brisbin is unlikely to have the last word. In light of the continuing interest in Scalia's work on the Court along with his relative youth, it is reasonable to expect that the future will bring a number of studies of the justice's decision making from a variety of perspectives. Nevertheless, Brisbin's insightful examination sheds considerable light on the way that several factors, including an ideological agenda, a political philosophy, and a legal methodology, converge to shape Scalia's votes and opinions. A coherent, understandable Scalia emerges. When all the analysis is complete, however, Brisbin's Scalia remains consistent with the attitudinalists' Scalia insofar as the driving forces behind the justice's decision making are ideological, founded on his conservative vision of American politics.

Brisbin's framework has potential beyond the present study of Justice Scalia. Not only is it likely to be useful for analyzing Scalia's future votes and opinions, particularly if he appears to take a new direction, but also it might be used to examine the decision making of other justices. Especially promising in this regard is the way that Brisbin juxtaposes Scalia's reasoned elaboration against pragmatic liberalism. The contrast might provide the framework for comparing the decision making of two or more justices. Likewise, the Warren Court's pragmatic liberalism might be contrasted with other approaches that capture the jurisprudence of the Burger and Rehnquist Courts. At any rate, Brisbin's framework clearly provides an important addition to the growing body of literature that uses a variety of methods to understand judicial decision making.

The International Origins of the Federal Reserve System. By J. Lawrence Broz. Ithaca, NY: Cornell University Press, 1997. 269p. $35.00.

Andrew J. Taylor, *North Carolina State University*

For J. Lawrence Broz, the sources of the Federal Reserve Act of 1913 can be found in the internationalization of the American economy. Rather than suggest that the Fed's foundation furthered solely the interests of the money center banks, Broz argues that, to a large extent, the Federal Reserve System provided critical public goods in the form of a stable payments infrastructure that was to make regular financial panics a thing of the past. This is not to say that New York banks were conspicuously absent during the formation of the Federal Reserve Act. Indeed, Broz does not question the conventional wisdom that such banks did the heavy lifting in the policy process. But he does reject James Livingston's influential argument (*Origins of the Federal Reserve System*, 1986) that rent-seeking Wall Street forced the legislation on Congress so as to increase the entry barriers for state-chartered banks that wished to be competitive. Broz shows, for example, that the act neither altered the right of banks to choose between state and national chartering nor removed the extant statutory obstacles to branch banking that were so ferociously protected by the small banks.

Why, then, did the big national banks work so hard? The answer, Broz writes, is that the Federal Reserve Act unleashed the process of internationalizing the dollar. The U.S. dollar already had the potential to be one of the few currencies of international commerce in 1913. The U.S. economy and share of world trade were growing rapidly. There existed, however, critical domestic institutional impediments to internationalization. The establishment of the Fed removed these barriers by (1) iterating the nation's commitment to price stability through adherence to the gold standard and (2) creating more sophisticated discounting and entirely new rediscounting mechanisms that were to provide the requisite liquidity in U.S. financial markets. New York banks especially benefited from internationalization of the dollar by collecting the commissions and fees that go along with the issuance of international currency and by saving the transaction costs that accompany the conduct of international commerce in foreign money. This story is, for the most part, quite new.

Broz wraps his argument in a theoretical approach that borrows heavily from the thinking on collective action by Mancur Olson. Whereas Livingston, for instance, conceptualizes the Federal Reserve Act as an example of the "privileged group" model of policymaking, Broz talks of a "joint products" approach. The latter framework better explains the legislation because, "by conjoining private benefits with the production of public goods, central bank charters internalized a portion of societywide benefits within a subset of the community, creating incentives for these individuals to demand and supply institutions that had wider social benefits" (p. 246).

The book unfolds in six substantive chapters that, among other things, describe the content of the Federal Reserve Act, explain the internationalization of currencies, mark the growth of the U.S. economy, reveal how banking interests lobbied for the legislation, and apply the theoretical framework to the founding of other central banks, such as the Bank of England. A detailed explanation of Broz's theory in an introduction and summary act as bookends to these chapters.

This is all intelligent and interesting stuff. There seem to me, however, to be two major weaknesses. The first is that Broz is too enthusiastic about the theoretical contribution of the book. The "joint products" model is advertised as qualitatively different from the usual exchanging of specifically targeted rents for pressure exerted by a self-interested and narrow private concern. But is this really the case? The difference between the two approaches could easily be seen as one merely of degree. Broz's story could be construed as the empirical vindication of a variation on the "privileged group" theory—the very theory he rejects. Here, the "public good" of a stable payments system could be interpreted as part of the rents received by large private banks because it is inextricably linked to the pay-off of the internationalization of the dollar. Just because others benefit from policy change does not mean that it cannot be viewed primarily as favoritism to a particular sector. After all, tax breaks and subsidies to individual industries also benefit those members of the public who happen to be their customers. No one is particularly puzzled to find out that consumers did not lobby for the industry's special treatment, however. What is more, I do not think the model is particularly unique. Economic actors will often support government policy aimed at promoting the public interest—perhaps literacy or an antihunger program. Their support is often linked to desired rents through a process of logrolling with other interests or politicians. Healthy public relations, as well as the received rents, make it worthwhile to internalize costs unrelated to private gain. All this makes me feel that Broz's most valuable contribution

is empirical rather than theoretical. The international connection is of most interest in this book, not the spin on Olson.

My second criticism is that Broz's explanation for the Federal Reserve Act is far too sociocentric. In many ways, therefore, this is another dig at his theoretical approach. To be sure, chapter 5 describes the statute's experience through the tortuous legislative process in Washington, but for the most part Broz has a "black box" understanding of American government. The burgeoning political development literature surely provides new avenues by which to explore theoretically interesting explanations of the Fed. How did the legislation fit into the systematic expansion of federal administrative practices that was occurring at the time it was passed? It seems to me that the Fed is an integral part of this pattern. Instead, Broz revisits the well-trodden path of political change propelled by private interest. He also fails to examine the important issue of delegation. Why did Congress decide to create an agent—and a fairly autonomous one at that—to make critical monetary policy? I wonder if it is possible to answer such provocative questions with Broz's "international" argument.

All in all, *The International Origins of the Federal Reserve System* is an extremely interesting, assiduously researched, and well-written book. It uncovers valuable and new information connected to the establishment of the Fed. I recommend it unequivocally for all students of international and American political economy as well as those who are interested in the creation and development of political institutions.

Bitter Harvest: FDR, Presidential Power and the Growth of the Presidential Branch. By Matthew J. Dickinson. New York: Cambridge University Press, 1996. 267p. $49.95.

Joseph A. Pika, *University of Delaware*

Analysts of presidential staffing and presidents themselves have long ignored the administrative strategy of Franklin D. Roosevelt. Among academics, the prevailing consensus holds that Roosevelt's system of "competitive adhocracy" (p. 205) was enormously inefficient. Generalist advisers, cabinet secretaries, generals, and admirals were pitted against one another in a system of overlapping, frequently shifting assignments that increased the flow of information to the president and preserved his decision-making options but created disorder and frustration arising from his "ad hoc, divide-and-conquer administrative style" (p. 197). Through their actions, presidents have seemed no less critical. In place of Roosevelt's kaleidoscope of structures and advisers has arisen a markedly larger advising system characterized by staff specialization, expanded policy expertise, and substantial hierarchy. This detailed study of Roosevelt's administrative strategy seeks new insights into how contemporary presidents might derive better service from their advisers by emulating FDR's practices. Dickinson argues that mobilizing the economy, conducting a world war, and preparing for the postwar world were enormously complex tasks that were no less challenging than the environment confronted by contemporary presidents.

Central to Dickinson's argument is the contention that contemporary presidential advisory systems fail to enhance presidential power. The large, functionally specialized, White House-dominated advising system of the post-Roosevelt era—the "presidential branch"—exhibits inherent flaws that are illustrated through a case study of the Iran-Contra affair. In contrast to Roosevelt's small group of White House generalists, whom he personally supervised in their performance of tasks designed to meet his needs for information and expertise (and thereby reduce his bargaining uncertainty), modern presidents incur "insuperable management costs" (p. 9) in overseeing an army of aides or, as in Iran-Contra, suffer from failing to do so.

Dickinson demonstrates that Roosevelt's administrative strategy, far from being a direct reflection of his personality or the transfer of techniques developed during his prepresidential career, evolved during his presidency, a reflection of trial and error (e.g., his early experiments with cabinet coordinating committees), conscious design (his consultation with the Brownlow Committee), and evaluation of his recurrent needs (the enhanced responsibilities of the Bureau of the Budget). While most accounts of FDR's management approach emphasize the New Deal period, Dickinson pushes beyond to a close examination of Roosevelt's use of staff following passage of the 1939 Reorganization Act and the implementation of a new staff system, which consisted of both personal and institutional components. In confronting his wartime tasks, FDR resisted repeated appeals by aides, generals, the Congress, and observers to streamline his administrative practices by appointing a mobilization czar, to creating a White House-centered economic and policy staff, naming a war cabinet, and unifying the military services under a single chairman. His rejection of these suggestions, argues Dickinson, enabled FDR to protect necessary bargaining resources and to delegate carefully delimited authority to a quintet of particularly important aides without jeopardizing his constitutional power. This quintet included Justice James Byrnes, who left the Supreme Court to head the Office of War Mobilization and effectively became the "assistant president on the home front" (p. 149); Harry Hopkins, whose influence on foreign policy rivaled that of the secretary of state when his oversight of munitions and Lend Lease extended into international diplomacy; Admiral William Leahy, who chaired meetings of the Joint Chiefs of Staff as the president's liaison with the military; Judge Samuel Rosenman, who joined the White House staff to oversee political and legislative issues; and Harold Smith, Bureau of the Budget director, who became an indispensable institutional resource. Even in coming to rely more heavily on this critical group of aides, Roosevelt resisted official titles, based their authority on executive order rather than statute, and ensured that they preserved his own bargaining resources.

To most students of presidential staffing, Dickinson's study will be provocative but unconvincing. Like Roosevelt himself, Dickinson values bargaining advantage over efficiency, a perspective explicitly derived from Richard Neustadt's classic *Presidential Power*. Despite acknowledging the flaws in Roosevelt's strategy (pp. 77, 220), Dickinson regards his performance as effective, an evaluation that is far from universally endorsed. It is possible, in fact, that FDR succeeded in achieving the nation's wartime goals *despite* his administrative style rather than because of it, and that those same goals could have been accomplished at a lower cost and more effectively had the president been less concerned with preserving his options. Dickinson offers ample evidence that many of Roosevelt's most loyal contemporaries had deep reservations about how he chose to perform the nation's most critical tasks.

Also, it is not clear how readily any of FDR's successors could have replicated his administrative strategy. The effective use of competitive adhocracy may require a virtuoso performance by a president with exceptional political skills. Operationally, such a system runs the risk of presidential overload—burdening a president with an excessive volume of immediate decisions, a factor that many have suggested

affected Roosevelt as well. Moreover, following Roosevelt's example would be likely to create an even more personalized presidency than now exists, at a time when many believe the presidency should be built down not up. In fact, excessive personalization is precisely the complaint that led to the construction of post-war advising structures in economic and foreign policy, areas in which FDR's previous dominance was regarded as dangerous.

Revisionist interpretations of Eisenhower's management practices have now become a staple in the literature. Dickinson's study may well inaugurate another round of reexaminations focused on FDR. Although Dickinson urges future research to cull for additional cases of presidents who may have used FDR-like techniques (chap. 9), it is much more likely that this study will trigger future research into the Roosevelt record with a new appreciation for the management lessons likely to be revealed.

The Color of Politics: Race and the Mainsprings of American Politics. By Michael Goldfield. New York: New Press, 1997. 404p. $19.95.

Mamie E. Locke, *Hampton University*

Studs Terkel has argued that race is the "American obsession" that permeates every facet of our social existence. There are not many issues that one can discuss which do not have race at the root of the discourse, whether the debate is about the criminal justice system or welfare reform. Michael Goldfield uses this conception of race as the springboard for an analysis of the role of race in guiding and shaping the American social and political system.

Goldfield begins the analysis with a general discussion of race in the United States, arguing that it has been at the center of major turning points in the nation's history. This focus on race is never more pervasive than during periods of economic downturn. The frustration and resentment are not targeted at those who manage and benefit from the economy, however, but at explosive and intangible issues like affirmative action. Like many hot button issues, affirmative action is one of those concerns for which a real, viable solution (e.g., finding jobs) is never addressed. Instead, minority groups are scapegoated as being the cause of the problem. Goldfield further posits that race rhetoric in political campaigns in the recent era is reminiscent of the Jim Crow period. This racial animosity is often accompanied by moral righteousness. The author finds the parallels and the continuous focus on race as paradoxical and asks why such tactics work. To answer this and many other questions, he begins with an historical analysis of race in an effort to bring clarity and understanding to the current role it plays in the American polity.

The introductory chapter discusses very briefly a variety of theories on race, ranging from biological assumptions to ignorance. Goldfield argues that these theories cannot be fully understood without a clear analysis of the class and economic roots of racial formation and systems of racial oppression. He selects five critical periods (turning points) in American political development to explain the genesis of the social and political system of racial subordination that undergirds racial thought in the contemporary period. The periods highlighted in following chapters are the colonial era, Revolutionary War/constitutional era, Civil War/Reconstruction era, Populism period, and the Depression/New Deal era. In each period, Goldfield discusses the relationship between race and class in shaping the particular movement and the subsequent responses to the disruption and transformation of American social, economic, and political life.

The period that follow these turning points (post–World War II, civil rights movement, and white racist coalition building) builds on the lessons from the time preceding it. In each of these defining moments of American history, there has been a failure to combat racism in any meaningful way through the development of substantive policies designed to promote racial equality. According to Goldfield, race has had a significant effect on governance and how various races and ethnic groups, particularly blacks and whites, interact with one another. Since colonial times race has been central to the outcome of any struggle. Resolutions to these struggles have altered social and political relations, leading to new arrangements of social control and a reorganized system of racial domination and subordination.

Goldfield's underlying hypothesis is that white supremacy is the reason for the inability of workers in the United States to develop and sustain class organization and consciousness. He argues that the system of white supremacy has also played a central role in all the critical turning points of U.S. politics. This is a main thesis and continuing thread throughout the fabric of the book. Goldfield claims that the development of the system of white supremacy and the ideology used to justify it originated from and is based in the socioeconomic system. The principal function of both the system and the ideology has been to control and exploit the labor of African Americans and other nonwhites as well as to control white workers in order to isolate them from potential allies. Hence, white supremacy and chauvinism have prevented the development of a sustained, solidaristic, class-based labor movement.

Goldfield continues to support this theme by arguing that white supremacist ideology and beliefs have been institutionalized through many economic, political, and ideological supports. It has been "the failure to recognize the importance of white supremacy and the unwillingness of white workers, white labor leaders, and white radicals to confront it head on [that has] been a primary factor in keeping workers from organizing on a class basis," (p. 31). Even when class consciousness and organization have blossomed during any given movement, the commitment to the fight against racial discrimination has been at the top of the political agenda, not class struggle.

If one is to understand the major structure of the race question in the United States, then there must be an understanding of the demand for exploitable labor. The thread connecting this demand is the relationship between race and class. The early characteristics of race relations and servitude in the colonial period set the tone for much of the early labor struggles. White supremacy and racial slavery were developed because they advanced the most powerful economic interests in the colonies. Racist theories of black inferiority and racist attitudes were used to justify a system of racial domination and subordination that gave power and profit to white southern plantation owners. Race was also used as a barrier to prevent oppressed groups from uniting with natural allies. For example, plantation owners, as a mechanism to control the white working class, used poor whites as overseers and slave patrollers to subordinate blacks. Aspects of these racist theories and attitudes remain in contemporary society.

While such overt violence as lynching and brutal murders of blacks and white supporters of biracial causes has disappeared, the pattern of violence continues in other forms: church burnings, vigilantism, and right-wing terror. In the current era, the colonial concept of "free, white, and twenty-one" and the privileges attached to it is alive and well. Those embracing the concept believe that they have a moral right to racial preferences over nonwhites in every venue. As Gold-

field points out, "the racist ideology that defined Black people as biologically inferior and socially undesirable was developed to justify Black slavery. This racist ideology, in new and contradictory forms, evolved, developed, amplified, and amended, lives on in America today" (p. 48).

Goldfield has written an easy to read, informative book assessing the role of race in the political and social development of the United States. He does so through an historical examination of critical periods of the nation's history as well as an assessment of the labor movement and the lack of the formation of a lasting multiracial labor coalition. He draws parallels from the historical period to the current one. He admits to being circuitous in his approach, and this is the major shortcoming of the book. He restates many historical events in each era that are rather cliché, and in some instances he states the obvious. He uses a variety of secondary sources whose arguments are quite familiar to readers. While many of these authors (Beard, Foner, DuBois) are certainly notable, there is nothing new in their arguments, nor does Goldfield add anything new. He also makes the argument that race has been a central feature of U.S. politics to the point of overkill. The repetition of this theme disguises the underlying theme of race overshadowing class issues. His book is equally a discussion of labor relations and the barriers that have thwarted working-class solidarity.

Despite these limitations, the book has value in its linking of the historical development of the subordination of African Americans to the current white racist coalition building, with its blame-the-victim approach to problem solving. His message that diversionary tactics have been used to avoid addressing the real causes of injustice and inequality in the United States is quite noteworthy. The fact that the system has failed to develop substantive policies to eradicate the scourge of inequality underscores the institutionalization of racism. In Goldfield's view, "white people's attitudes will only be changed substantially on racial matters when a determined group that is uncompromising and relentless on the issues, proselytizes, organizes, and refuses to go away" (p. 462). This group should be composed of both genders and various races and ethnic groups. Then, and only then, will a just and equitable society be created.

African American Women and the Vote, 1837–1965. Edited by Ann D. Gordon, with Bettye Collier-Thomas, John H. Bracey, Arlene Voski Avakian, and Joyce Avrech Berkman. Amherst: University of Massachusetts Press, 1997. 217p. $45.00 cloth, $15.95 paper.

How Long? How Long? African-American Women in the Struggle for Civil Rights. By Belinda Robnett. New York: Oxford University Press, 1997. 256p. $35.00 cloth, $18.97 paper.

Carol Nackenoff, *Swarthmore College*

This monograph and a collection of essays effectively argue that to treat gender in the history of African American struggles for political equality one cannot just "add women and stir." Rather, to incorporate women, new methods and new theorizing are required. As the essays make clear, existing paradigms fail to appreciate the heart and scope of the experiences of African American women and the intersectionality and simultaneity of struggles around race, sex, and class. These new works follow in the tradition of Paula Giddings (*When and Where I Enter*, 1984), offering greater historical depth and guidance on recovery of primary source material. Both books contribute to their stated goals of discovering and learning more about black heroines in the struggle for political equality and citizenship. Robnett and several authors in *African American Women and the Vote* go farther and contribute importantly to the project of retheorizing.

How Long? How Long? is a very impressive and theoretically rich piece of scholarship by sociologist and women's studies scholar Belinda Robnett. A chapter rethinking social movement theory and one on theoretical conclusions frame the book, with the rise of the civil rights movement in the South and its ultimate unraveling from below by 1966 marking the progression of Robnett's story. Most chapters add fresh insights to understanding the formal organizations, formal and informal leadership, and grassroots mobilization of the civil rights era. Robnett finds complex interactions and offers an exceptionally vivid and compelling specification of the way regional culture, race, gender, class, and education shaped leadership possibilities, roles, and experiences.

Robnett has drawn on a rich treasure of interviews with movement participants, including open-ended telephone interviews with 25 women she conducted from 1990 to 1992. The narratives of movement participants are beautifully and seamlessly woven into the analysis. The interview data allow her to make good on her goal of avoiding essentializing categories, instead relying on "narratives to express the identities of the actors in question and define their own social locations" (p. 5). She generally moves with ease among engagement with the scholarly literature, frames of analysis, and the interview materials. There are, however, points at which the reader may dread seeing the term "bridge leader" (with its four variants) or the terms primary and secondary formal leader one more time.

How Long? How Long? remodels and redefines leadership, combatting scholarship that has failed to see women's participation in the civil rights movement as leadership. Robnett demonstrates the importance of methods that do not define leadership on the basis of formal titles, prominence in organizational records and newsletters, or even frequent speaking at meetings. Titles, she argues, generally fail to reflect women's authority in this movement. Women with formal titles were likely to be relegated to secretarial functions in the office. Women without titles, who worked in the field, often had considerable autonomy to lead, considered themselves leaders, and were recognized as such (sometimes as charismatic leaders) by their co-workers and by those they mobilized. In contrast to the assumption that leaders mobilized followers, Robnett argues that women leaders in the community frequently recruited and mobilized male formal leaders.

Though cultural and societal norms established some limitations on women's formal leadership, Robnett finds the lens of gender discrimination inadequate for analysis of women's roles. "The exclusion of most women from formal leadership positions created an exceptionally qualified leadership tier in the area of micromobilization" (p. 191). They bridged formal organizations and the grassroots. Bridge leaders, many of whom were women, were critical to building and sustaining grassroots mobilization. Their mobilization skills built the base on which the civil rights movement rested. They helped forge political consciousness and group solidarity in the African American community. Their own courage and willingness to put their lives in danger in moments of crisis and leadership breakdown mobilized via emotion—a vital component of movement recruitment and mobilization, Robnett argues. At the time, these women did not (by and large) experience the limitations on their formal leadership opportunities as gender discrimination. Especially in those organizations that were least hierarchical, women were able

to lead in ways they had never done before, and they felt their empowerment. In the early years of SNCC, people "leaped out of" prescribed roles (p. 194). "Ironically, it is this 'now' perceived limitation that served to catapult and sustain the identity, collective consciousness, and solidarity of the movement" (p. 191). Since bridge leaders provided the glue that held the movement together, when it unraveled, it was from the grassroots up. New hierarchical arrangements, emphasis on male leadership, and black nationalism in SNCC pushed bridge leaders and the agendas of their Southern grassroots followers (voting rights, jobs, health care) off the screen.

When dealing with charges and perceptions of sexism in the movement that date from that time, Robnett is inclined to dismiss them, which is at first glance unsettling but upon greater reflection quite nuanced. One example will suffice. She considers at length one 1964 paper produced by two white women charging SNCC with gender discrimination, and her argument stresses positionality. She points out that white women were more likely to work in the office than their African American counterparts (among other reasons, their presence in the field could endanger the lives of black activists), and they therefore experienced more traditionally gendered divisions of labor than field workers. The complex nature of the relationships among gender, class, and race again appears.

Robnett examines women's activism in the NAACP, CORE, SCLC, SNCC, Mississippi Freedom Democratic Party, the Women's Political Council of Montgomery, and the Montgomery Improvement Association, noting that leadership opportunities for women varied with the level of centralization in the organization. The portrait of Martin Luther King is not very flattering. King was reluctant to capitalize on grassroots victories and energy in this period, totally unwilling to imagine women as leaders, and deaf to their counsel and ideas. If Robnett is enthusiastic about the early years of SNCC, she nonetheless acknowledges that SNCC could not have taken the road to empowerment it did unless there was at least one other movement organization (e.g., SCLC) that engaged the state and made compromises (p. 195). In these organizations, male leaders frequently had to deal with the white power structure. "The women were not faced with the pressures of national credibility and the need to compromise" and therefore had the luxury of remaining more true to their constituents (p. 165). The author is partial to those women leaders least willing to yield or compromise their own visions, including Ella Baker, Fannie Lou Hamer, and Gloria Richardson (featured on the dust jacket), whose work was undermined by male leaders.

African American Women and the Vote is a collection of essays written chiefly by historians and African American studies scholars, growing out of a 1987 conference held at the University of Massachusetts Amherst. In revising the papers, authors incorporated a few later contributions to scholarship. Several essays that frame their arguments for the simultaneity of race and gender struggles, as opposed to giving precedence to one or the other, retain a mid-1980s flavor.

The volume is organized chronologically, charting key turning points in the political activity of black women. It begins with the first antislavery convention of American women and ends with the civil rights struggle in the 1950s and 1960s. An express goal is to lay the groundwork for a comprehensive political history of African American women. It is a project of historical recovery and, as Bettina Aptheker suggests, empowerment and self-esteem.

Despite the title's emphasis on the vote, another important goal of the book is to challenge and expand traditional definitions of politics. To look only at suffrage, from which many African American women were excluded during many of the years under scrutiny, is to miss or miscalculate the actual power wielded by African American women. As Terborg-Penn points out in an overview essay, suffrage was "merely one seam in the fabric of women's political struggle" (p. 20). Several essays illustrate "the independent initiatives by black women toward establishing a power base in the community from which to assert greater influence and control over issues crucial to their daily lives" (Aptheker, p. 206). Essays, case studies (including black female legal challenges to emerging Jim Crow statutes), and examinations of the activities of particular black feminist leaders (Frances Ellen Watkins Harper, Charlotta A. Bass) display the rich variety of political concerns of female African American activists.

Several essays offer far richer insights and fertile theoretical reformulations than the rest. Cynthia Neverdon-Morton's essay on southern African American women's organizations from 1895 to 1925 is noteworthy for highlighting the important community-building role played by rural women involved in social service programs, for emphasizing the frequent flow of information and influence from local organizations and activists upward, and for examining possibilities and limits in interracial cooperation within women's organizations. The special standouts in this volume are the essays by Evelyn Brooks Higginbotham on clubwomen and electoral politics in the 1920s and, most notably, Elsa Barkley Brown on southern black women's political history from 1865 to 1880.

Too frequently, the 1920s are cast as a decade in which women's mobilization collapsed after their Nineteenth Amendment victory. Higginbotham's northern urban African American clubwomen had a strong sense of political efficacy and saw themselves as in politics to stay. In the 1920s, organized African American women played a significant role in mobilizing voters; they engaged in door-to-door canvassing and talked and held meetings on political issues of concern to the community. In African American communities increasingly differentiated along class lines, clubwomen prepared women for citizenship. Like Neverdon-Morton, Higginbotham traces the fate of fledgling alliances with predominantly white organizations, here the League of Women Voters.

Elsa Barkley Brown's dazzling essay on the immediate post–Civil War years is a remarkable example of what placing African American women at the center of the analysis can do to retheorize citizenship struggles. For these newly freed women, husband, children, and community at large were bound up with their vision of autonomy; "individual freedom could be achieved only through collective autonomy" (p. 69). African American women did not understand freedom, citizenship, representative institutions, or property in the same individualistic and market-oriented terms that their northern liberators employed. Their worldview involved an ethos of mutuality; members of the community were responsible for one another. An example of this differing vision of citizenship finds African American men and women thronging to early state conventions and legislative sessions, expecting not just to watch but to participate alongside delegates. Women refused to remain passive spectators. African American women saw themselves having a vital stake in the male franchise, considered the vote a collective good, and imposed sanctions against African American males who dared to vote Democratic. "That African American women did not operate inside the formal political process does not negate the intensely political character of their actions" (p. 86). Barkley Brown persuasively reminds us that current scholars have lost

the language with which to express the concepts that these African American women understood.

To the extent these two new books expand our notion of the scope of African American women's political activity, reconceptualize and acknowledge their leadership, and retheorize African American women's place in struggles for citizenship and civil rights, they merit praise and careful attention. Robnett and Barkley Brown deserve an ovation.

The Bully Pulpit: The Politics of Protestant Clergy. By James L. Guth, John C. Green, Corwin E. Smidt, Lyman A. Kellstedt, and Margaret M. Poloma. Lawrence: University Press of Kansas, 1997. 221p. $35.00 cloth, $19.95 paper.

Laura R. Olson, *Clemson University*

Like other types of social leaders, clergy serve a role that is not explicitly political. Nonetheless, many do become involved in politics. Clergy are simultaneously citizens, religious professionals, and institutional leaders. As a result, their political attitudes and behavior are shaped by a myriad of forces. *The Bully Pulpit* represents a major step forward in our understanding of why and how clergy become politically involved. It is also a significant empirical contribution to the literature on religion and political behavior.

Not since the 1970s has such serious attention been paid to the political orientations of clergy. Fortunately, the authors have taken this important task very seriously. The wealth of data they have collected on the political attitudes and behavior of Protestant clergy is remarkable; there is also much to be learned from their interpretation and analysis. Over the years these authors have worked diligently to create reliable and valid religious measures, and this effort has served them—and a host of other scholars—well.

The book draws on two decades of research on thousands of Protestant clergy serving eight traditionally white evangelical and mainline denominations (the Assemblies of God, Christian Reformed Church, Disciples of Christ, Evangelical Covenant Church, Presbyterian Church U.S.A., Reformed Church in America, Southern Baptist Convention, and United Methodist Church). The denominations the authors have chosen capture the variety of white Protestantism, but the reader may be left wishing for similar information on African American Protestant clergy, who have traditionally been more politically active than any other group of ministers in the United States. The authors begin by providing rich contextual detail about a handful of specific clergy in a series of anecdotes reminiscent of James Davison Hunter's introduction to his noted book (*Culture Wars*, 1991). These stories, while enormously interesting, are unlike the rest of the book; the chapters that follow are occupied by the presentation and technical discussion of the quantitative data.

The authors base their analysis on the notions of *theology* and *social theology*, which constitute the primary predictors of ministers' political goals and activities. Ministers' basic theological orientations, they argue, are the central components of their overall belief systems and as such may be expected to have a profound effect on their political attitudes. Theological traditionalists (evangelical Protestants) are expected to focus their energies on moral reform, while theological modernists (mainline Protestants) are supposed to espouse a social justice agenda. Social theology refers to "beliefs connecting theology to public affairs" (p. 8). The authors argue that there are two predominating social theologies at work in the United States today: the individualism of conservative evangelical Protestants and the communitarianism of liberal mainline Protestants.

White Protestant clergy therefore come in two distinct breeds, as the authors see it: traditional individualists (conservative evangelical ministers) and modern communitarians (liberal mainline ministers). This "two-party system" dichotomy characterizes the political agendas and activities of white Protestant clergy, as table after table in this book demonstrate. Traditionalist clergy embrace "moral" issues and vote Republican; they prefer to involve themselves politically by stating their views in religious settings. Modernist clergy are more likely to be concerned about social justice and to vote Democrat; they prefer to participate in a wide range of political activities both inside the church and in the surrounding community.

There is, however, diversity within these two "parties"; not all traditionalists are Christian Right activists, and all modernists do not embrace 1960s-style protest politics. In fact, only 9.7% of all clergy these authors surveyed fall into each of these categories. Clergy engage in a wide range of political activities, but some do not participate at all. Moreover, while some clergy prefer to engage in direct political action, others favor a strategy the authors label "cue giving," which involves taking explicit stands on issues or candidates. In short, while ideological orientations appear to fit tightly into a two-party system, political participation may be somewhat less rigidly dichotomous.

This study thus begins to address the important theoretical need to understand the circumstances under which social elites (in this instance clergy) make the choice to translate their leadership into the political realm while others do not. Understanding the circumstances under which clergy become interested and involved in politics paves the way for future research on how clergy create political contexts within their churches. Yet, the authors focus on national patterns, which means that they sacrifice some level of detail in their exploration of intradenominational pluralism. While they argue early on that social context is an intervening variable between theology/social theology and clergy goals and activities, they do not operationalize the notion of context formally. They consider a few contextual measures (such as church size) in their analyses, but their real focus is on the predictive power of the theological constructs. This emphasis on theology and ideology provides an important—but in my view incomplete—account of how and why clergy become politically involved.

Two fascinating challenges are posed by this important and useful book; these flow from the authors' *a priori* assumptions and constrain the study's broad applicability somewhat. First, the utility of the "two-party system" dichotomy would be diminished if the scope of the analysis were moved beyond white Protestants. In particular, neither African American Protestants nor Catholics fit neatly into either of these categories because (in different ways) both groups embrace a complex combination of traditionalism and modernism.

Second, I question whether theology (variously defined and named) is the crucial independent variable for understanding the political orientations of clergy. The exclusion of other important variables tapping such things as local political context may clear the path for the theology variables to succeed no matter what. The way these theological variables are used also suggests that denominational affiliation may be a sufficient proxy predictor of the political orientations of clergy. As I see things, the path between these two variables is far more complex and circuitous than the authors seem to imply. Moreover, there is tremendous diversity within the traditionalist and modernist camps of white Protestantism, which the authors note, but this diversity does not come through as clearly as it could in the book. A simple but telling

observation appears on p. 154, when the authors state directly that "many of these variables are interrelated: theology, social theology, political agendas, and political beliefs." This observation succinctly captures both the major contributions and limitations of this work.

The Bully Pulpit is an important empirical contribution in the best tradition of normal science. It is a valuable compendium of information on the political attitudes and behavior of white Protestant clergy; I, for one, am very glad it exists. Some readers may also find that the book stimulates more questions than it answers, so it is sure to inspire future studies of the political orientations of clergy. These authors have collected an almost overwhelming amount of empirical data. They also have many interesting things to say about the potential mobilizational roles of theology and ideology. The empirical theory they generate about clergy political participation is not yet complete, however. The thriving subfield on religion and politics, in which these authors are pioneers, will undoubtedly continue to work to address this concern.

Enforcing The Law: The Case of the Clean Water Acts. By Susan Hunter and Richard W. Waterman. Armonk, NY: M.E. Sharpe, 1996. 248p. $66.95 cloth, $28.95 paper.

James P. Lester, *Colorado State University*

The implementation of environmental policy in the United States over the past two decades is an important topic that has gone largely unexamined. Since we are clearly in an "implementation era" as far as environmental policy is concerned, much more research needs to be directed to the area of subnational implmentation of federal environmental policies and programs. This book by Susan Hunter and Richard Waterman addresses that research deficit by examining the enforcement of the Federal Water Pollution Control Act of 1972 (the Clean Water Act) and its various amendments during the 1970s and 1980s. In doing so, they posit two alternative models of regulatory enforcement, the "enforced-compliance" and the "negotiated" model. These are not pitted against each other; rather, the working hypothesis is that "the diverse nature of water pollution problems across the fifty states has created the essential circumstances under which 'pragmatic enforcement' is most likely to occur" (p. 9). The latter approach really means a consensual, more negotiated, approach as opposed to the strict approach.

In examining their hypothesis, the authors argue that the type of bureaucratic response adopted (strict or negotiated) is directly related to the diversity of the regulatory environment. This environment includes the various actors involved, the economic conditions relevant to the problem being regulated, the organizational structure employed by the regulating agencies, the manner in which these agencies interact with one another, the way in which relevant legislation is written, the demographic conditions of the target population for the regulatory action, and the nature of the regulatory problem. Their data are drawn from the EPA's National Pollutant Discharge Elimination System (NPDES) program for 1975–88 and 1989 through mid-1992, together with extensive interviews with EPA personnel, industry representatives, and environmentalists.

Essentially, they find that the diversity of the regulatory environment does indeed matter; specifically, demographic variables (especially state population) are related in such a way that the greater the number of people living in a state (and the greater the racial stratification as well), the greater is the level of NPDES enforcement actions. Moreover, perceptions of water quality, state organizational structures for protecting the environment, the state income level, and interest group strength (e.g., mining, industrial, and agricultural interests) matter as far as enforcement is concerned. It is surprising, to the authors at least, that traditional political variables, such as LCV scores, registration rates, turnout, and legislative professionalism do not matter with regard to enforcement actions.

Their findings suggest to me that the severity of the water pollution problem is related to enforcement by state implementors. In other words, large populations (especially with a significant amount of poverty) are likely to be associated with pollution severity because people pollute; similarly, the presence of polluting industries will also be associated with pollution severity; and states are responsive to pollution severity will organize to prevent and control pollution. This is what we would expect, and such conclusions leave me with optimism rather than pessimism. If these severity variables are important—and the authors say they are—then politics probably should not matter. Politics would matter if these states' responses were unrelated to severity. In other words, if states are not responsive to the severity of conditions, then politics takes on even more importance; if states are responsive to severity, then politics has not gotten in the way. While my interpretation is admittedly critical of politics, it is a realistic one, for politics itself means dissensus and conflict about how best to proceed on this issue. By responding to the severity of the problem, agency officials are thus acting in a positive manner (and therefore not gridlocked by political divisiveness).

When the authors turn their attention to water pollution outcomes (i.e., improvements in water quality), they find neither population nor racial characteristics to be very strongly related to outcomes, but still they suggest that the finding about racial characteristics and water quality raises rather serious questions about environmental justice. This is a curious interpretation. How can it be that state implementors are, on the one hand, responsive to pollution severity (including states with a large contingent of African Americans) but, on the other hand, not responsive when it comes to the results of their efforts. How can they find some potential evidence of environmental racism in one instance (water quality outcomes) but not in another (water policy outputs)? Could it be that actions taken to remedy a problem do not always produce the intended results in a timely manner, but still agency representatives are behaving in an affirmative and responsible way? The authors pose a paradox for us in that their reversal of findings offers for them two competing explanations, rather than a singular integration of two separate levels of analysis. Can one argue (to the contrary) that state bureaucrats are effective on one level but not on the other, even though their efforts on both levels are well intentioned?

The authors make several policy recommendations, including a shift from "command and control" strategies of enforcement to an "economic incentive" approach. The latter would rely more on the marketplace as opposed to government as the key actor in implementing environmental policy. Their suggestion, while certainly plausible, has been suggested before by neoconservative regulatory reformers, such as Murray Weidenbaum and George Stigler, among others. Yet, it is perhaps the one alternative from among many that has drawn the most criticism and skepticism from environmentalists. How can we reconcile the authors' finding that government actors seem to be doing a good job with their suggestion that we rely more on the marketplace as an approach to implementation?

A final concern is the authors' use of theory and their

epistemology. Why, for example, did Hunter and Waterman not draw more on the literature to guide their analysis of the crucial variables affecting state implementation? There are connections that could have been made between this project and the broader literature on policy implementation. I would like to see how these findings complement (or challenge) other work in this important phase of the policy cycle. Moreover, why did they adopt an essentially inductive approach? Why not combine it with a deductive one? At the end, it would have been useful if they had related their findings to the literature.

Despite some reservations, I consider this is very useful work for policy practioners and for graduate classes on environmental policy and intergovernmental relations. The book raises many important questions about how best to proceed with subnational implementation of environmental policies. Students will find much in this book that will stimulate classroom debate and further empirical analysis.

New York Politics and Government: Competition and Compassion. By Sarah F. Liebschutz, with Robert W. Bailey, Jeffrey M. Stonecash, Jane Shapiro Zacek, and Joseph F. Zimmerman. Lincoln: University of Nebraska Press, 1998. 234p. $45.00 cloth, $20.00 paper.

Robert F. Pecorella, *St. John's University*

The thesis of this book is that politics in New York exhibits a continuing tension between the pressure to keep the state economically competitive and the tendency of state government to be compassionate in dealing with needy citizens. This theme is employed consistently by the principal author, who contributes six of the twelve substantive chapters, but only sporadically by four other authors. The book, loosely divided into a section dealing with political context and one on government institutions, is well suited for use as supplemental reading in a state politics course.

The opening section, which deals with historical, political, and intergovernmental issues, is uneven. On the positive side, an introductory chapter establishes the regional ethnic diversity of the state by tracing the settlement patterns of New York's many waves of immigrants. A subsequent analysis of state-federal relations includes two noteworthy observations. First, since the 1975 New York City fiscal crisis, the state's Congressional delegation, once considered highly fragmented, has proven much more effective in lobbying for state needs in Washington. Second, federal spending in New York is concentrated largely on social programs like Medicaid and not on developmental areas, such as procurement contracts. In other words, federal spending assists policies of compassion but not competitiveness. The chapters on pluralism and the state constitution, in contrast, are frustratingly sketchy. Moreover, it is puzzling why the chapter on the state constitution focuses more on proposed constitutional reforms than on describing the effect of the existing document on state governance.

The section on political institutions includes descriptive essays and insightful political analyses. Addressing the issue of continued party dominance in the face of decreasing party attachments among voters, Jeff Stonecash analyzes the support bases of the two major parties. He points to Democratic control of urban areas, particularly New York City, and Republican dominance in the suburbs and rural areas of the state as the regional manifestation of the diverse interests represented by the two major parties. The regional nature of party power is a bridge to Stonecash's second chapter, which analyzes the legislature. After detailing the evolution of legislative professionalization in New York, he argues compellingly that the highly partisan, strong leadership system in the state legislature reflects and reinforces regional political conflict in the state. Stonecash concludes that the combination of institutional professionalism and a strong leadership system has made the state legislature a potent policymaking force in its interactions with the executive branch.

The executive branch is the focus of two chapters, one on the office of governor, the other on executive agencies. In the former, Sarah Liebschutz makes the point that New York's strong executive is a function of both the impressive range of formal powers held by the governor and the political skills of the individuals who have served in that office. By including an outdated quotation, however, about the governor's powers in relation to a "part-time legislature," she undermines Stonecash's contemporary assessment of legislative equality with the executive branch. She also misses an opportunity early in the chapter to point out that politics in Albany involves interaction between two of the most powerful and professional branches of state government in the country. The subsequent chapter on executive agencies is a largely descriptive overview of the state's bureaucracy.

The remaining institutional chapters focus on the judiciary, local governments, and New York City. In reviewing the judiciary, Liebschutz does a good job of describing a system which appears structured to resist description and of analyzing different methods of judicial selection. Joseph Zimmerman's review of local government authority, framed by Dillon's rule (the "ultra vires" conception), on the one hand, and home rule ("imperium in imperio"), on the other, makes it clear that efforts toward local autonomy have been tempered by court rulings supporting state supremacy. Robert Bailey's insightful contrast of "organizational capacity" and "political legitimacy" highlights the widening economic mismatch between job opportunities and the skills of local residents in New York City as well as the increasingly stark racial divisions characterizing mayoral politics in the city.

This book makes a notable contribution to the state politics literature despite a thematic flow. The decision to employ the term "compassion" to analyze New York's generous social programs was ill advised. Although compassion undoubtedly motivated many social reformers, the size of New York's welfare state is largely a function of New York City's political influence. In the early 1900s, the Tammany political machine used its control of state government to counter Populist challenges to its power base in the city by promoting workmen's compensation, child labor, and a series of progressive factory laws in Albany. During the 1930s, the new class of urban reformers expanded the reach of social programs in an effort to cope with the social instability brought on by the Depression.

In more recent years, the size and scope of the welfare state have grown as gubernatorial candidates of both major parties sought the support of New York City voters and public-sector employees. Successful Democratic candidates have needed a two-thirds vote in the city to overcome Republican margins upstate and in the suburbs. Conversely, successful Republican candidates for governor, with the notable exception of George Pataki in 1994, have needed to hold down the extent of their losses in New York City. Thomas Dewey and Nelson Rockefeller, for example, averaged more than 40% of the New York City vote in their combined seven victories. As a result, the city's social needs have been included on the executive agenda in Albany regardless of the party in power.

Executive focus has now been coupled with that of a powerful legislature, which, as Stonecash points out, reflects regional interests. Since 1975, the Democratic assembly has emphasized policies favorable to urban service recipients and

providers while the Republican senate has stressed the interests of suburbanites and rural residents. Policy compromises designed to satisfy these diverse constituencies have often meant larger government, less fiscally responsible government, or both. The size of government in New York, therefore, is explainable as a function of electoral strategies and political calculations built around New York City. Whatever its other merits, compassion is less than useful as an analytical tool for understanding New York politics.

Left Out: Pragmatism, Exceptionalism, and the Poverty of American Marxism, 1890–1922. By Brian Lloyd. Baltimore, MD: Johns Hopkins University Press, 1997. 461p. $45.00.

Clyde W. Barrow, *University of Massachusetts at Dartmouth*

Brian Lloyd's *Left Out* presents a direct challenge to the generally accepted histories of U.S. socialism, which attribute the failure of the American Socialist Party to its doctrinaire Marxism (Daniel Bell, *Marxian Socialism in the United States*, 1967) and to the divisive factional politics that resulted from esoteric debates over Marxian theory (David A. Shannon, *Socialist Party of America*, 1967). In contrast to this widely accepted image, Lloyd argues that the history of American Marxism has always been intertwined with American pragmatism, and "indeed, the notion that pragmatism and Marxism share a significant expanse of philosophical and political ground supplies a trademark motif to the history of American radical tradition" (p. 5).

This should be an eye-opening work for contemporary socialists who view a Marxist/pragmatist synthesis as the philosophical basis for renewing democratic socialism, since most adherents of a pragmatic socialism are completely unaware of their historical continuity with this tradition. It is important, for Lloyd, that previous attempts to fuse Marxism and pragmatism resulted in "avowed Marxists . . . fighting with non-Marxist—and explicitly anti-Marxist—thinkers for possession of the same scientific preconceptions" (p. 6). In other words, because philosophical pragmatism proved far more compatible with new liberal politics than with socialist politics, pragmatism became the wedge that enabled liberals to "take Marx away from the Marxists." The historical lesson for Lloyd is that socialists need to discern and uphold "Marxist theoretical orthodoxy" (p. 414), and "this project requires, as a first step, moving the discussion off the ground upon which Madison, Beard, and Marx look like philosophical kin" (p. 11).

Since Lloyd considers the poverty of American Marxism an "ideological condition," he reconstructs the development of American socialist theory from 1890 to 1922 through an approach "that is both rigorously textual and steadily contextual," although the bulk of Lloyd's "analytic work consists in close textual readings" of socialist theorists (p. 14). The analytic goal is to document a successively widening gap during this critical period between American socialist theory and the so-called "Marxism of Marx" (p. 7). Lloyd's reading of American socialist theory resounds with echoes of Louis Althusser and Etienne Balibar (*Reading Capital*, 1970) and Goran Therborn (*Science, Class, and Society*, 1976). In fact, Lloyd's analysis applies the structuralist critique of "historicism" and "empiricism" to the development of American socialist theory. Hence, in reconstructing the thinking of key socialist theorists, Lloyd juxtaposes each of them to the Marxism of Marx, which he equates with the categories that "define Marxism as a distinctive brand of socialism": mode of production, productive forces, social formation, and social relations of production (pp. 157, 99). Thus, despite the *political* factionalism of the American Socialist Party, Lloyd argues that these disputes were minor skirmishes within "a single problematic," that is, the evolutionary/pragmatic socialism of the Second International (p. 57).

Lloyd traces the development of American socialist thought through three periods of development. From 1901 to 1905, leading Socialist Party theorists embraced a Spencerian Darwinism that located (and subordinated) Marx within a more encompassing positivist and evolutionary historical framework. Thus, American socialism began with Marx in a subordinate position, and this "error" established the basis for a second period (1905–12), when American socialists began to substitute Thorstein Veblen for Marx (whose writings, we should recall, were not available in English translation until 1905). Given "Veblen's ideological kinship with pragmatism" (p. 58), the substitution of institutional economics for Marxian economics provided the necessary bridge to pragmatism after 1912. In this critical transition, socialists shifted away from Marx altogether to William James and John Dewey. This shift resulted in a Marxism "shorn of the abstract, non-empirical concepts of *Capital*" (p. 83) and thus deprived American socialists of "the distinctive and abstract character of Marxist political economy" (p. 123). Lloyd's final assessment is that this "ideological poorhouse" left American socialists unprepared to assimilate anything but a "practical Lenin" after the Russian Revolution (p. 408), rather than the "nonpositivist, non-Darwinian Marx that Lenin read" (p. 410); that is, the Lenin who emphasized that "without revolutionary theory there can be no revolutionary practice."

Historians and political theorists, including myself, will pose numerous challenges to Lloyd's analysis and conclusions, but as one might expect of an Althusserian, this book is the most intense and systematic analysis of American socialist theory produced to date. Lloyd not only provides a very detailed and meticulous reading of key socialist theorists, such as James T. Kloppenberg (*Uncertain Victory*, 1986), but also situates his analysis comparatively by drawing explicit links with similar debates raging throughout the Second International. Lloyd also makes a political point that is worth considering independent of his methodological framework: The gap between Marxism and socialism, on the one side, and pragmatic liberalism, on the other side, has been submerged and systematically obfuscated throughout most of this century to create an artificial "progressive synthesis" and "liberal consensus." Indeed, whether or not one accepts the details of Lloyd's analysis, it is worth exploring whether the great intellectual mistake of American socialists is their continuing tendency to be fellow travelers of middle-class liberal politics.

Welfare as We Knew It: A Political History of the American Welfare State. By Charles Noble. New York: Oxford University Press, 1997. 210p. $45.00.

Gretchen Ritter, *University of Texas at Austin*

Why is the American welfare state so different? It is a question which has engaged students of American politics for much of the century, yet on which scholars have yet to agree. It is also a question particularly relevant in the wake of the 1996 welfare reform law, which fundamentally changed a half-century-old commitment to public entitlement for poor women and their children. To understand the current retrenchment, Charles Noble provides a broad historical and institutional review of American social policy in the twentieth century. He argues that the weaknesses and peculiarities of

the American welfare state are rooted in fundamental features of the American political system.

The claim that the American welfare state is exceptional is based on a comparison particularly to the social democratic states of Western Europe. In contrast to these nations, the United States has done little to offer universal entitlements for income, housing, or health care to its citizens. Except for the social provisioning offered to the elderly, most American social programs are underfunded and narrowly targeted to politically weak groups. The vulnerability of such programs was clearly demonstrated in the latest round of welfare cuts, which may be responsible for the current upsurge in hunger and homelessness in many American cities.

Scholarship on American welfare state exceptionalism may be broadly divided into three approaches. Many comparativists emphasize the relative balance of power between labor and business as crucial to the establishment of social provisioning. According to this view, in the United States, where labor is politically weak and business is politically strong, effective political support for welfare never developed. In contrast, other scholars have focused on the particularities of the nation's political institutions, both within and outside the government (especially the nature of the party system). Catch-all parties, federalism, and judicial review are some of the systemic features which have purportedly thwarted efforts at increased government activism. Finally, scholars of political culture point to the role of American liberalism in shaping popular attitudes toward public entitlement. Accordingly, the popular preference for small government and disfavor toward the dependent poor have limited the appeal of social provisioning for all but the most deserving, such as veterans and the elderly.

In *Welfare as We Knew It*, Noble combines aspects of the first and second approaches while rejecting the relevance of political culture as an explanation. For instance, he uses opinion poll results to show substantial public support for the view that "the state should guarantee every citizen food and basic shelter" (p. 12). Instead of political culture, then, Noble argues for an analysis that shows "how societal interests and political institutions *combine* to shape the environment in which reformers act" (p. 16). Noble emphasizes not only the role of weak labor and strong business interests, but also the ongoing relevance of racial division to undermining support for social provisioning. In the area of political institutions, he argues that decentralization has been the most important limiting factor in social policy development. These fundamental features of the American political context have made the task of building a strong welfare state a daunting one even for the most ambitious and effective political reformers. Even so, Noble reviews historical moments of welfare state development to argue that there were missed opportunities that might have been taken.

Having established the problem and his theoretical framework, Noble devotes the central chapters of his book to an analysis of historical instances of welfare state development. Three of these chapters examine periods of welfare state expansion in the 1910s, 1930s, and 1960s. In his chapter on the New Deal, Noble is critical of FDR for failing to go farther in establishing a universal commitment to full employment. Politically, Roosevelt is faulted for trying to gain the support of hostile business groups and for being too reluctant to challenge the conservative southern wing of his party. The latter portion of the book focuses on the retrenchment of the welfare state since the 1970s. According to Noble, the genius of the Reagan administration's efforts to limit social provisioning were centered not in conservative ideology but in the fiscal restraints provided by a ballooning budget deficit and new pay-as-you-go spending rules. During the 1990s, Newt Gingrich and his allies are credited with providing the ideological vision (with its emphasis on voluntarism, cultures of poverty, and individual behavior) that undercut public support for welfare. The Clinton administration's capitulation to this vision in 1996 is portrayed as political realism under extremely unfavorable circumstances. In his final discussion of future welfare state possibilities, Noble stresses the need for fundamental institutional reform in the electoral system and labor law.

The strength of this book lies in its often astute political analysis of the circumstances under which social policy proposals succeeded and failed. Furthermore, Noble's particular attention to the role played by racial politics and competitive federalism in limiting welfare expansion seems especially insightful. More generally, this book provides a useful synthesis of the work of many other scholars who have studied the American welfare state in historical perspective.

Yet, some aspects of the book are less convincing. In some ways its understanding of which societal interests are relevant seems quite traditional. There is relatively little analysis of the role of gender in welfare state development, which is particularly striking given the wealth of recent scholarship on this subject. As Theda Skocpol and others have shown, the origins of American social provisioning lie largely in the efforts of middle-class women reformers in the early part of the century to establish programs to benefit women and children. While Noble acknowledges the role of women's groups in his chapter on the Progressive Era, he devotes most of the chapter to discussing why labor was absent and business was hostile to national social policy. Furthermore, he is fairly explicit in arguing that a maternalist welfare state (centered around women as mothers) is inherently weaker than a paternalist welfare state (centered around men as workers) would have been. In other scholarship on the maternalist origins of American welfare, there is a great deal of contention over whether the original maternalist programs were positive or negative (most acknowledge that in the long run AFDC was negative), but few would go so far as to argue that paternalist policies are inherently superior.

When Noble brings his discussion up to the present he also fails to discuss the role of gender in the politics of welfare reform. This is striking, since the main program under assault by conservatives in 1996 was AFDC. Much of the congressional and popular debate on welfare reform revolved around the supposed sexual and maternal behavior of the women who receive public support. It seems that Noble misses much of this because of his assumption that class interests are the ones that matter most (except perhaps for race) and his tendency to downplay the role of political culture in welfare development.

Noble denies the relevance of political culture and ideology for two reasons. Empirically, he argues that polls show greater support for welfare than would otherwise be expected. Theoretically, he contends that culture and ideology tend to be endogenous variables without independent causal influence. His first point is important; certainly, political outcomes should not be taken as simple reflections of public preferences. Yet, it is not clear that we should be satisfied with an analysis of political culture which relies on the selective use of polling data. Theoretically, most interpretivists would agree that culture cannot be treated as an isolated, exogenous variable. To think otherwise is to misunderstand the nature of culture, which is embedded in lived experience. But to deny culture's relevance to political understandings and outcomes is another matter. The problem is one of scholarly analysis rather than one of causal influences.

Welfare As We Knew It makes a useful contribution to the ongoing debate over American welfare state exceptionalism. Noble's grasp of the scholarship in this area is substantial, and his use of a historical institutional analysis is quite insightful. Finally, his bottom-line argument, that the constraints on welfare state development derive from fundamental features of the American political system, is convincing.

Women, Media, and Politics. Edited by Pippa Norris. New York: Oxford University Press, 1997. 269p. $19.95 paper.

Laurie A. Rhodebeck, *Oberlin College*

Pippa Norris describes the objective of her edited volume as an attempt to "see whether the media frame gender politics 'with a different eye,' which hinders women's participation in public life" (p. 11). Media framing provides the organizing concept for this collection of thirteen original essays. Norris uses the concept in the tradition established by Erving Goffman (*Frame Analysis: An Essay on the Organization of Experience*, 1974), popularized by Todd Gitlin (*The Whole World Is Watching: Mass Media in the Making and Unmaking of the New Left*, 1980), and now widely applied by scholars in sociology, political science, and communication studies.

Framing provides a way to think about how information is presented to the public. News frames are persistent patterns of selection, interpretation, and presentation that lend order and meaning to complex issues, processes, and events. They have been described as "pegs" that organize the world for both journalists and their audience. Communications scholars emphasize the value of news frames for journalists: They enable journalists to process and package information quickly and routinely. Because the organizational imperatives of the news business make framing an inevitable feature of reporting on politics, evaluations of media performance should consider several questions about news frames. What is the frame for a particular story? Which alternative frames might have been employed? How does the frame for one story reinforce or undermine frames for other stories? What factors explain the choice of a specific frame? What are the consequences of using one frame rather than another?

Norris argues that the heightened salience of gender in American politics, manifested by such phenomena as the gender gap in voting and the increasing number of women running for public office, provides the basis for explicitly gendered news frames. Gender has become a common peg for news stories about certain voters, candidates, leaders, and issues, prompting Norris to question whether a gendered frame advances or retards women's participation in public life. Since news frames tend to reflect the dominant cultural perspective, groups challenging this perspective often criticize the manner in which the mainstream news media portray their interests and actions. Criticisms of gendered framing focus on the stereotypical presentations of women, the neglect of issues important to women, and the lukewarm evaluations of women's achievements.

To assess these concerns, Norris has assembled thirteen essays whose authors include journalists and a variety of established and emerging scholars in political science and communications. The two chapters in the first section of the book examine the role of men and women as journalists. Four chapters in the second section analyze recent campaign coverage of gender politics. Three chapters in the third section explore changing images of women in power. Finally, four chapters in the fourth section address topics that range from the framing of the women's movement and feminism, to the gendered characteristics of talk radio, to efforts at improving media coverage of women. I would describe each of the thirteen chapters as an attempt to answer one of the following questions: What are the gendered frames used in reporting on women? What factors help account for such frames? What are the effects of these frames?

Several characterizations of gendered news frames emerge from the various analyses. Stories about politics tend to be presented in masculine terms. Accounts of the political gender gap, feminism, and the women's movement oversimplify the nature and significance of these phenomena. Coverage of women leaders, whether heads of state around the world or congressional representatives in the United States, accentuates their roles as pioneering outsiders who are agents of political change. News about these women often focuses on their contributions to policies associated with women's issues, rather than their work in other policy domains. The good news is that these women receive about the same amount of coverage as men in comparable positions, and the coverage is generally free from sex stereotypes.

Explanations for the development of gendered news frames often start with journalists. This book offers evidence that the increasing number of women in journalism has resulted in expanded coverage of women's issues and greater reliance on women as quotable sources. Women journalists are also more likely than their male colleagues to cover women politicians. Revised professional guidelines have helped eliminate blatant sexism from the news, but they have not ensured attention to women on a par with that accorded men. Journalists are not, however, solely responsible for the creation of gendered news frames; women who are the subjects of news stories also contribute to the framing process. Two findings from the book support this point. One is that women campaigning for Congress are more likely to emphasize women's issues as their unique domain of competence. The other is that leaders of the women's movement are sometimes to blame for providing simplistic or conflicting frames about their movement. None of the essays examines how broad cultural forces shape news frames.

As for the effects of gendered news frames, the evidence in this volume is confined to the reactions of potential voters. The findings indicate that women candidates are more likely to be successful when their campaign emphasizes issues stereotypically associated with gender. Contributors to the book also speculate that news framing of feminism may affect the public's understanding of and support for the women's movement as well as the repertoire of strategies available to the movement.

On the whole, my reactions to this book are favorable. The essays range widely over a variety topics likely to interest scholars who study either politics and the media or politics and gender. Initially presented as draft papers at a conference held at the Joan Shorenstein Center on the Press, Politics and Public Policy in August 1995, the chapters have clearly benefited from interactions among the conference participants. All explicitly cross-reference other chapters in the book when relevant points arise. The majority employ rigorous, even imaginative, research designs representing a variety of methods—personal interviews with reporters and editors, focus groups, experiments, surveys of voters and campaign managers, quantitative and qualitative analyses of news coverage.

I do have some reservations. One concerns the use of the framing concept. As sometimes happens when ideas gain

broad popularity in different disciplines, conceptual clarity suffers. We think we know what one another means, but perhaps not. How do news frames differ from other message forms, not only in their structure and content but also in their effects? When is a news frame gendered? When is it not? An extended theoretical discussion about framing as a process of symbolic communication would have enhanced the value of the concept as the book's principal unifying device. My other reservation concerns assessments of the effects of gendered news frames. Several essays raise objections to the ways in which news stories about women and their interests are framed. A recurring example is the complaint that coverage of women politicians overemphasizes their work in policy domains associated with women's issues. Maybe so, but publicizing their expertise in such domains has helped women candidates gain credibility and support among voters. This is the book's only empirically demonstrated effect of gendered news frames. Although a few other possible effects are mentioned, no others are empirically examined. This is an essential step toward identifying what Norris asserts are the "significant consequences" (p. 7) of gendered framing. Without such evidence, we are far from understanding the circumstances under which gendered news frames advance or retard women's participation in public life. Perhaps this volume will stimulate further research on these potential effects.

Race, Politics, and Governance in the United States. Edited by Huey L. Perry. Gainesville: University Press of Florida, 1997. 240p. $49.95 cloth, $21.95 paper.

Georgia A. Persons, *Georgia Institute of Technology*

Huey Perry has produced an interesting anthology on black politics that is focused around the concept of deracialization. His major contention in this volume is that something critical and illuminating needs to be said about the concept of deracialization and its treatment in the scholarly literature up to the publication of this anthology. That argument is strongly implied but is never clearly made, perhaps due to the fact that the editor never directly acknowledges the major scholarly contributions on this topic, such as the works of Charles Jones, Joseph P. McCormick, Zaphon Wilson, and Alex Willingham (see Georgia A. Persons, ed., *Dilemmas of Black Politics: Issues of Leadership and Strategy*, 1993). The omission of a full discussion of the earlier scholarly work in this area undermines the clarity of the analyses in this volume, as Perry does not effectively establish an analytical or conceptual point of departure. That he refuses to make direct acknowledgment of the aforementioned scholars and their works, of which he clearly attempts to make "a straw man," is all the more interesting when one considers the fact that Perry has known each of these contributors as close colleagues for at least two decades.

The title of this volume whets the reader's curiosity but promises much more than it delivers. Much of the failure of the book is due to the rather limited analytical framework set forth in the introduction. One example is what serves as the analytical crux of the editor's framework, that of a distinction between "traditional" black politics and deracialized politics. Such a gross dichotomy obscures the many nuances and major variants of black electoral politics and the perplexing questions and grand contradictions raised by them. In short, one cannot point to a time when the dominant pattern of black politics can be characterized as traditional with any useful distinctiveness. Thus, to draw a dichotomy between traditional black politics and deracialized politics is much too loose an analytical divide to be useful.

The contributions in this anthology by Roger Oden on the election of U.S. Senator Carol Moseley-Braun and by Mylon Winn and Errol Palmer on Norman Rice's election as mayor of Seattle are very useful in that they address the central question raised by earlier work on deracialization. How can a black candidate effectively mobilize a white majority so as to secure victory in an electoral contest in which the historical primary support base for black candidates, that is, black voters, is a significant minority in the population? Such efforts by black candidates on a relatively large scale are intriguing and analytically significant, and they were thus analyzed in earlier works on deracialization although they were not unprecedented occurrences. The most interesting phenomenon that earlier work on deracialization attempted to incorporate was the reach of some black candidates in pursuing offices traditionally not much sought by blacks, such as statewide election at the gubernatorial level and U.S. Senate contests.

The second and more analytically important distinction drawn in earlier work on deracialization involved what had been a dominant but not exclusive pattern of racial insurgency, which had pervaded black politics during a time when a majority of black candidates were seeking to become the first black elected in a given locale and to a given political office. Insurgency quickly faded to a more accommodationist style and even to concerted racial reconciliation attributable to a changed political context, the realities of governing, and increased black-on-black competition that altered the currency of race in unpredictable ways.

The contributions in this anthology cover some electoral contests previously analyzed within the context of deracialization. The volume also contains, however, an assortment of analyses of electoral dynamics surrounding efforts by black contestants to win elective office in a variety of racially defined settings. The reader will not find enough effort devoted to developing and setting forth a framework for analyzing deracialized governance as distinctive from whatever its counterpart might be. Despite the interest whetted by such an idea, there are ample reasons to doubt that development of such a distinctive and informative framework as deracialized governance would be an easy task.

One contribution particularly stands out, the chapter by Lenneal Henderson on Kurt Schmoke's tenure as mayor of Baltimore. The piece sets up a context for addressing the core issues of politics and governance in black-governed cities; that is, a highly complex set of intertwined problems ranging from crime, to the flight of the white and black middle class, to the at once energizing and destabilizing forces of globalization, all of which pivot around issues of political economy. Although readers will long for more development of this analytical path than Henderson provides, they will greatly appreciate his central contribution to the volume and to critical thinking about urban politics generally: White racism persists as a factor in urban politics, race is essentially a shibboleth in explaining much that is of critical importance in the governance of urban America. The challenge for scholars of black politics is to understand this contradictory dynamic and capture it in conceptual terms. We might use the notion of deracialization in this effort but in a wholly new manner if we wish to make significant contributions to either the theory or practice of black politics or to an understanding of the relentless press of broadly based socioeconomic change that forms the crucible of all politics.

God at the Grass Roots: The Christian Right in the 1996 Election. Edited by Mark J. Rozell and Clyde Wilcox. Lanham, MD: Rowman & Littlefield, 1997. 292p. $60.00 cloth, $22.95 paper.

The Christian Coalition: Dreams of Restoration, Demands for Recognition. By Justin Watson. New York: St. Martin's, 1997. 295p. $35.00.

Stuart A. Lilie, *University of Central Florida*

In quite different ways these books increase our understanding of the dynamics of the Christian Right. The Watson book studies the Christian Coalition, focusing on the thought of Robert Reed and Pat Robertson, while the Rozell and Wilcox book analyzes at the state level the role played by the Christian Right in the 1996 elections. Although the Watson book is the outgrowth of a department of religion dissertation, it is a mature, careful analysis free of the usual shortcomings of dissertation-based books. Watson studies the publications and speeches of the Christian Coalition but does not use formal interviews or mass surveys. Early chapters deal with the history of the Christian Right, concluding that the Christian Coalition has institutionalized the evangelical revolution. This institutional treatment will be familiar to students of the Christian Right, but the general reader will find these chapters useful background. All readers will find much originality and value in the systematic analysis of the often-contrasting views of Reed and Robertson.

Watson's background in theology and religious history is reflected in the mastery with which he organizes and critiques the admixture of theology, sociology, politics, and history that characterizes the Christian Right.

The overarching question Watson asks is whether the Christian Coalition, and by extension the Christian Right more generally, seeks recognition or restoration—goals which he finds ultimately incompatible. To seek recognition, "a place at the table," in a pluralist society is ultimately incompatible with a desire to return Christianity to the centrality and primacy it presumably once enjoyed. Recognition is a confirmation of pluralism; restoration is a rejection of pluralism. Watson concludes that both these goals are taken seriously by the Christian Coalition leaders, even though such a position is "confused and contradictory" (p. 179), a contradiction that goes largely unrecognized by Christian Right leaders. In reaching this conclusion Watson rejects two other answers. He dismisses the Trojan horse argument that the Christian Right's plea for recognition is simply a deliberate ploy to gain sympathy and support, with the actual goal remaining restoration. He also rejects the view that a now more politically mature Christian Right recognizes the strategic limitations of attempts to restore a Christian nation, and thus the goal truly has become one of recognition.

While Watson's conclusion has merit if not drama, the major strength of the book is the finely detailed analysis of these two goals in the two chapters analyzing the coalition's "dreams" off restoration and recognition. An illustrative example of the wealth to be found in these chapters is Watson's analysis of the parallels between the multicultural movement, roundly criticized by the coalition, and the coalition's desire to seek proper recognition of its own culture. Both movements express a sense of victimization, recall a usable past, and critique the source of their oppression. Here as elsewhere, Reed and Robertson agree upon the general outline of the problem but differ greatly in their analysis of its nature and source. Robertson, who often manifests Hofstadter's "paranoid style" of politics, sees Christian victimization as part of a larger centralized, even Satanic, conspiracy traceable to the Enlightenment. For Reed, religion has been marginalized because of the social and institutional dynamics of modernity, because of evangelicals' withdrawal from politics, and because of the Left's failure to appreciate its own reliance on religious values as a means of legitimizing change. Reed is correctly understood in the press as advocating a more pragmatic, inclusive approach for the Christian Right. Some of the intellectual differences Watson demonstrates between Reed and other leaders may help us understand Reed's pragmatism as not simply strategic but as also reflecting a less rigid, less conspiratorial view of the problems facing people of faith.

The volume edited by Rozell and Wilcox provides a wealth of background and data at the state and local level in fourteen states. This focus is particularly appropriate given the Christian Coalition's goal of building strength at the subnational level. The chapters reveal tremendous state-to-state variation in the effectiveness of evangelical mobilization. The success of the Christian Right ranges from the consolidation of strength in South and North Carolina, Georgia, and Texas to two states where the movement, for different reasons, is not in contention, West Virginia and Maine. The other states studied, Virginia, Florida, Michigan, California, Washington, Oregon, Minnesota, and Kansas, fall somewhere in between in roughly that order. These chapters are mostly authored by acknowledged scholars in a field where theories and analytic categories have been greatly refined in recent years. They reflect normal science in a way that would have been impossible in the study of religion and politics only decades ago. Each chapter generally includes an historical description of the state's political culture, the specifics of candidates and issues in the 1996 election, the outcomes of the election, and speculation as to the future possibilities for the Christian Right in that state. Published shortly after the election, most of the analysis of individual voting behavior relies on exit polls and aggregate data. Thus, some of the voting behavior analysis may be superseded by fuller surveys. Nevertheless, the institutional and political analysis will remain of value, especially given the tendency of general electoral treatments to neglect and misread the significance of the Christian Right.

Overall, 1996 was a mixed year for the Christian Right. There were significant losses but also signs of consolidation. The chapters in this book make clear that the success of the Christian Right, even in states where the movement is relatively strong, has several limitations. On a statewide basis, as compared to more localized opportunities, candidates closely identified with the Christian Right have great difficulty in being elected, as evidenced by Ellen Carswell's decisive defeat for governor in Washington and by the 1994 defeat of Oliver North for the U.S. Senate in Virginia. When candidates share many of the values of the Christian Right but are not explicitly tied to the movement, their chances are much better, as evidenced by John Warner's reelection to the U.S. Senate in Virginia. Another limitation, found even in states such as Texas, where the Christian Right is largely in control of the GOP organization, is that initial successes often tend to activate and energize groups opposed to the Christian Right agenda. The dynamics of the Christian Right with the Republican Party is still another limitation. In addition to the concerns Republican leaders have about the electability of candidates closely identified with the Christian Right, Republican moderates disagree with the Christian Right on many issues and often see evangelical activists as uncompromising political parvenus. GOP moderates often contribute to the electability problem by labeling as radical the Christian Right candidates in primary battles and some-

times even supporting Democratic candidates in general elections. Thus, while the Christian Right generally needs the Republican Party, ironically it is sometimes more successful in local areas where the party organization is relatively weak. Where the GOP is strong, the inherent limitations of the Christian Right within the party structure are likely to be more acute.

If the movement is to overcome these limitations, Christian Right leaders are faced with a major dilemma. On the one hand, they may broaden the range of issues they support and deemphasize their more strident positions to become more attractive to the Republican Party and the general electorate. This approach is advocated by Ralph Reed and runs the risk of losing core supporters for whom the commitment to these issues is their central motivation. On the other hand, to remain true to these core issues in order to preserve membership risks the continued alienation of Republican leaders and the general electorate. In upcoming elections this debate between the pragmatists and purists will continue. The tensions between evangelical and Republican leaders will also continue, particularly given the more recent debate surrounding the effect of independent political ads on the election of conservative candidates. As these issues and debates unfold, this book provides a wealth of information and analysis to help us understand the commonalties and the subtle variations among the states in the fortunes of the Christian Right.

Race, Law and Culture: Reflections on *Brown* v. *Board of Education*. Edited by Austin Sarat. New York: Oxford University Press, 1997. 238p. $45.00 cloth, $16.95 paper.

Elliot E. Slotnick, *The Ohio State University*

Race, Law and Culture is a collection of diverse essays commemorating the 40th anniversary of *Brown* v. *Board of Education*, drawn from a conference held at Amherst College. Among the contributors are several law professors (Peggy Cooper Davis, Lawrence Friedman, Paul Gewirtz, Gary Peller, and David Wilkins), political scientists (George Kateb and Austin Sarat), an African-Americanist (Hazel Carby), an anthropologist (Carol Greenhouse), and a freelance journalist (David Garrow). These essays "answer" few questions but suggest the range of views about *Brown* and its legacy, underscoring, as noted by Sarat, that *Brown* "unsettled as much as it resolved" (p. 7).

The volume is organized in distinct, albeit closely related, sections loosely mirroring the components of the book's title. Part I, "Brown and Its Legal Contexts," focuses most directly on the decision itself. Part II, "Racial Discrimination and Antidiscrimination," ponders the adequacy of *Brown*'s vision of the antidiscrimination principle as articulated in Earl Warren's opinion. Part III, "Reading the Realities of Race," explores the broad social consequences of *Brown* on American lives, both black and white. While some analysts have raised serious questions about the extent to which *Brown*, per se, played a pivotal role in fostering change in American society (see Rosenberg's *The Hollow Hope*, 1991), none of the essayists represented here takes such a minimalist posture. Still, substantial disagreement can be found among these authors about what, precisely, the decision accomplished. These essays will be justifiably attractive to a wide audience of academicians, legal professionals, students, and laypersons. I will focus, however, on those pieces that will hold particular appeal to political scientists.

Davis's essay, "Performing Interpretation," opens the book by suggesting how the argumentation dance of the opposing lawyers, Thurgood Marshall and Robert Carter for the plaintiffs and John Davis representing South Carolina, served to structure the way in which the case was viewed by the justices. The language of the plaintiff attorneys successfully subverted the image of the United States as a white polity while countering Davis's portrayal of a "closed" constitutional text. Marshall and Carter "seized authority to participate in the shaping of doctrine.... And the Justices followed" (p. 44). For those who "know" the *Brown* opinion, Davis's road map successfully traces the antecedents. One wonders, however, why she did not go farther to demonstrate the plaintiff's success directly through Warren's own choice of words and interpretive approach. Viewed broadly, Davis's essay also highlights the important role of litigators in framing adjudication and the potential importance of oral argumentation in judicial decision making.

Friedman's contribution, "Brown in Context," moves back in time to the "timid hand wringing" that met the decision. We are reminded that in such watershed cases it is all too easy to overstate the importance of the text itself. Thus, "anybody who feels that a more tightly crafted and better reasoned opinion... would have had a bigger (or even different) impact on life in rural Mississippi (or even Washington, D.C., for that matter) than what Warren actually wrote, can't be living on this planet" (p. 51). Friedman's persuasive critique of "the world of the legal mandarins" will, undoubtedly, provoke a good deal of contumely: "It is a deadly sin of law professors to imagine that their little internal quarrels have a vital effect on the actual impact of cases" (p. 65). Friedman also explores the manner in which *Brown* cast aside the formalistic "person from Pluto" approach to legal reasoning dominant in previous litigation. Ultimately, it is difficult to find fault with Friedman's conclusion that what drove *Brown* "was the strong sense that segregation was just plain wrong" (p. 61).

Flowing nicely from the Friedman piece is Garrow's essay, "From Brown to Casey," which argues that *Brown* stands for the repudiation of historical intent as an interpretive device. Marking the advent of the "modern" Court, *Brown* fanned policy activism by relying on a "dramatically expansive and aggressively anti-historical reading and application of the Fourteenth Amendment's Equal Protection and Due Process Clauses" (p. 75). The result has been a "better America precisely because of how the Court has ignored the Constitution's historical limitations" (p. 75). Garrow concludes that "substantive due process is—as it should be—a fundamental and fully accepted aspect of present-day American constitutional doctrine" (p. 83).

The thrust of Garrow's essay, like its predecessors, goes well beyond *Brown*. Problematically, however, these chapters (as well as others) seem to take it as a given that interpretive judicial backsliding on the Court both will not and cannot occur. At bottom, these readings tend to minimize or ignore a substantial array of legal scholarship, popular writing, and court decisions in recent years that have circumscribed federal power. The sense of irreversibility about the thrust and nature of judicial power (and not, of course, about the holding of *Brown*) that permeates these essays simply does not ring true. Many chapters (add Gewirtz's "The Triumph and Transformation of Antidiscrimination Law," discussed below, to this brew) are written almost as if the 1980s never occurred and are both not alive in some quarters today and may not possibly flourish in the future.

Gewirtz's essay, perhaps more than any other, underscores both the broader implications of *Brown* beyond race (in locales such as discrimination premised on gender, age, disability, and religion) as well as the substantial changes that

have occurred in the antidiscrimination principle. Thus, while the antidiscrimination model has "triumphed" in the sense that the domain of its applicability has dramatically widened, the fundamental meaning of nondiscrimination has been altered. Whereas, traditionally, nondiscrimination meant treating people the same, it has come to underscore the necessity of treating them differently in order to accommodate differences. Ironically, the one area in which such a transformation has, arguably, not taken place as strongly as Gewirtz suggests is the area of race-based affirmative action. Indeed, one (perhaps unintended) consequence of Gewirtz's analysis is to demonstrate how far out of synch contemporary societal controversy on affirmative action is when contrasted with other areas in American life that have been more receptive to accomodationist policies.

Gewirtz's thoughtful contribution balances the benefits and the liabilities of the trends he examines in an unabashedly self-searching analysis. Particularly troublesome for the author is the transformation of the societal rationale for affirmative action from the necessity of correcting past discrimination to the seemingly less justifiable goal of seeking diversity for its own sake. While one may disagree with Gewirtz about whether changes in the equality principle have been salutary, it is most important to point out (as the author does not) that such changes have not been, necessarily, reflective of philosophical societal shifts in defining equality or, necessarily, the consequence of preferred choices by civil rights advocates. Rather, they reflect, at least in part, tactical choices made in the wake of *Bakke* and other decisions, where the diversity rationale, for all intents and purposes, was the only approach to pursuing affirmative action that received judicial sanction. Clearly, the movement to the diversity rationale may have been driven by the Court at least as much as it has been by the civil rights movement.

The remaining contributions to the volume are worthy of the reader's attention, although they are somewhat less focused on *Brown* per se and appear less likely to tap the sensibilities of "mainstream" political scientists. Kateb, for example, argues that Harlan's *Plessy* dissent did a "better" job than Warren's *Brown* opinion in identifying the many recipients of segregation's harm, including the harm done to the majority race as well as to the notion of republican citizenship. While persuasive, it is difficult to approach Kateb's analysis without summoning up Friedman's image of the debates among legal mandarins. Other essays run much farther afield, tapping some of the exogenous effect of the *Brown* ruling.

In the final analysis, the reader leaves *Race, Law, and Culture* in much the same way that one might a very filling and satisfying multicourse meal from an unusually eclectic and well-conceived menu. You have tasted numerous flavors as well as some combinations that you have not experienced before. Not all are equally memorable, yet none have left a bad taste, and all the dishes were well worth trying. I have little doubt but that you will recommend this restaurant to others and will look forward to comparing your dining experiences.

The Betrayal of the Urban Poor. By Helene Slessarev. Philadelphia, PA: Temple University Press, 1997. 204p. $59.95 cloth, $18.95 paper.

Michele Hoyman, *University of Missouri-St. Louis*

In *Betrayal of the Urban Poor*, Slessarev asks a central and compelling question in urban politics and political economy: Why have social policies failed to alleviate the problems of the urban poor? The governmental solutions to the problem of the urban poor were myriad: equal employment opportunity legislation (Title VII of the Civil Rights Act of 1964), job training programs, fair housing acts, and minority set-asides, among others, all of which Slessarev critiques as ineffectual. In the current environment, it is both refreshing and rare that a scholar (or activist) is asking this question, either from a normative or an empirical perspective. In the 1960s this question dominated the social discourse, and there was an urgency to it being answered, since there were several mass mobilizing movements: the civil rights movement, the community organizing movements among the urban poor, and the anger boiling over into urban riots. In my view, the question of how best to end urban poverty remains an important and compelling question in terms of justice and in terms of social science research, and I applaud Slessarev's efforts at keeping the question alive. Slessarev is uniquely situated to address this issue, being a Chicago insider, former activist, and social researcher.

The richest parts of this book are the tales from Chicago about urban political experiments, grassroots organizing efforts, and social reforms, with the fascinating Chicago politics as a backdrop. I found the author's analysis of Mayor Harold Washington's time in power particularly poignant. In fact, I found the Chicago story subtext much more convincing and interesting than the general federal policy subtext.

Slessarev's thesis is that the raft of social programs passed in the 1960s failed to ameliorate the problems of the urban poor. This is not startling news, since many liberals and conservatives alike wrote the obituaries of these programs, some of which are now defunct. Slessarev's conclusion is really also her starting premise. The author does not adduce an overwhelming amount of evidence to support her conclusion, but instead accepts it as a given, although not a controversial one. She spends more time and data on explaining the failure of the programs and developing the reasons for their failure. She blames the very interest groups and advocates who lobbied to get these programs passed—the Democratic Party, organized labor (and particularly the craft unions in the construction industry), and middle-class blacks.

The strong points of this book lie in the analysis of the social history that is conveyed vividly to the reader: the context in which each of these policy solutions was born, how the flaws were evident at the program's inception, and the fact that some of the weaknesses in the policy or program were deliberate. Chapters 1–4 are a good read as regards the social case for the goal of economic equality for the urban poor. As such the book should be required reading for every new student of urban politics. For the many undergraduates who may ask themselves or their instructors why a particular program was needed, Slessarev provides a full and compelling answer.

The book is ambitious in the range of policies it examines: equal employment opportunity legislation (such as Title VII); job training and job creation programs; a Chicago case study of problems of access for minorities to skilled trades jobs; minority business set-aside programs and the inevitable capture by civil rights and black political leaders of the benefits from minority set-asides; Community Development Corporations; Office of Economic Opportunity; the modern-day enterprise zones; and business tax subsidies. It is to the author's credit that the book addresses the wide gamut of social solutions.

This book excels at social history and social dialogue but does less well at objective social science research. It seeks to explain the context in which each of the legislative ideas was born—such as its origins and social history—and the author

does this extraordinarily well. The book also seeks to identify the culprits (the actors who derailed the purpose of each of these policies) and whose interests dominated over the quiet and powerless interests of the urban poor. The author blames organized labor (and its effort to protect seniority) for the failure of Title VII of the Civil Rights Act, middle-class blacks for exploiting the profits of the minority set-aside programs without doling out the benefits to the underclass (the "trickle-down" effects of economics), and the Democratic Party, particularly the Democratic machine in Chicago.

There are two social policy cases in which I found Slessarev's analysis to be less than convincing. The author could have explored some alternative hypotheses or at least moderated her indictment of her "usual suspects." These cases were (1) the equal employment opportunity legislation (Title VII), with regard to which Slassarev argues that unions blocked attempts to make the civil rights act benefit the urban poor, and (2) the access of minorities to the building trades, regarding which she again argues that unions blocked the upward mobility of the urban poor. I will spend some time amplifying why the equal employment case is not as simple as she describes. The goal of that legislation in the view of many scholars was not necessarily to redistribute income from rich to poor, as the author suggests. It was to end discrimination in employment. Most congresspersons who voted for it probably conceived of it as ending overt acts of discrimination, but the courts later defined discrimination to include discriminatory effects. The legislative history would bear out this point, for example, the amendment to include "no preferential treatment" in the bill.

The major problem with Title VII compliance was weak enforcement, not the labor movement's protection of seniority. As a policy of regulating race relations (which admittedly was potentially redistributive), Title VII suffered many more severe handicaps than just the labor movement's insistence on inserting in Title VII the 703h. clause, which preserved bona fide seniority systems. The enforcing agency for Title VII, the Equal Employment Opportunity Commission, was crippled from the outset, since it had no enforcement powers until the statutory amendments to the Civil Rights Act in 1972. It had to rely on the Department of Justice to enforce the law. Enforcement of Title VII can only occur when an individual employee is brave or foolish enough to file a charge against the employer, as opposed to the automatic review of wage data that occurs in Great Britain, which increases compliance. Weakness in compliance is just one rival explanation that the author could have developed a bit more.

Another factor Slessarev ignores is labor's broader social and economic role. For example, without the support of organized labor, Title VII would probably never have been passed. Also, the role unions play in upward wage mobility for workers, especially for blacks and females, is understated in the book, except for a passing reference that Martin Luther King signed on to the union organizing campaigns for black workers in the South. Even today, union representation is a major predictor of whether black and women workers will experience the black-white or male-female wage gap or will achieve wage parity by occupation with their white male counterparts.

It also needs to be pointed out that seniority is not in and of itself discriminatory. Why do unions (and congresspersons) fight so hard to preserve seniority? Is it because older is better? No. It is because seniority is a conflict-free neutral way of distributing scarce resources, such as promotions, premium vacation days, or office space (Congress), and for eliminating the favoritism, even racism, that employers might use as a selection criterion in the absence of seniority. I am not refuting what the author says about the practice of segregated locals. To put this practice in an accurate social historical context, however, she should give the reader some notion of just how segregated all the other American institutions were at the time, particularly the ones governed by the democracy of a white majority, such as Congress, school boards, and state legislatures.

While the courts did rule from the late 1960s to the late 1970s, in cases such as *Quarles* v. *Phillip Morris*, that narrow forms of seniority locked in present effects of past discrimination (hiring, for instance), three additional things need to be pointed out. First, the Supreme Court in 1977 reversed *Quarles*, a fact not mentioned by Slassarev. In *Teamsters* v. *United States* (431 U.S. 324, 1977) the Court upheld any seniority system which was bona fide. Second, the seniority system is neutral. Only if it is combined with point of hiring discrimination, that is, hiring all blacks into one department and all whites into another, would there be any discriminatory effects of even narrow forms of seniority. For instance, at the time the United Steelworkers of America and thirteen major steel companies signed the consent decree ending a federal court case of discrimination, the average number of years of company seniority did not differ for blacks and whites, nor did the average total earnings (counting overtime) for blacks and whites in the Alabama steel mills. Yet, some departments within the steel mills were all black and others were all white, a function of employer hiring. Third, unions are bound by the regulation of labor laws, such as the Landrum-Griffin Act of 1959, requiring that they hold democratic elections, so they are often caught on the double pincers of a white male majority constituency versus advocacy of minority rights. In summary, Slassarev blames unions for what under U.S. labor law is solely a management right, the right to hire.

I am not claiming that unions had totally clean hands, but their hands were no dirtier than other democratic, majoritarian institutions in that era. There is much evidence, not looked at by Slassarev, that union bureaucracies such as the International Union of Electrical Workers (IUE) and United Steelworkers of America did bring considerable pressure to bear on locals to comply with civil rights laws. Winn Newman, legal counsel for IUE and others, was a champion of civil rights within the labor movement, and he brought landmark pregnancy discrimination and comparable worth suits against employers in the federal courts when employers resisted Title VII compliance at the collective bargaining table. The United Auto Workers also has a much more aggressive record on social policies than is suggested by Slassarev, although her archival research on the internal review boards was certainly interesting and original. The United Steelworkers of America invested enormous resources in setting up a network of regional civil rights compliance officers solely to assist locals in complying with civil rights goals. The Textile Workers of America and Amalgamated Clothing Workers of America were successful in organizing integrated shops or majority black shops in the South, to bring the dual gain of economic and social justice to workers in Oneita Mills and J.P. Stevens, far ahead of the social curve in the Carolinas.

Thirty years after these programs, we still have urban poor. The author paints the picture a bit too starkly. I am not as certain as she that the villains would be labor, the black middle class ("Black leaders representing interests of middle class supporters was one of the four reasons why the Black economic goals were not fulfilled," p. 6), and the Democratic Party. What is clear is that Slassarev asks a very important

question and engages the social dialogue on this point. Again, kudos are deserved for keeping this ember alive.

Pasteur's Quadrant: Basic Science and Technological Innovation. By Donald E. Stokes. Washington, DC: Brookings Institution, 1997. 196p. $38.95 cloth, $14.95 paper.

James A. Desveaux, *University of California at Los Angeles*

In *Pasteur's Quadrant*, the author calls for a complete reassessment of the linear model of science and technology, embraced since the publication of Vannevar Bush's landmark *Science: The Endless Frontier* (1945). Stokes's farewell contribution to social science arrives at a most propitious time, as policymakers and leaders in the scientific community try to justify continued public support for science and technology in a post–Cold War environment.

Bush, who was director of the Office for Scientific Research and Development (OSRD) under Roosevelt during World War II, offered a blueprint for postwar research and development policy. He feared that unless specific safeguards were created, and boundaries established, support for basic research would give way to applied. In his book, he set about to justify the importance of publicly funded basic research and to enshrine as a matter of public policy the bifurcation between basic and applied. Stokes argues that the work, and especially the interpretations attached to it in later years, skewed our understanding about the connection between science and technology, establishing a false dichotomy. According to Stokes, though Bush argued that only those nations lending strong support to basic scientific research would remain powerful industrial competitors (through knowledge transfer to other sectors), he knew that science and technology were really quite interdependent. For instance, the United States had become one of the preeminent powers in industrial technology well before it achieved the prowess of the Europeans in basic science.

Stokes makes the point that linear thinking about how basic science feeds applied research is empirically weak. In one Department of Defense survey he cites, an analysis of hundreds of critical events contributing to the development of twenty weapons systems showed that fewer than one percent of those events owed anything to nondefense basic research, and less than 10% were connected to basic research of any kind. The author's essential point is that sometimes technology leads to and is closely tied to fundamental research, and we may find ourselves at a considerable disadvantage if we fail to acknowledge the importance of use-inspired research and development. Japan is held as the obvious example of a nation that has explicitly encouraged use-inspired research.

Pasteur's Quadrant has two primary purposes. First, it seeks to understand the logic and history behind the post–World War II science and technology paradigm and to offer a thorough critique of it. Second, and more significant, Stokes wants to substitute for this basic-applied dichotomy a different and more realistic model. He argues that all research may be characterized according to two considerations: Does it constitute a quest for fundamental understanding? Is the inspiration for it based on considerations of use? Each question forms an axis of a four-cell matrix. The book takes its title from the quadrant of the matrix describing research as driven by a quest for fundamental understanding *and* inspired by considerations of practical use. Louis Pasteur's research career illustrates the thrust of Stokes's perspective. "It deserves to be known as Pasteur's quadrant in view of how clearly Pasteur's drive toward understanding and use illustrates this combination of goals" (p. 74). Stokes describes fundamental research conducted in the absence of any considerations of use as "Bohr's quadrant," because the giant of quantum physics was involved purely in discovery. Research driven entirely by considerations of practical use, without concern for fundamental knowledge, Stokes terms "Edison's quadrant."

The author maintains that his two-dimensional model for thinking about science and technology makes sense for two reasons: It more accurately depicts the purposes behind the research enterprise, and it also helps us to connect the work of scientists with the larger policy implications of what they do, who should pay for it, and how and why we should care. In the process, he presents a convincing case for reassessing the conventional wisdom about basic and applied research and the relationship between science and its "downstream" applications. Much of the research in the United States is not conducted in neatly segregated strata. The interplay between basic and applied research *is* complex, and there *are* important distinctions within each category. Yet, there are formidable political and organizational challenges to implementing Stokes's model. One obstacle is that fundamental research is generally conducted along disciplinary lines, and departments and universities have often resisted attempts to synthesize advances in a given field with considerations of the practicality or usefulness of that research. As beneficiaries of the established paradigm, they have incentives to try and maintain it.

Another obstacle is that funding agencies must somehow justify their expenditures to Congress. When an agency like the National Science Foundation funds basic research, the choices it makes are likely to be less controversial (even if members of Congress regard those expenditures as silly or frivolous) than if the research had more obvious practical application. For instance, Washington's recent technology programs—designed explicitly with an eye toward the practical—have been labelled "corporate welfare." Furthermore, the more use-inspired the research, the more likely it is that funding agencies will seek to view the worth of projects according to their own organizational perspectives. As Stokes admits, agencies are generally careful about how they define themselves and their mission, and it is difficult to see how they would be forced to overcome their parochialism with a more catholic perspective. And congressional committees tend to reinforce this tendency for agencies to compartmentalize. Bush had advocated the creation of a National Research Foundation after the war as a superordinate agency to coordinate all research, and perhaps such an agency could help make Stokes's prescriptions a reality.

Stokes holds up the National Institutes of Health as an organizational model for successfully carrying out research in Pasteur's quadrant. NIH may be exceptional, however, in using a peer review process while taking into account the nation's medical needs in funding decisions. Though other research agencies have had their budget curtailed or have come under attack (such as the National Institute of Standards and Technology), Congress has been consistently generous in its NIH appropriations. Congress apparently has little problem with funding biomedical research that is clearly use-inspired and that may benefit downstream users, such as firms. Yet, many critics reject the idea that nonbiomedical taxpayer-funded research should benefit, say, commercial users, except as an accident.

Vannevar Bush may have understood the politics of research funding well enough to reify a distinction, and construct an artificial barrier, between science and technology in order to justify and buffer many of the research institutions in

the United States. He may well have recognized that many future research efforts would be neither entirely basic nor applied, but that the scientific community could live with this anachronism and deal with it informally.

Donald Stokes demonstrates a very deep understanding about how we got to where we are, what is wrong with and hypocritical about this dominant perspective of a bifurcated science and technology policy, and why it urgently needs reassessing. As a bonus, he offers a new paradigm in its place. The real challenge is to convert some of these ideas into a workable implementation strategy acceptable to policy leaders and dominant interests in the science and technology community. This is a fine addition to the social science literature on science and technology policy. And, consistent with his prescriptions, the author has written a very *useful* book for political, industry, and university leaders in the United States.

Faces of Feminism: An Activist's Reflections on the Women's Movement. By Sheila Tobias. Boulder, CO: Westview, 1997. 332p. $24.00.

Martha A. Ackelsberg, *Smith College*

Right at the beginning of this fascinating analysis of the contemporary U.S. women's movement, Sheila Tobias notes that it is "not a scholar's book for scholars" but a personally engaged study, part memoir, part history. The combination is powerful and effective: She brings to the task both her own history of extensive and varied involvement in the women's movement and a thoughtful and provocative analysis of its strengths and weaknesses over time. The book is, as a consequence, an excellent resource for those wishing to understand both the trajectory of the women's movement and the larger U.S. political context in which it took place and which—in Tobias's view—profoundly shaped it.

The greatest strength of the book is its insistence on locating the movement for women's liberation within the context of late-twentieth-century U.S. politics. As Tobias notes (p. xii), "feminists may think they led a unique revolution, but in fact theirs is a classic example of democratic reform employing multiple strategies against multiple targets." Tobias takes the reader from the heady days of the 1960s civil rights, antiwar, and countercultural revolution through to the contemporary era, one—she argues—that is characterized more by backlash and retrenchment in the larger political arena and by fissures within the women's movement. In the process, she does an extraordinary job of offering a broad-ranging multicultural history of the women's movement.

The book's structure is part chronological and part topical. A first chapter sets the terms of the discussion, arguing that feminism, most basically, is about the politicization of gender. While nineteenth-century suffragists and women's rights advocates had focused primarily on gaining equal access for women to the public/civic arena, and Margaret Mead's *Sex and Temperament in Three Primitive Societies* (1935) had argued that gender roles are culturally constructed, it was Kate Millett's *Sexual Politics* (1970), with its focus on "patriarchy" as an ideology and set of practices that create and justify the subordination of women to men, that provided the theoretical grounding for what Tobias terms "the new feminism" (commonly termed the "second wave").

The chapters that follow trace the historical development of the U.S. women's movement from the nineteenth through the mid-twentieth century. The particular strength of these chapters is that they both locate women's struggles in the context of larger U.S. political issues and movements and, even more unusually, effectively weave together many strands to create a whole. This book goes beyond what has become the conventional acknowledgment of the roots of feminism in the nineteenth-century abolitionist and twentieth-century civil rights movements to integrate the experiences, struggles, and achievements of native, African American, Mexican American, and working-class/labor union women throughout these events. What results is a truly multiclass, multicultural history of women's struggles in the United States, an achievement that, in itself, is no small feat.

In the remainder of the book (roughly two-thirds of the total), Tobias explores what she terms the three "phases" of the movement, arguing that what has characterized its history is a shift from near-unanimity among feminists about goals and strategies to the contemporary situation of ever-greater differences and fracturing. The first phase, roughly 1968 to 1972, focused on "role equity" issues, "those rooted in the moral imperative that all people, at least as far as government is concerned, ought to be treated the same" (p. 93). The role equity phase, characterized by a high degree of agreement among feminists as to what the issues were and how they should be pursued, demanded "equal access" and "equal opportunity" for women and equal treatment for women "without challenging the notion that men and women are different" (p. 94).

The second phase moved beyond role equity to what Tobias terms "role change," characterized by a questioning of traditional sex roles and attempts to create new political categories. This phase highlighted somewhat more controversial issues, such as redefining rape as assault, sexual harrassment, affirmative action, abortion (and the related constellation of issues around pregnancy "disability," feminist demystifications of motherhood, and so forth), and culminated in the ERA battle. During this period, Tobias argues, while feminists remained mostly in agreement among themselves about both issues and strategy, they began to run into more effective political opposition, the most dramatic example of which was Phyllis Schafly's StopERA. Here, Tobias offers an interesting analysis of why feminists were not able effectively to counter Schafly's movement: They failed to understand the real anxieties of many women, could not deal with the issue of women in the military, and did not give sufficiently high priority to passage of the ERA until it was too late.

The third and final phase constitutes the contemporary one, characterized by issues on which feminists disagree among themselves. These include women in the military, comparable worth, the "mommy track," pornography, and "illegitimacy," in addition to issues such as sexual orientation, class and racial differences, and understandings of the nature and characterization of "families," topics which have divided feminists to one degree or another since the mid-1970s. Tobias's discussion of comparable worth and the "mommy track" are particularly well done, and her chapters on sexual orientation and abortion provide excellent brief histories of these issues.

The final few chapters pick up and develop on these themes of increasing difference and division. On the more "upbeat" side, Tobias offers an excellent discussion of the relation between feminist theory and feminist politics, as well as an appreciation of the scope and influence of feminist scholarship on the academy and the larger culture. The titles of the final three chapters, however, reveal Tobias's major concern: "Fissures into Fractures," "Surviving the 1980s," and "The End of a Movement." Feminism as a movement, she argues, is effectively dead: The political center in the

United States has moved so far to the right that the early successes of the women's movement can hardly be repeated. In fact, not only are feminists divided among themselves about what the new agenda should be, but also they are increasingly fighting rearguard actions to defend rights (e.g., abortion) seemingly long-since won. Even more, feminist wariness of mainstream, partisan politics may be counterproductive at the present time: Feminists, she argues, are in danger of remarginalizing themselves, just at the moment when full incorporation into the mainstream is within view. In what seems to me a strange shift from her discussions of the energy of the early independent women's movement, Tobias argues that the challenge for the future is to work out a compromise between feminist goals and mainstream political activity.

Still Seeing Red: How the Cold War Shapes the New American Politics. By John Kenneth White. Boulder, CO: Westview, 1997. 386p. $29.95.

Howard L. Reiter, *University of Connecticut*

Most attempts to explain changes in American party politics over the past several decades have focused on domestic trends, such as race, economics, culture, and technology. In this fact-filled volume, John Kenneth White of Catholic University places Cold War considerations uppermost as a way to explain how the Republican Party climbed out from under the New Deal juggernaut, then dominated presidential elections for decades, and finally lost its presidential footing after 1991. Those Cold War factors, White points out, entailed both anticommunist ideology and foreign policy. In his view, it is far from coincidental that the end of the Cold War was followed by the first two consecutive Democratic presidential victories in thirty years.

Still Seeing Red is organized chronologically, beginning in the late 1940s and ending fifty years later. Switching back and forth between the major parties, White presents the Republicans as diverting the electorate's attention from social welfare issues in order to play to their strengths of ideological orthodoxy and diplomatic and military firmness. Only when the Soviet Union collapsed was the party left without purpose and cohesion, a disorientation reflected in the most recent presidential campaigns. Beginning with Henry Wallace and continuing through all the campaigns of the 1970s and 1980s, the Democrats proved vulnerable to charges of softness on communism and foreign policy disarray. As the Cold War ended, the voters shifted their attention to domestic affairs, with happy results for Bill Clinton. Yet, the 1990s have also seen increases in economic inequality, political polarization, and lack of common purpose. Nevertheless, White ends his narrative optimistically: "The anger that has characterized the post–Cold War political contests is beginning to ebb, and a greater sense of personal and political responsibility is gradually emerging" (p. 285).

With a wealth of telling detail, White succeeds in demonstrating that parties scholars need to qualify their explanations of political trends by taking foreign policy and anticommunism more into account. No one who reads *Still Seeing Red* should ever again ignore the important role of the Cold War in helping to shape presidential election outcomes of the past half-century. In correcting our emphasis, White has performed a valuable service.

Given the many strengths of this book, the reader wants even more. The volume is long on narrative but short on theory building and methods, and this occasions numerous questions. How, for example, does this study fit into theories of agenda setting; under what circumstances does ideology or foreign policy rise to the top of the agenda? How did the Republicans succeed in moving social welfare off the agenda in 1952 after failing to do so in 1948? In light of Cold War references in such documents as the 1947 Civil Rights Committee report, *To Secure These Rights*, and the amicus briefs in *Brown* v. *Board of Education*, what are the connections between the Cold War and race? (A book whose introduction is entitled "Who Are We?" might have explored how white America's external "other" intersected with its internal "other.") Why were Republican disputes over foreign policy confined largely to the early Cold War years, while Democratic infighting crystallized with the war in Vietnam? How can we integrate into this analysis the fact that Democrats controlled Congress throughout most of the Cold War, while Republicans have dominated that branch only after the dissolution of the Soviet Union? Finally, given White's frequent citation of survey results, can careful multivariate analysis demonstrate the effects of the Cold War on party identification and presidential election voting that he claims?

Like most authors who write corrective works, White at times overstates his case. One of the charms of *Still Seeing Red* is the periodic reference to events in the lives of future leaders, but White's suggestion that it was the Cold War that first inspired the teen-aged Bill Clinton and Hillary Rodham to go into politics (p. 14) seems a stretch. White also exaggerates the dovish nature of the Democrats in recent decades. For example, his presentation of Walter Mondale's presidential nomination as a product of New Left agitation is based on selective quotation of the Democratic platform that year and ignores Mondale's call for quarantining Nicaragua. At times, too, White suggests that any reference to foreign policy, even Richard Lugar's citing of his Foreign Relations Committee experience in the 1996 campaign (p. 227), is a Cold War artifact. Finally, White might have paid greater attention to the periods within the Cold War. During its first two decades, when Soviet-American tensions were highest, the Republicans won a lower proportion of presidential elections than during the latter two decades of detente. How does this fit with White's analysis?

These questions and differences of judgment illustrate that *Still Seeing Red* is bold enough to state a clear thesis and big enough to chew on and argue with. That it is not the last word on the subject indicates my hope that it will inspire others to continue down the path White has broken. With the publication of this book, he has reminded us that it is not just the economy, stupid.

The Recall: Tribunal of the People. By Joseph F. Zimmerman. Westport, CT: Praeger, 1997. 194p. $55.00.

Charles Press, *Michigan State University*

Zimmerman's study of recall at the state and local level supplements his previous works on participatory democracy, populism, and the Massachusetts town meeting. Its strengths are in its thorough examination of the legal aspects of recall, including relevant judicial decisions, and in the philosophical arguments for and against recall of elected officials. The author systematicly and fairly presents the arguments pro and con before aligning himself with those who favor the recall.

Zimmerman opens with an overview of the recall movement. Its roots lie in the Jacksonian distrust of public officials with which its Populist formulators agreed. Its first use was in 1903 against a Los Angeles councilman, who was successfully recalled, and those who came to be supporters of the

Progressive Party of 1912 helped its spread. Opposing initially were President Taft and influential members of the American Bar Association. By 1913 it had been adopted in nine states, and now the constitution in sixteen states authorizes the recall in one form or another. Ten additional states have statuatory provisions, some allowing the procedure only in home-rule cities. The chapter ends by suggesting that the present decline in voter participation has rekindled interest in forms of participatory democracy such as the recall.

Chapter 2 examines the differing legal provisions that allow recall only for cause, that is, for malfeasance, misfeasance, or nonfeasance, or that permit the voters to recall an official without giving a specific reason. The author then examines the varying provisions for petition gathering, the recall election, and so on.

Next follows a review of the few elections in which state officials, including legislators and judges, have been recalled and a discussion of the advisability of recalling judges. Zimmerman then attempts to assess the prevalence of recall at the local level, which is difficult since no central records are kept. He ends the chapter with examples of local recall elections culled from descriptions in books, magazine articles, and journals. He concludes that recall has been seldom used, despite fears of its opponents. The states most frequently using the recall are California, Michigan, and Oregon.

Zimmerman then presents the arguments pro and con and adds a final chapter, "A Model for Voter Sovereignty." He outlines provisions that state constitutions and local charters should include, in his view, to ensure participatory democracy. The discussion ranges widely from initiative, referendum, and recall to open meeting laws and other features. Finally, an extensive bibliography on recalls is appended.

The book falls short in two respects. First, Zimmerman cites only the Lawrence Sych analysis of voting patterns when an official was recalled. Zimmerman can hardly be blamed, since few such empirical studies have been made. Sych found in a recall of a Wisconsin assemblyman that conversion of former supporters was the main factor explaining the result, and partisanship and turnout were unimportant. It appears that in a successful recall election the proponents are able to frame the issue in a way which cuts across party or other allegiances that determined the outcome of the original election. One would like to see more precinct analyses or polling of citizens of local recall elections, rather than only anecdotal reports.

Second, the author cites several times the fact that the courts in Pennsylvania and Connecticut gutted the state's recall provisions for home-rule cities. Other state courts upheld such provisions. One suspects that legislators as well, by making the provisions for the use of recall more difficult, may also be restricting its use. I think the book needed to examine in one place whether the opportunity of citizens to use recall has been increasing or decreasing. While the dates for adopting recall through 1913 are listed, I could not find such dates for the remaining adoptions. Has any state, except perhaps Alaska, adopted or expanded its recall provisions in the last fifty years or so? Have any such expansions, if they exist, only applied to recall in local elections? I note that the list of the twenty-six states that have recall in their constitution or by statute shows nineteen are west of the Mississippi, generally in areas of Populist and Progressive strength. Fifteen of these have recall in their constitution. Of the seven states east of the Mississippi, only Michigan has recall in its constitution.

Despite these caveats, Zimmerman has produced a study that students of participatory democracy will find and, in his view, public officials and citizens should find useful. It is now the major reference work on the subject.

Comparative Politics

Dilemmas of Democracy in Nigeria. Edited by Paul A. Beckett and Crawford Young. Rochester, NY: University of Rochester Press, 1997. 450p. $65.00.

John A. Wiseman, *University of Newcastle upon Tyne*

It is a central paradox of Nigerian politics that, despite a military government for nearly three-quarters of the postindependence period, there remains a widespread and strongly held belief among Nigerians that military rule is an aberration and what *ought* to exist is a democratically elected civilian government. The tenacity of this belief is all the more remarkable given that during the two periods of civilian rule, from 1960 to 1966 and from 1979 to 1983, Nigerian democracy was deeply flawed and was marred by high levels of ethnoregional tension and corruption. This attachment to the idea of democracy as the norm has to a large extent been officially internalized by the military. Therefore, military intervention is presented by its perpetrators as an emergency response to a specific crisis, and military rule is justified on the grounds that it is preparing the way for the creation of democratic civilian rule. No regime in Nigeria has ever attempted to present military rule as a permanent fixture.

In their introduction to this excellent collection of essays, the editors suggest the insightful formulation of "permanent transition" to encapsulate the essential features of politics in most of the postindependence period. Indeed, in his own solo contribution, Crawford Young goes even farther by suggesting that the terminal colonial period between 1945 and 1960 can be looked at in a similar light. Permanent transition has two crucial aspects. First, military regimes are legitimized on the basis of what they are (supposedly) moving toward rather than on the basis of what they are. Second, the whole convoluted process of transition enables the military to incorporate significant elements of the political class into the procedures associated with transition (drawing up constitutions, regulating political parties, and so on), thus creating "a confused and chaotic form of military-civil diarchy" (p. 5).

This book had its origins in a conference held at the University of Wisconsin-Madison in November 1995. This was a little over two years after the transition steered by President Babangida was aborted at the last minute with the annullment of the June 1993 presidential election. The conference was given added poignancy when, as it was meeting, the news came from Nigeria that human rights campaigner Ken Saro-Wiwa and eight of his colleagues had been executed by the military government. It is reported that conference participants, a large majority of whom were Nigerians, shared "a consensual understanding of the ever-increasing centrality of the question of democracy to the future of the Nigerian polity" (p. xiv). This reflects a fairly

widespread perception among observers of Nigeria, perhaps the most eloquent of whom is the Nigerian Nobel laureate Wole Soyinka, that unless the country can create a workable democracy, the most likely outcome is the disintegration of the state.

The nineteen essays in this collection cover virtually all aspects of permanent transition in Nigeria. The most detailed attention is paid to the analysis of the Babangida period and the evolving situation under Sani Abacha, but enough historical background is provided to locate recent developments within the political trajectory of postindependence Nigeria as a whole. While a chapter focusing directly on the military and its increasingly factionalized structure would have been extremely welcome, one has to recognize that this topic is probably too politically sensitive within Nigeria for the necessary empirical research to be undertaken.

Because shortage of space in this review makes impossible individual consideration of all nineteen contributions, I will focus on the most innovative and/or controversial. Two essays destined to provoke controversy for different reasons are those of Peter Ekeh and Sabo Bako. In a splendidly revisionist piece, Ekeh attributes much of the responsibility for the problems associated with constructing democracy in postindependence Africa to the failure of African nationalism to promote the rights of the individual as opposed to "people's rights." He concludes that "these meanings of freedom and democracy contributed substantially to the tyrannies that have arisen in Nigeria and elsewhere in post-colonial Africa" (pp. 91–2). Bako's essay is likely to prove even more controversial, especially among southern Nigerians. From the perspective of most of the latter, successive military regimes in Nigeria, particularly since 1983, have promoted northern/Muslim interests at the expense of southerners and Christians: Attaching Nigeria to the Organization of Islamic Conference (OIC) in 1986 is often cited as an example of Muslim bias in the government. Bako, however, argues that Babangida (himself a Muslim) operated a "systematic bias against Muslims and Muslim interests" (p. 294). This is, to say the least, an interesting and challenging view.

The essay by Oyeleye Oyediran benefits from a participant-observer perspective. The author was a nominated member of the Political Bureau, which was established by Babangida (described by Oyediran as a "wily dictator") to design his transition to democracy. The essay contains many insights, especially in relation to disagreements within the bureau, which would be denied to most researchers.

The final essay in the volume, by Olufemi Vaughan, provides a fascinating discussion of the roles of traditional leaders in Nigerian politics and their relationships with the military regimes. He is correct to stress "the remarkable adaptability and resilience of chieftaincy institutions" (p. 430), a feature often missing from accounts of Nigerian politics.

Several essays discuss the proposal whereby the presidency will rotate among political representatives from six designated ethnoregional zones over thirty years, with each zone entitled to one five-year term. Chudi Uwazurike presents this, not uncritically, as an attempt at a consociational solution to Nigeria's cleavages. This reviewer remains entirely unconvinced. First, if the level of trust and long-term confidence among ethnoregional blocs required by such a scheme already existed, then it would be unnecessary to have such a scheme. It is difficult to imagine that a bloc would be satisfied with a constitutional "turn" 25 years in the future. To date, six years is the longest that any civilian regime has survived. Second, if, as many contributors to this volume believe, Sani Abacha plans to use the current transition, scheduled (at present!) to conclude in October 1998, as a mechanism to install himself in power as an "elected" national leader, then any confidence in long-term power sharing is bound to be a bit thin. Finally, Nigerian history provides many examples of the ability of political leaders from one ethnoregional bloc to adopt unrepresentative clients from another in order to provide some camouflage for their dominance. In the First Republic, the Northern People's Congress government coopted the Yoruba politician Samuel Akintola in order to dislodge their main Yoruba antagonist, Obafemi Awolowo. The result was a descent into violent anarchy in the Western Region.

Dilemmas of Democracy in Nigeria is a major contribution to the literature on Nigerian politics. It is essential reading not only for any scholar interested in Nigeria but also for those with wider interests in questions of democratization and military rule.

The Russian Parliamentary Elections of 1995: The Battle for the Duma. By Laura Belin and Robert W. Orttung. Armonk, NY: M. E. Sharpe, 1997. 203p. $62.95.

Russia's 1996 Presidential Election: The End of Polarized Politics. By Michael McFaul. Stanford, CA: Hoover Institution Press, 1997. 169p. $17.95.

Steven L. Solnick, *Columbia University*

The Russian parliamentary elections of 1995 and presidential elections of 1996 were milestones. For the first time, a democratically elected legislature and president stood for reelection in Russia without intervening violence. These two volumes, written quickly after the elections by authors who had closely tracked their progress (Belin and Orttung for the Open Media Research Institute; McFaul for the Carnegie Endowment for International Peace) serve as definitive accounts of each contest. They differ, however, in their relative success at explaining the apparently divergent outcomes of the two races.

Belin and Orttung provide a straightforward and objective account of the 1995 legislative elections, which handed the Communist Party a surprisingly large victory. The volume is well written and clearly organized, beginning with a discussion of the limited power of parliament under Boris Yeltsin's 1993 constitution and an examination of the fight over the 1995 electoral law. The authors then review the major parties that featured in the 1995 contest, the issues that dominated the campaign, and the role of the media. The final three chapters review the results of the vote. Belin and Orttung's discussion of the failure of Russian parties to consolidate in advance of the vote should add new fuel to ongoing debates about the interaction of electoral rules and party development. The Russian electoral system was explicitly designed to encourage smaller parties to merge into stable electoral coalitions. The Duma employed a hybrid system of representation in its inaugural elections in 1993: Half the 450 deputies were elected in single-round first-past-the-post races in single-mandate territorial districts, while the remainder were elected through a national-list system of proportional representation, with a threshold of 5%. In 1993, eight of thirteen parties competing in the national-list vote won seats in the Duma, while many of the deputies elected in single-mandate districts eschewed party identification altogether. The authors provide a useful summary of the complex struggle over writing an electoral law to govern the 1995 contest, as

Yeltsin's government sought to limit the number of seats filled through party-list voting. Unfortunately, the authors restrict this discussion to the arguments advanced by the Russian protagonists themselves, ignoring the extensive literature on the effects of different electoral rules. Indeed, they seem to dismiss much of this work by suggesting that "theories . . . can never accurately forecast human behavior during the campaign" (p. 10). A more nuanced consideration of comparative and theoretical work on electoral systems could have helped readers place the Russian debate in some broader context.

More significantly, the authors miss an opportunity to analyze fully one of the great puzzles of the 1995 Russian vote: Despite rules that should have fostered party consolidation, there were 43 separate electoral blocs, of which only four parties cleared the threshold of 5%. Half the electorate "wasted" its vote on parties shut out of the Duma. Belin and Orttung blame the self-destructive fragmentation on the shortsighted ambitions of party leaders, but some consideration of other cases (such as Poland, where minuscule "sofa" parties ultimately joined to form larger coalitions) would have been welcome. Yet, their narrative should at least permit future comparative studies to move beyond the largely superficial treatment afforded the Russian case in such recent works as Gary Cox's *Making Votes Count* (1997).

Despite their success in characterizing the dynamics of the 1995 campaign, Belin and Orttung occasionally resort to frustrating oversimplification. For instance, they claim that the upper house, the Federation Council, "had no choice" but to accept the compromise electoral law once its leadership revealed that the Duma would override any veto (p. 29). Yet, the inevitability of a veto alone does not explain the senators' reversal; it must be understood in the context of a Russian legislative system so anemic that both chambers must constantly struggle against appearing irrelevant. Similarly, the authors' brief discussion of why politicians fought for places on the twelve-person "federal" party list, rather than regional lists, neglects to mention why the distinction was significant. In fact, only the individuals on the federal list were assured seats in the Duma if their party crossed the five-percent threshold; the electoral law assigned deputies from regional lists according to the regional performance of their party in the vote.

The most disappointing aspect of this useful volume is the low profile of the Russian voter. Belin and Orttung focus primarily on the campaign activities of party leaders and the platforms and advertisements crafted by them. This focus suggests that party leaders were too short-sighted or venal to see the electoral advantage in consolidating in advance of the vote. The authors do not provide a basis for readers to judge why strategic voting on the part of the electorate seemed so thoroughly absent or ineffective. They devote little attention to the differences in the actual constituencies of the different parties on election day, as opposed to their targeted constituencies during the campaign. One chapter, written by Ralph S. Clem and Peter R. Craumer, considers regional dimensions of the vote in some detail, but even here the discussion is limited to the manipulation of aggregate data. Thus, it is stated that the government did better "in provinces where the population was younger" (p. 138), but any attempt to argue that younger voters supported the government would suffer from obvious problems of ecological inference.

By contrast, McFaul's crisply written and concise analysis of the 1996 presidential elections moves the Russian voter much closer to the center of the analysis. By incorporating polling and survey data, as well as unpublished documents from Yeltsin's campaign showing target vote levels by region, McFaul allows the reader to appreciate fully the strategy that produced Yeltsin's reelection. Yeltsin's campaign initially targeted nationalist and Communist voters directly, with disastrous results. McFaul argues that Yeltsin's revamped campaign team realized that the only chance at victory was to exploit the inherent polarization of the electorate into pro- and antireform blocs and to present Yeltsin as the only credible proreform candidate. Once his non-Communist challengers were marginalized, Yeltsin could present himself as a lesser evil than the Communist Gennadii Zyuganov.

The crux of Yeltsin's plan relied on convincing anti-Communist voters that no other candidate could effectively challenge Zyuganov. In other words, though half the Russian electorate wasted its vote in December 1995, McFaul maintains that Yeltsin's victory rested on a gamble that voters would act strategically in 1996. Why did this gamble pay off? McFaul argues, in part, that the rules of the 1995 parliamentary vote encouraged party proliferation, while the 1996 presidential race encouraged polarization, but the account of the 1995 race presented by Belin and Orttung lists features explicitly designed to encourage party consolidation. More convincing is McFaul's assertion that Yeltsin simply ran a better funded and more sophisticated campaign than his non-Communist rivals. If we accept this reasoning, however, then Yeltsin's narrow first-round lead appears to have been the result of sincere rather than strategic voting.

Any verdict on the significance of strategic voting in 1996 is complicated by ambiguities in McFaul's own account of the polarization of the electorate. On the one hand, McFaul argues the electorate was cleaved into "those for the status quo and those against it" (p. 13). In such an environment, he claims, "potential 'third way' candidates had no serious chance" (p. 13), and, consequently, Yeltsin's first task was to become "the only viable candidate from the reformist camp" (p. 27). On the other hand, McFaul suggests that a pivotal 20% of the electorate could be considered "centrist" (p. 27), and he cites Zyuganov's own advisors lamenting their failure to win "new voters beyond the hard-core opposition" (p. 66). McFaul makes it clear that the logic of polarization was the key to surviving the first round, while capturing the centrists was the key to winning the second round, but his analysis would have been strengthened by a more nuanced view of the electoral bifurcation he traces back to 1991. In fact, the consistent presence of a centrist "swamp" vote throughout this period could have opened the door for a third-way candidacy, had the potential flag-bearers not been so weak and had the Communists not intensified the polarization through their own ideological inflexibility.

The real strength of McFaul's volume is his definitive account of the major campaigns. The inside stories of the Yeltsin and Zyuganov operations are dramatically told, with extensive reliance on interviews, campaign documents, polling data, and news reports. McFaul argues convincingly that the conduct of the campaigns was decisive, and that a more adept effort by Zyuganov, or a less skillful job by Yeltsin's team, could have yielded a different outcome. This is in striking contrast to the conclusion presented by Belin and Orttung that the parties' campaigns had only marginal influence on the 1995 outcome.

In his final chapter, McFaul argues that the Russian system is increasingly biased in favor of concentrated economic interests at the expense of diffuse societal interests. While this is an important point, the chapter seems divorced from the election analysis that precedes it. The role of business interests in ensuring Yeltsin's reelection—and the windfalls they reaped in its wake—are considered here, but all too briefly. Curiously, the volume concludes with extensive ap-

pendices of regional voting data that are barely mentioned in the text itself. In addition, several of the tables lack sufficient description of the data to be of much use (appendices 5 and 6, for instance, provide figures for Yeltsin's and Zyuganov's "electorates" in 1993 and 1995 without indicating which parties were grouped together for 1995 or even which national election from 1993 is presented). Despite these lapses, McFaul succeeds in combining a clear analysis of the strategic logic of the 1996 election with an effective evocation of its behind-the-scenes drama and intrigue in a slim and affordable volume.

Theft of an Idol: Text and Context in the Representation of Collective Violence. By Paul R. Brass. Princeton, NJ: Princeton University Press, 1997. 317p. $55.00 cloth, $17.95 paper.

Sumit Ganguly, *Hunter College of the City University of New York*

Paul Brass, one of this country's leading political scientists working on India, has written a fascinating, frustrating, and occasionally polemical book on the politics of collective violence in northern India. This work is fascinating in its detailed discussion of a number of remote incidents. In his descriptions and analyses of these events, Brass displays his substantial knowledge of the politics of caste and community in some of India's most violence-prone regions. Simultaneously, however, the book is quite frustrating, for reasons related to its analytical perspective.

All of Brass's previously published work was within the canons of empirical social science. In this book, however, Brass adopts a postmodernist perspective in his attempts to explain varieties of collective violence in India. For any social scientist trained in the tradition of empirical social science, this work poses problems. Brass provides a number of distinct accounts of a single event. In each account he attempts to show how the particular vantage point of an observer leads to the description and etiology of the eruption of collective violence. It is beyond the scope of a brief review to summarize the rich and thick description that Brass provides of instances of ethnoreligious collective violence. Instead, one such episode, dealing with a putative mass rape at a village in eastern Uttar Pradesh, shows how the national government of Prime Minister Indira Gandhi sought to exploit an allegation of police repression against poor and low-caste villagers to undermine and dismiss the elected U.P. government. Brass also shows that sections of the Indian English-language press were complicit in uncritically accepting the preferred account of the national government in New Delhi. Consequently, these "privileged" accounts, to use a favored postmodernist term, obfuscated other, more compelling reasons for the national government's seeming interest in and concern for the plight of the downtrodden. Instead of a genuine concern about police misbehavior and about the failure of the state government to enforce its writ throughout Uttar Pradesh, the centralizing regime of Indira Gandhi simply sought a pretext to dismiss a government of another party. Ironically, as Brass's deft attempt to reconstruct the events that led to the dismissal of the government shows, it is not entirely clear why a postmodern analysis of the events generates any particular insights or propositions into the workings of India's political system under Indira Gandhi.

What particular advantages, then, accrue from the adoption of this method of analysis? Brass attempts to answer this question with the standard postmodernist response: His multiple narratives underscore the contingency of various explanations for a particular event. There is little doubt in his mind that certain events involving police brutality, state complicity, and ethnoreligious violence did occur. Yet, various narrations of the particular episode reveal the interests and power positions of those narrators. Here the postmodernist approach challenges empirical social scientists who believe that causal logic and the careful collection of evidence can enable us to apprehend the causes of a specific incident. Instead Brass argues that the "truth" of a particular event may never be discovered. All one can excavate from the detritus of communal riots and outbreaks are various versions of the "truth."

His postmodernist commitment notwithstanding, Brass demonstrates an extraordinarily fine-grained knowledge of the politics of significant portions of Uttar Pradesh and Bihar, two of India's poorest and most violent states. That said, there are some problems with the inferences that he draws from his careful study of the cases of ethnoreligious violence. His use of the term "north India" is quite deceptive. The patterns of ethnoreligious violence vary considerably throughout this sprawling region, which includes states as diverse as Jammu and Kashmir and West Bengal. In West Bengal, for instance, a Communist government has successfully contained ethnoreligious violence. In Kashmir, India's only Muslim-majority state, however, the blatant interference of the national government in the internal affairs of the state has contributed to a sanguinary, ethnoreligious, and secessionist insurgency.

Brass's propensity to make wide generalizations from a limited array of cases is more evident when he discusses the shortcomings of India's various political and social institutions. This tendency is most evident in his discussion of the role of the elite English-language press during Indira Gandhi's tenure. Brass describes the press at one point as "Goebbelsian." There is little question that significant sections of the Indian press supinely submitted to Indira Gandhi's henchmen during the "state of emergency" in the late 1970s. Yet, many editors actually courted arrest and were incarcerated for refusing to comply with the dictates of government censors. In the aftermath of that period Indian journalists have routinely exposed corruption in the highest echelons of government, revealed cruel labor practices, and focused attention on the plight of India's disenfranchised.

Brass is on far more secure footing when he develops the concept of an "institutionalized riot system." He argues that certain cities in northern India are far more prone to ethnoreligious violence owing to these sociopolitical configurations. Briefly, these riot systems involve groups of well-heeled political activists, whom Brass calls "riot specialists," who often mobilize the poor and dispossessed to organize and foment riots. In one of the more illuminating, if ultimately inconclusive, chapters of the book, he provides a fascinating account of the career and death of one such riot specialist, Kala Bachcha, in the aftermath of the brutal riots that convulsed the city of Kanpur in Uttar Pradesh. The accounts of Bachcha's exploits during the riots vary considerably. Nevertheless, Brass's careful reconstruction of the events that led to Bachcha's demise provides a compelling portrait of communal relations and violence in one mid-sized Indian city that has undergone substantial industrial decline and concomitant social change.

The utility of these concepts notwithstanding, the reader is still left with a nagging question that these postmodernist accounts fail to answer: What larger material and social forces provide conditions conducive to the emergence of "institutional riot systems" in distinct parts of India?

Political Parties and Democracy in Portugal: Organizations, Elections, and Public Opinion. Edited by Thomas C. Bruneau. Boulder, CO: Westview, 1997. 145p. $55.00.

European Portugal: The Difficult Road to Sustainable Democracy. By José M. Magone. London: Macmillan, 1997; New York: St. Martin's, 1997. 217p. $59.95.

Howard J. Wiarda, *University of Massachusetts*

Portugal in 1974 was the first country in the so-called third wave of late-twentieth-century democratizations. The "revolution of flowers" in April 1974 was followed shortly by the Greek and Spanish democratizations, then by a wave in Latin America, then political openings and greater pluralism in East Asia, next in Eastern Europe and the former Soviet Union, and finally, though weakly, in Africa and some Islamic countries. So it is appropriate, almost a quarter of a century later, to assess both the process and the outcome of the initial "transition to democracy."

There is a teleological flavor to much of the literature on democratic transitions. After the fact, the transitions always look much simpler, easier, and inevitable than they do at the time, as if the outcome were foreordained. The so-called transitologists have informed us of the strategies to be used, the pacts to be signed, and the processes and markers by which the transition becomes the *consolidation* of democracy. The literature seems too often dominated by true believers, the formula too pat, the outcomes too unilinear.

These two books on Portugal remind us of how uncertain and open-ended the process often is, even in the most successful of the recent cases. Democratization was by no means the only possible outcome in Portugal; a right-wing coup, a communist takeover, continued military rule, complete breakdown and disintegration were also conceivable. There was nothing inevitable, unilinear, or foreordained about the process. Strong leadership, political culture changes and continuities, foreign involvement and manipulation, luck, timing, and fortuna—none of which are dealt with adequately in the theoretical literature on transitions—were all important variables in the Portuguese case, from which few general rules and certainly no formula for successful transitions emerge. And perhaps we have learned by now, or should have, that the transitions away from Marxism-Leninism in Eastern Europe and the former Soviet Union are so fundamentally different from the transitions away from authoritarianism in southern Europe and Latin America that the same models and formulas cannot be indiscriminately used for both.

Both author José M. Magone, in chapter 2, and editor Thomas C. Bruneau, in his Introduction, walk us through the steps and stages of the Portuguese transition. They show us how uncertain and ambiguous the process was and how fragile the democratic outcome. The Armed Forces Movement (MFA), which staged the initial coup that ousted the long-term, authoritarian-corporatist regime of Antonio Salazar and his successor, Marcello Caetano, was not at first committed to democracy. The well-organized Portuguese Communist Party (PCP) sought to hijack the revolution, as did General Antonio Spínola. U.S. Ambassador Frank Carlucci and a number of Western European countries and parties sought to aid the democratic forces, while the Soviets and their allies supported the PCP. Portugal became a Cold War battlefield on top of the confusion, uncertainty, disintegration, and street politics reigning in the domestic situation.

Magone traces these events factually while also imposing his own teleological vision of "participatory" or "sustainable" democracy on them. Bruneau is also a committed advocate; his focus is political parties. Bruneau wants to make the case that political parties have become *the* central actors in Portuguese politics since 1975, and his Introduction places heavy stress on their role in the mid-1970s' maneuvering for advantage and in the gradual consolidation that follows. He argues that political parties in Portugal "remain vital" and "play the central role in linking the population to government." He concludes that they are the "key political institutions in both the transition and current political process." At a broader theoretical level, Bruneau believes that, unlike much of Western Europe, parties and the party system have become more important in Portugal.

In arguing his thesis, Bruneau leaves too many questions unanswered. What is the nature of Portugal's political parties? How well organized are they? Are they mass based or more narrowly clientelistic? Are they mainly concessions to foreign (U.S., Western Europe) fads and the requirements of international assistance money, or have they really sunk roots in the Portuguese soil? Are they merely updated versions of Portuguese *personalismo*, *caciquismo*, and rival "baronies," or are they genuine political parties? The poll data available suggest that all parties have low repute, among the lowest of all Portuguese institutions. In his eagerness to give the parties center stage and elevate them to an importance they may not have, Bruneau neglects these crucial issues.

Strikingly, the contributors to this volume are much more forthright on these more complex issues than is the editor. What Bruneau has done—and what constitutes the main value of this book—is ask three recent Ph.D.s, who wrote dissertations on the three main Portuguese political parties, to summarize their research findings. They have done this admirably in three striking substantive chapters that go considerably beyond the editor's overly pat conclusions. Carlos Cunha writes on the PCP, Juliet Antunes Sablosky on the Socialist Party (PS), and Maritheresa Frain on both the Social Democratic Party (PSD, topic of her doctoral thesis) and the Social Democratic Center (CDS, the Christian democrats). A final chapter is by Mário Bacalhao, who summarizes and updates his earlier, well-known explorations of public opinion on political parties as well as election results since 1975.

Cunha summarizes a wealth of research he has done in Lisbon, Paris, Prague, and Moscow on the PCP. In this rich chapter, he shows the overwhelming and personalistic dominance of PCP leader Alvaro Cunhal on the party for the last four decades, Cunhal's insistence on following a Stalinist course even after that had become outdated and discredited, and how that stance has hurt the party electorally in recent years. Antunes Sablosky's chapter on the PS reveals how the party was created in exile by a small elite, how dependent it was on outside advice and financial assistance (British, German, Scandinavian, and U.S. sympathizers) in its early years, and the party's deep internal divisions, the jockeying for power by different factions and individuals, and its actual "thinness" in contrast to its popular image of strength. Similarly, Frain demonstrates the weak institutional base of the PSD, the veto power of its several "barons," and its continuing clientelistic and patrimonialist features. Individually, these are rich substantive chapters, and together they present a picture of the main parties as weak and disorganized, with many striking continuities from Portugal's nondemocratic past, and quite at variance with the editor's strong but perhaps somewhat wishful conclusions.

That is also the viewpoint of Margone's refreshingly insightful book. Like the younger scholars brought together by Bruneau, Magone seems not to be distracted by the conceptual and methodological disputes that sometimes sidetracked an earlier generation of Portugal, and democratization,

scholars. To be sure, he has his own point of view, a rather romantic and idealistic vision of participatory or what he calls "sustainable" democracy. He has read all the transitology literature and cites it, but he seems not to be overly impressed and, thankfully, often goes off in his own uninhibited directions.

Magone argues that the two stages of democratization, transition and consolidation, are insufficient. Instead, he insists democracy must be thoroughly *institutionalized*, that it be *qualitative*, that it reach down deeply into society to encompass basic attitudes, interpersonal and social relations, and political culture as well as institutions. Magone's other main contribution is to emphasize how much democratization and Europeanization were interrelated in the Portuguese case. By "Europeanization" he means both economic and political transformations, and he argues that the transition to democracy was also linked to the development of a more liberal, open-market economy. He emphasizes that Portuguese elites *had* to conform to international pressures both for democratization and for economic liberalization as a condition of joining the EU, which makes Portuguese democracy something of a concession to foreign pressure.

Magone has additional chapters on political parties, the local political system, interest groups, civil society, and public policy. In all these chapters, including the one on political parties, the author is less sanguine about Portuguese democracy than is Bruneau. He emphasizes that Portuguese democracy is still incomplete, often woefully so, that the outcome of the transition is still not certain, that the closer one examines the Portuguese system *below* the level of formal institutions, the more problematic it becomes. While Magone clearly has his preferences, there is no teleological outcome here, no model that has to be verified regardless of where the facts lead.

In the process Magone stresses that the Portuguese state has been restored as the main "steering factor" in development; the presidency is the only stable institution; new (but not necessarily "neo") forms of corporatism are emerging; *all* institutions (parties, interest groups, civil society) remain fragile and underinstitutionalized. He emphasizes that Portuguese institutions are still often dominated by clientelism, elitism, and patrimonialism, and hence the continuities with the past may be as striking as the changes since 1974.

Recently, some transitologists have been making a distinction between electoral democracy (by now, widespread) and liberal democracy (still quite rare). The latter may take generations to develop and requires fundamental changes in political culture as well as in institutions. By Magone's analysis, all this is still incomplete in Portugal; in his view, and those of the younger contributors to the Bruneau volume, Portugal is still a "half-way house," capitalist and patrimonialist at the same time, democratic as well as elitist, only part way along the road to genuine popular participation and equality.

Manufacturing Insecurity: The Rise and Fall of Brazil's Military-Industrial Complex. By Ken Conca. Boulder, CO, and London: Lynne Rienner, 1997. 281p. $55.00.

Wendy Hunter, *Vanderbilt University*

Between 1970 and 1985, Brazil became a Third World leader in military research and development and a major producer and exporter of armaments. By the late 1980s, the defense sector was but a shadow of its former self. In *Manufacturing Insecurity*, Ken Conca explores the domestic and international conditions that initially promoted and later constrained the growth and development of military industrialization in Brazil.

In the first part of the book, Conca describes and analyzes the confluence of domestic-political and global-market forces that directed and helped sustain resources to the country's defense sector in its take-off phase, effectively insulating it from other economic demands domestically as well as global competition. Inspired by visions of technological autonomy and great-power status, Brazil's military governments (1964–85) took advantage of their ability to control crucial political and economic resources to boost military industrialization, especially after 1968. They diverted resources preferentially to the defense sector, instituted export policies supportive of it, and guaranteed a domestic market for arms when necessary. These state policies coincided with and conformed to favorable conditions in the global arms economy, including a strong demand for medium-tech arms and easily available international financing. In the boom phase of military industrialization, commercial considerations, strategic goals, and technological initiatives dovetailed nicely.

As Conca explains in Part Two of the book, Brazil's defense sector fell in the second half of the 1980s as dramatically as it rose a decade earlier. Its fall was due to the decline of the same forces that had facilitated growth. The global market for the kinds of weapons systems sold by Brazil contracted markedly when four important customers—Iraq, Saudi Arabia, Libya, and Egypt—cut their importation of arms by more than 60%. No significant new buyers emerged. At the same time, Brazil's debt crisis and weakened position with global lending agencies, such as the International Monetary Fund, hurt new investment and export-led growth in the defense sector. Foreign financing had been largely responsible for technological advances in the sector in the early 1980s. To worsen matters, the military's ability to influence the economic policies necessary to maintain and foster military industrialization (e.g., keeping defense expenditures at high levels) diminished with the transition to civilian rule in 1985.

In explaining how democratization worked against the continued growth of the arms industry, Conca makes an interesting observation. The insularity of defense production, intended by the military to minimize outside interference, cut both ways. That the sector was shielded from broader forces in society, the economy, and politics initially worked in favor of its development. Yet, the enclave structure over time precluded the support of actors (such as labor, the scientific community, and politicians) who otherwise might have played a key role in protecting the (later beleaguered) industry from demise. In this respect, Brazil's defense sector does not fully conform to what is implied by the term "military-industrial complex." In many advanced industrial countries, coalitions that have included a broader array of social, political, and economic actors have been vital in defending arms production from economic downturn and adverse global conditions.

Conca illustrates his argument with reference to case studies on four different weapons systems: the AM-X tactical fighter, the VLS satellite launcher and ballistic missile, the Ossrio battle tank, and the nuclear submarine program. These individual cases support his overall interpretation. At the same time, they show that the salience of the international and domestic structures he emphasizes varied across specific programs. For example, the military's political autonomy domestically was particularly important for the growth of the parallel nuclear program. Service autonomy, consistent political support, and international market expansion were key in the case of the armored vehicle and aeronautics industries.

In Part Three, Conca uses the Brazilian experience as a point of departure for speculating about the prospects for military-industrial development in the Third World more broadly. Given that the defense sector in Brazil ran up against severe limitations—due to factors that arms producers in other developing countries could be expected to encounter as well, and despite a context that remained relatively favorable in several other respects—Conca is skeptical about the chances for sustained military-industrial development in most of the Third World. Yet, one factor lacking in Brazil that Conca does not underscore, a significant external security threat, would seem to play a crucial role in bolstering the defense sector in such countries as India, Israel, and South Korea. While unable to answer fully all the questions he raises, Conca provides interesting food for thought and new ideas for comparative investigation in this final chapter.

In sum, *Manufacturing Insecurity* provides an excellent overview of the context surrounding the boom and bust of the Brazilian arms industry between the early 1970s and late 1980s. A model of clarity, organization, and analysis, it makes a significant contribution on an important and understudied topic. At a time when the vast majority of the literature on Latin American militaries emphasizes the changing political role of the institution, Conca's examination of recent changes in the military's ability to affect economic policymaking is refreshing and fills an important gap. The difficulty of carrying out research on military industrialization in Brazil also deserves emphasis. To his credit, the author managed to conduct about 70 interviews with participants associated in some capacity with the industry. It is indicative that he was unable to obtain interviews with officials from Engesa or Avibras, two of the largest and most important firms in the defense sector. *Manufacturing Insecurity* is recommended reading for students of Brazil in general, analysts of Latin American militaries, and those interested in military industrialization in the developed and developing worlds.

Making Votes Count: Strategic Coordination in the World's Electoral Systems. By Gary Cox. Cambridge: Cambridge University Press, 1997. 340p. $59.95 cloth, $18.95 paper.

Melvin J. Hinich, *University of Texas at Austin*

Every serious scholar of political systems should read this book. Gary Cox has superbly organized the theoretical and empirical literature to illuminate the complex interactions between the rules used to select the winners in a political system and the behavior of candidates, parties, and voters. He builds on the hypotheses of Duverger, which are clearly explained in chapter 2. Chapter 3 presents a snapshot of various electoral systems used in the world as of 1992. This chapter and Appendix A are great sources for anyone who wishes to study and compare electoral systems. Chapter 4 investigates strategic voting in single-member districts operating under a variety of single-ballot systems. Chapter 5 investigates multimember districts operating under various proportional representation systems. Chapter 6 investigates single-member districts operating under dual-ballot methods.

Cox is a master when it comes to explaining ideas generated by a logic-based theory. A reader with no training in game theory or decision theory can grasp the essential ideas presented in this book. Similarly, someone with no technical knowledge of statistical methods can follow the arguments based on data. There is no impediment in this book to deter someone who wants to understand what Cox is arguing.

This strength is also the main weakness of the book. It is not clear to me what assumptions are really driving the deductions in the theory. I think that I know what is going on, since I am familiar with the theoretical literature, but the text is not sufficiently rigorous for me to check on details that are important for a rigorous analysis. Cox is a very careful and thoughtful scholar, but I have found in my own work that it is easy to make theoretical slips when striving to make difficult ideas accessible to people who are not used to precise argumentation. I am troubled by the question of how critical the equilibrium results are to the assumption that the voting games in the book have common knowledge. Common knowledge in extensive form games requires that all agents know all the moves that all the players have plus the preferences of all the players. Such an assumption is standard in modern game theory, but I question its plausibility for building a scientific theory of politics. Had Cox decided to illustrate these theoretical issues in the text he would have risked losing many potential readers.

Similarly, the empirical results leave me unconvinced, since the technical details I need to judge a statistical analysis are not provided in the book. Cox has opted for convincing readers who are uncomfortable with modern statistical methods. His results are convincing on the surface, but I have seen a number of statistical claims contradicted when confronted with hard analysis.

The core assumption about voters is that they vote instrumentally. They make some sort of calculation about the probability that their vote will improve the outcome with respect to their preference order. This type of rationality is a very strict form of rational action. All that is needed for rational action is that the decision maker behave in a consistent manner with respect to a utility function which can be measured in principle. The measurement issue rules out the tautology where any set of choices can be rationalized by some idiosyncratic utility function which strongly depends on parameters that cannot even be measured in a thought experiment.

Consider the following rational choice model that I believe will yield the same sorts of results, which Cox discusses. A person votes because he likes participating in the voting ritual. There is no need to invoke Calvinist civic duty terms into the voter's calculus. The satisfaction can depend on the closeness of the race and the choices to be voted on. Thus, the utility increase of participation is not a fixed value. Once the decision to vote is made, then "strategic" calculations only apply if there are more than two choices.

Extending this type of approach when there are more than two choices, suppose that a voter is deciding which party to vote for in an election for parliament in an electoral system with proportional representation. The voter can vote for her most preferred party regardless of whether the party has a chance to get into the parliament. But if the voter likes voting for a party which gets into the ruling cabinet as a result of coalition formation after the election, this voter will consider the likelihood that each of her top choices will be in the cabinet. This type of decision rule is not instrumental; it is subjective and is rational given the definition of rationality I have made here.

The type of rationality Cox uses is odd given the way large democracies are governed. It is not clear how major national decisions made in the District of Columbia by the various elected agents, appointed bureaucrats, and judges are driven by the outcomes of national and regional elections. Gaining utility from participation given some expectations of who will win makes more sense to me than imposing a strategic calculus on voters that is the same as is used for the active agents—the politicians and parties.

These active players in the political game get constant feedback from their moves and can learn, but a voter who actually makes or breaks a tie cannot learn that she did it. Only when voters are organized into voting blocs in which the leadership can observe voter shirking can mass politics be viewed as a coordination game using conventional game theoretic constructions.

Yet, taken as a whole, this book is a very important contribution to our knowledge about electoral systems. It will be the major book in this area for some time to come.

The State against Society: Political Crises and Their Aftermath in East Central Europe. By Grzegorz Ekiert. Princeton, NJ: Princeton University Press, 1996. 435p. $59.50 cloth, $19.95 paper.

Michael Bernhard, *Pennsylvania State University*

This is one of those rare works that combines a rich comparative historical understanding with a compelling theoretical framework. Ekiert pinpoints several critical junctures in the institutional evolution (1956–89) of East Central European Communist regimes following the death of Stalin. In this sense the book shares the ambitious theoretical agenda posed by Collier and Collier's *Shaping the Political Arena* (1991) for Latin America and Luebbert's *Liberalism, Fascism, or Social Democracy* (1991) for interwar Western Europe.

The three cases used to illustrate these critical junctures are the Hungarian Revolution, the Prague Spring, and the Solidarity period in Poland. It is not the crises per se but their aftermaths that are most significant in the shaping of new institutions. Thus, the dependent variable is the result of the postcrisis demobilization. Ekiert not only explains whether the party-state can secure "reequilibration" (in the sense that Linz coined this term in his work on the breakdown of democracies) but also the nature of the new equilibria. In the Polish case (1980–89) the authorities failed to reestablish equilibrium, and this was the opening that led to the democratization of the region.

In Hungary (1956–63) and Czechoslovakia (1968–76) the party state successfully restored order. Still, the nature of these new equilibria are not what one would expect, given the crises from which they emerged. The rapid uncontrolled mobilization of the Hungarian Revolution and its violent suppression by the Soviet army led to pragmatic and flexible reformist policies after an initial period of strong repression, whereas the much more limited and controlled mobilization of the Prague Spring and the considerably less violent occupation of the country by Warsaw Pact forces led to highly rigid and repressive policies. The Hungarian case was a critical juncture because it set the limits of de-Stalinization. In turn, the Czechoslovak case redefined those limits by curtailing revisionist attempts at reform. This established the institutional conservatism in the bloc associated with Brezhnev's rule in the USSR.

Ekiert comparatively analyzes each case in terms of three independent variables: the capacity of resistance or opposition movements that emerged during the crisis, the capacity and strategies of the party-state, and the international context of the demobilization period. Through the carefully controlled comparison of these variables across the cases, Ekiert provides a convincing explanation for the outcome of all three. It is the sophisticated use of the comparative method across three empirically rich case studies that is the strength of this study as a work of political science. Ekiert further demonstrates the utility of his findings by showing how the variations in outcomes were significant for the patterns of the transition to democracy in all three cases.

The book also makes significant contributions to a revised understanding of East European communist systems. The most important point is that not the actual crises but "the stage of demobilization and reequilibration... left the most enduring consequences" for institutional evolution (p. 26). Furthermore, Ekiert's criticism of the conceptualization of postcrisis behavior in communist regimes as "normalization" (defined as a return to the status quo ante) and his use of "reequilibration" are strong improvements both in terms of empirical accuracy and the ability of his theory to account for change.

Ekiert's book also reiterates and provides strong evidence for a number of new understandings which have emerged over the last several years (particularly in the work of younger scholars and those who conducted fieldwork in Eastern Europe in the 1980s). While these ideas cannot be uniquely attributed to Ekiert, he is an eloquent spokesman for many of them. First, his book is yet another confirmation that East European communist regimes were far more fragile than theories of totalitarianism and posttotalitarianism led us to believe. Second, it explains East European politics as a product of the interaction of state and society, which in terms of understanding developments in the 1970s and the 1980s in many of the countries in the region (most notably Poland and Hungary), is an improvement over the more conventional Sovietological approaches that view East European politics as a product of Soviet policies, the overwhelming power of the prerogative state over a subject society, or the primacy of elites. Furthermore, Ekiert provides ample evidence for the related assertion that this state-society relationship was the major source of variation in the domestic politics of the countries of the region. Third and finally, this state-society approach highlights the importance of the domestic politics of individual countries despite political and economic dependence on the USSR. Ekiert's analysis strongly refutes the notion, often encountered in works by analysts with a passing knowledge of Eastern Europe, that democratization is only explainable with reference to developments in the USSR. What analyses such as Ekiert's point out is the significance of events in Eastern Europe for the bloc as a whole, including internal reform in the USSR. In this regard, the failure to reestablish reequilibrium in Poland was critical not only for that country but also for the demise of the Soviet bloc as a whole.

Despite many obvious strengths, the work is open to some minor criticism. For instance, the Hungarian account could have been improved by greater detail on the contours of the New Course policies introduced by Nagy and the successful efforts of the Stalinists to undermine them. In addition, Ekiert underestimates the significance of the changes introduced during the Prague Spring on a systemic level, arguing that "the reformist institutional vision... did not depart radically from the major tenets of the Marist-Leninist state-idea" (p. 140). Here I differ with him and see the abolition of censorship, the introduction of the secret ballot in internal party elections, and a degree of economic decentralization as major departures in the direction of a different model of socialism. We will never know if it was a viable one due to the actions of the Warsaw Pact.

On a theoretical level I have only one quibble. Ekiert describes his analysis with three different results, I am at a loss to understand how these represent paths and not just individual developmental trajectories. Even if this is the case, Ekiert's analysis represents an insightful historicism, and this in no way undermines its many significant findings.

The book deserves a wide audience. Its exemplary use of comparison makes it significant for all those interested in

small-n qualitative methods. East Europeanists will appreciate Ekiert's attention to detail, his careful interpretations of the cases, and how he builds to a new understanding of the significance of key events in the postwar history of the region. It is also the sort of book that the many political scientists interested in using Eastern European data to test their theories of democratization, party-formation, voting, and so on, should read to develop a keener understanding of the politics of the region.

The Catholic Church and Democracy in Chile and Peru. By Michael Fleet and Brian H. Smith. Notre Dame, IN: University of Notre Dame Press, 1997. 378p. $45.00.

Contradiction and Conflict: The Popular Church in Nicaragua. By Debra Sabia. Tuscaloosa: University of Alabama Press, 1997. 256p. $34.95.

Philip J. Williams, *University of Florida*

These two books add to the growing literature on religion and politics in Latin America. Fleet and Smith's comparative study of the role of the Catholic church in the process of democratic transition and consolidation in Chile and Peru examines both the efforts of national church leaders and local clergy and activists. Sabia's work traces the emergence and evolution of the popular sector of the Catholic church in Nicaragua, focusing on the political and religious differences that fragmented the popular church after 1979. The two books represent distinct approaches to studying the church and politics in Latin America. Whereas Fleet and Smith adopt a more institutional perspective for understanding the church's evolving role, Sabia views the church as a "community of faithful" and focuses primarily on the grassroots level.

Fleet and Smith provide a fairly comprehensive treatment. Their theoretical approach, while not particularly novel, considers the effect of both religious and secular forces on the church's institutional development in each country. These include the church's traditional core concerns; the dominance of the institutional church model; secular challenges; and new political roles. Chapters 2 and 3 discuss the historical evolution of the church in Chile and Peru, testing a series of hypotheses based on the theoretical framework. Chapters 4 through 7 examine the church's role in the transition and consolidation phases in both countries. This is followed by a comparative analysis of the two cases and a brief discussion of possible future scenarios.

The strength of the study is its examination of the role of national church leaders in the periods of democratic transition and consolidation. In the Chilean case, Fleet and Smith provide convincing evidence that church leaders played a vital role in moving the transition forward. The Archbishop of Santiago, Cardinal Fresno, was especially instrumental in creating opportunities for dialogue between opposition leaders and the Pinochet regime. According to the authors, "the hierarchy was most influential when it was least prophetic, when it worked behind the scenes on behalf of compromise instead of denouncing evils and abuses publicly" (p. 272). Local Catholic activists also played important roles in the transition, participating in a range of social movement activities while exerting a moderating influence on the opposition movement. During the consolidation phase, the church's influence has been "modest and at times obstructive" (p. 274). The hierarchy's inflexible position on moral issues, such as divorce and abortion, has done little to cultivate democratic debate on issues of national import, while local activists have provided only "residual support for consolidation" (p. 193).

In Peru, the authors argue, the church had much less influence on the democratic transition. This had to do in part with the very different nature of the military regime in Peru, which was much less repressive than its Chilean counterpart. During its initial phase, the Velasco regime implemented a number of significant social and economic reforms intended to benefit lower-class Peruvians, some of which were supported by church officials. Moreover, divisions with the hierarchy limited to church's ability to speak with a unified voice. Likewise, local Catholic activists, while participating in opposition movements, were more fragmented along political lines than in Chile. During the consolidation phase, the church's role was even more limited. The bishops did little to contribute to the strengthening of democratic values and processes, and local lay activists received little encouragement from the hierarchy. As Fleet and Smith point out, the church's ability to contribute positively to the consolidation phase in Peru has been complicated by the political violence of Sendero Luminoso, the collapse of traditional political parties, and successive economic crises.

Although the study makes an important contribution to our understanding of the institutional development of both churches and their role in recent political transformations, the authors do not do justice to processes within the churches at the local level. The data from surveys of Catholics in Santiago and Lima are limited in their usefulness. In the Chilean case, the survey was conducted during the transition phase, but the authors provide no comparable data for the consolidation period. In the Peruvian case, the data were collected during the consolidation phase, and there is none from the transition period. This makes it difficult to compare the role of local Catholic activists from one period to the next and complicates any cross-national comparisons. Moreover, the data could have been brought to life by providing more detailed discussion of parish-level developments and including more "popular voices" in the analysis. Except for presenting excerpts from interviews with clergy and pastoral agents, the voices of "ordinary heros" are almost entirely absent. As Daniel Levine has made clear (*Popular Voices in Latin American Catholicism*, 1992), popular religious groups can instill in their members the development of new norms, values, and leadership skills that support democratic development.

Sabia's study of the popular church in Nicaragua is a much less ambitious undertaking. Chapters 1–5 cover familiar territory. After first discussing the effect of changes in Latin American Catholicism on the Nicaraguan church, Sabia traces the emergence and evolution of the popular church, including its conflicts and tensions with the Catholic hierarchy. Most of this discussion is based on earlier studies (Michael Dodson and Laura O'Shaughnessy, *Nicaragua's Other Revolution*, 1990; Joseph Mulligan, *The Nicaraguan Church and the Revolution*, 1991; and Philip Williams, *The Catholic Church and Politics in Nicaragua and Costa Rica*, 1989) and adds little new insight. More interesting are chapters 6–9, in which Sabia examines the popular church as "a community of faithful made up of four distinct ideal types" (p. 5): Marxist, revolutionary Christian, reformist Christian, and alienated Christian. The ideal types are derived from interviews by the author with members of three different Christian base communities in Managua. Drawing from her interview data, the author tries to show how political and religious issues contributed to the growing fragmentation of the popular church along the lines suggested by her typology. One of the most interesting insights of the book is the relative weight of revolutionary Christians within the popular church. According to Sabia's findings, reformist Christians, who

exhibit moderate views on both political issues and their relationship with the church hierarchy, clearly outnumber the revolutionaries. Consequently, the overwhelming majority of base community members are much less partisan than once thought and are more likely to exert a moderating influence on the popular sector of the church. These findings are similar to those of Fleet and Smith concerning the political and religious views of local Catholic activists in Chile and Peru.

Two of the ideal types used by Sabia are particularly problematic. Marxists, for example, no longer consider themselves Catholics. Some participate in the social projects of local base communities but are not practicing Catholics. If this is the case, then how can members of this group even be considered part of the popular church? The alienated Christian type is too broad and diffuse to be a useful analytic category. This group includes rich and poor, some who support the archbishop and others who do not, charismatics and recently converted evangelicals, ardent anti-Sandinistas, and those who have withdrawn from politics altogether. What becomes clear in Sabia's discussion of alienated Christians is the growing importance of new religious movements in Nicaragua. Whereas the popular church has been in a long period of decline, the charismatic renewal movement and evangelical Protestantism have experienced a boom in recent years.

The scant attention paid to new religious movements is a weakness of both these studies. As other scholars of religion in Latin America have argued (John Burdick, *Looking for God in Brazil*, 1993; Rowan Ireland, *Kingdoms Come*, 1991), the religious arena has become increasingly contested of late. Even within the Catholic church, an incredible explosion of new religious lay movements has added to the complexity of the church. The challenge for future studies of religion and politics is to make sense of the growing religious diversity of Latin America and its potential political significance for the region.

Ethnic Conflict: Commerce, Culture, and the Contact Hypothesis. By H. D. Forbes. New Haven, CT: Yale University Press, 1997. 304p. $30.00.

Milton J. Esman, *Cornell University*

The academic literature on ethnic conflict addresses two problems: the causes of ethnic conflict and how they can be managed or resolved. This book explores the first of these problems and also aspires to demonstrate the limitations of empiricism in the social sciences. It is intended for professional social scientists, not for concerned laymen or undergraduates. Much of it surveys and evaluates the treatment of ethnic conflict by other scholars, its style is rigorous and astringent, and it contains little empirical information. With its critical insights on much of the literature in several disciplines and its suggestions for expanding the range of current explanations, this book is a significant addition to scholarship and to efforts to deal intellectually with one of the most intractable phenomena in world affairs.

Forbes's main thrust is an attack on "contact theory," the widely held common sense intuition that increased contact among members of different ethnic communities increases mutual understanding by breaking down stereotypes, thereby eliminating the source of ethnic suspicions and conflicts. This view continues to be popular among laymen, public officials, social workers, and social scientists alike. Its many versions range from popular ditties, such as "Getting to Know You," to the conviction that foreign trade reduces conflicts among nations, to the extensive body of scholarship produced during the past half century by social psychologists ascribing ethnic and racial prejudice and discrimination to misperceptions of reality about other people. In the absence of such prejudices, ethnic-based conflicts would presumably disappear. The author then demonstrates that the fatal methodological flaw in this large and influential body of literature lies in confusing individual with collective phenomena. At the individual level and under some circumstances, greater contact may mitigate hostility, but at the aggregative level different dynamics prevail, and hostility stems from real differences of group interest.

This insight gives rise to the main counterhypothesis to contact theory: Far from being psychiatric in origin (false perception), ethnic conflict tends to be a rational response to competing group interests. These competing interests can be economic (land, capital, jobs, business opportunities), political (participation in public affairs, including voting and access to government services, resources, and offices, both civil and military), or cultural (relative status of religion, language, and public festivals). The competing interests hypothesis, with its rationalistic bias, has won broad support among sociologists, political scientists, and, of course, economists.

Forbes then posits a logical flaw in both of these explanations of ethnic conflict: There is nothing distinctively ethnic in either hypothesis. Why, then, should such nonethnic causes give rise to ethnic rather than to other possible expressions of conflict? To resolve this problem, he proposes an alternative explanation inspired by the late Karl Deutsch's treatment of modern nationalism. This "linguistic model" traces ethnic conflict to competing struggles for collective self-esteem among ethnic groups in contact, the impulse of each to prove its superiority and ensure its survival by imposing its way of life (especially its language and culture) on others, one method being the assimilation of outsiders into their community. Thus, the greater the contact, the greater the likelihood of conflict. Contrary to the main tradition in the modern social sciences, Forbes contrives an explicitly ethnic explanation for ethnic conflict. Among the practical, counterintuitive implications of this hypothesis, the author suggests that, "as the world shrinks and contact increases, assimilation will always lag behind the development of [ethnic] conflicts, so that more and more resources will have to be devoted to repression and education" (p. 208). Thus, with globalization, the spread of North American democratic culture may have to be implemented by increased repression.

The author devotes many pages to his convincing, though repetitious, efforts to undermine the contact hypothesis and to demonstrate that its failure can be traced to excesses of empiricism in the social sciences. To this reader, his critique of social science methods is less instructive than his analysis of theories about ethnic conflict. I would have felt rewarded had he extended his inquiry to an evaluation of the social constructionist explanation of ethnic solidarity and ethnic conflict, since this postmodernist approach has gained a wide following, especially among younger scholars. My suspicion is that most adherents to the social constructionist persuasion, with its emphasis on the fluidity and contingency of ethnic solidarities, would find Forbes's explanation, with its assumptions about the robustness of ethnic solidarities, quite unacceptable. To this reviewer, by contrast, the main contribution of this book is the hypothesis that recognizes ethnic solidarity as an independent variable, as an explanation in its own right

of ethnic conflict, rather than ascribing it entirely to extraneous psychological, economic, or political forces.

Russian Civil-Military Relations. By Dale R. Herspring. Bloomington: Indiana University Press, 1997. 272p. $35.00.

Mark Kramer, *Harvard University*

Civil-military relations in newly democratizing states of the Third World and in the former dictatorships of southern Europe, especially in countries that were under military rule, have attracted a good deal of attention among Western scholars. But the role of armed forces in countries that once belonged to the Warsaw Pact have received much less attention. Recent books about the post-Communist transformation of Russia, the main successor-state to the Soviet Union, have barely mentioned the civil-military relationship, much less analyzed it in any detail. The lack of attention to this issue has left a gap in our understanding of the Russian political system and the problems of democratization in Russia.

Fortunately, important groundwork for an assessment of the army's role in Russian society has now been laid by Dale Herspring, who has long been one of the most perceptive Western analysts of civil-military relations in former Communist countries. He is the author of many books and articles about military politics in the Soviet Union, East Germany, and Poland. His latest book builds on his earlier works.

In a brief introduction, Herspring reviews the three main models devised by Western scholars to analyze civil-military relations in the Soviet Union: the conflictual model associated with Roman Kolkowicz, the institutional-congruence model associated with William Odom, and the participatory model elaborated by Timothy Colton. Herspring's earlier publications compare and test these models at some length, and his purpose in this latest book is twofold: reassess their utility for an understanding of Soviet-era military politics, and see whether any of the three schemes (or some other models) are helpful in analyzing civil-military relations in postsoviet Russia.

Following the Introduction, the first eight chapters cover two periods of great upheaval in the Soviet era: the 1920s, when fierce debates emerged about military doctrine and the proper role of the newly created Red Army; and the late 1980s and early 1990s, when the far-reaching changes launched by Mikhail Gorbachev engulfed the army. Most of the issues and events covered in these chapters have been discussed, in one form or another, in Herspring's earlier work and in other recent books by Western and Russian scholars, but here they are concisely summarized to provide a baseline for the preliminary assessment of civil-military relations in postsoviet Russia, which forms the next two chapters. In a concluding chapter, Herspring returns to the three models he discussed at the outset to determine whether they are of any use in understanding those relations and the evolving role of the army.

Herspring's discussion of Soviet-era military politics is lucid and well done, but his decision to focus solely on the 1920s and the Gorbachev era leaves out the most disruptive period of all for the Soviet armed forces (the violent purges of the 1930s) and the years of greatest triumph for the army, in World War II. Although Herspring's analysis of the 1920s reveals illuminating parallels with the issues now confronting Russia's army, newly declassified archival materials have shown that even in the 1950s and 1960s the army was bedeviled by many of the same problems that afflict it today. It would have been worthwhile if Herspring had drawn on declassified documents and the plethora of new memoirs, including books by former high-ranking officials in the Soviet and Russian Ministry of Defense. A vast amount of once-secret material is now available in Moscow and through Western microfilm projects and repositories, such as the Cold War International History Project and the Harvard Project on Cold War Studies. The use of these materials would have further enriched, and perhaps refocused, the book's analysis.

Herspring's brief overview of the transition in 1991–92 from the Soviet Army to the Russian Army and his discussion of civil-military problems between 1992 and mid-1996 is a valuable starting point both for scholars of civil-military relations and for those who want to understand the uneven process of democratization in Russia. It is unfortunate that these two chapters are so compact—just 29 pages in total—because they provide an insightful look at the complex civil-military interactions in Russia. Some key episodes, notably the August 1991 coup attempt, the October 1993 showdown at the Russian parliament building, and the war in Chechnya, are covered in only the barest detail. The effect of these events on civil-military relations was so great that a more extended discussion would have been in order. At the same time, Herspring is justified in dealing at some length with the adverse effect of defense budget cuts, the sharp decline in morale, and the deep rifts that have emerged within the military. These sections provide an admirable summary of complicated events and trends.

Herspring convincingly argues that the three models of Soviet military politics, especially Kolkowicz's and Odom's, are inadequate not only when applied to the Soviet Union, but also and even more, when applied to Russia. He rightly points out that only one of the models (Colton's) was of even limited use in analyzing periods of rapid change. The deficiencies of these approaches lead Herspring to question whether it is really worth trying to come up with a new model for the study of civil-military relations in Russia. He does not deny that, at some point in the future, new models and theoretical concepts could prove useful, but he doubts that meaningful generalizations will be feasible until the political situation in Russia stabilizes and the army "begins to take on more of the characteristics of corporateness and cohesion of the old Soviet military" (p. 190).

Herspring's caution and skepticism are well taken, but his criteria are too stringent. The book's epigraph is a remark by Samuel Huntington that "one measure of a theory is the degree to which it encompasses and explains all the relevant facts." Herspring doubts that any model or theoretical approach can "encompass and explain all the relevant facts" in the turbulent and unstable climate of Russia. The counter to this, however, is that theories can be useful even if they do not explain *all* the relevant facts. Well-constructed theories of civil-military relations can facilitate comparative analyses (both temporally and on a cross-country basis) and can shed light on patterns and events that would otherwise seem random or inexplicable. Careful tests will show whether these theories are in fact useful means of analyzing military politics in Russia and elsewhere.

Despite the brevity of Herspring's book, it provides a cogent and well-informed point of departure for those hoping to understand the role of the army in Russia's post-Communist transformation. It will be especially useful for courses in Russian politics and twentieth-century Russian history.

Post-Marcos Politics: A Geographical and Statistical Analysis of the 1992 Presidential Election. By Carl H. Landé. Singapore: Institute of Southeast Asian Studies; New York: St. Martin's, 1996. 171p. $49.95.

Jeffrey M. Riedinger, *Michigan State University*

Carl Landé offers Southeast Asian scholars another seminal contribution in this quantitative analysis of the 1992 Philippine presidential elections. Combining aggregate data from provincial and city-level electoral returns and agricultural and population censuses, he provides an in-depth geographical and demographic analysis of the election. Landé finds that, for most of the seven candidates, linguistic-regional loyalties were more important in explaining their electoral support than other demographic characteristics, such as socioeconomic status, educational attainment, or religion. The analysis also reveals two splits within the 1992 electorate: between former supporters of presidents Marcos and Aquino and between the political left and the more conservative mainstream. Landé expects only the latter schism to endure. Building on the data analysis, he surveys the development of Philippine party politics before and after the period of authoritarian rule under Ferdinand Marcos. Philippine elections in the post-Marcos era have been typified by a breakdown in the relatively stable two-party system which characterized premartial-law politics. The author concludes that, in the near term, Philippine party politics will be marked by continued instability. Coupled with the checks and balances of the constitutional separation of executive and legislative powers, this instability will result in weak and divided central government.

Landé complements the basic presentation of electoral data with both simple correlational analysis and semipartial correlational analysis. The former measures the degree of covariation between the percentage of vote each candidate received by province and 37 demographic and ecological variables. The latter measures the degree of variance in votes received as explained by the same demographic and ecological variables after controlling for the influence of the other variables. The methodological discussion is readily accessible to readers with limited knowledge of quantitative analysis.

Linguistic differences in the Philippines predate Spanish colonial rule. As used in the home, no single language unites more than one-third of the population. These linguistic differences were the strongest single predictor of voting patterns in the 1992 election. Regional support for native candidates, or for nonnative candidates who enjoyed strong backing from leading regional figures, was also crucial to the electoral outcome.

Despite the importance of linguistic-regional characteristics in the 1992 election and the very poor performance of the most issue-oriented candidate (Jovito Salonga), Landé identifies a series of small steps in the evolution of Philippines politics from voting based on linguistic-regional characteristics to issue-based voting. This evolution reflects the decentralization of government activities under the Local Government Code of 1991 and the resulting increase in the importance of local elections and the independence of local politicians, coupled with urbanization and the increasing educational attainment of the populace. Self-described cause-oriented nongovernmental organizations and a fledgling progressive party hope to build on these dynamics to advance issue-based politics, beginning with local and provincial elections.

Some of the data and findings are consistent with the patron-client explanations of Philippine politics, which Landé helped pioneer in the 1950s. Other data and results are at odds with such analysis. Landé posits that there has been a shift from traditional patron-client politics to machine politics predicated on short-term material rewards. Unfortunately, the patron-client explanation cannot be fully tested with the aggregate data available to Landé. These tests will have to wait for individual voter data, such as would be gathered in exit polls. The aggregate data also do not permit analysis of the influence of characteristics evenly distributed across provinces, notably gender and age. Nor would the analysis reveal the effect of factors equally influential across all provinces.

Conspicuous by its omission is any extended discussion of the influence of electoral rules. The author focuses only on the use of blank (write-in) ballots and the timing of elections. Voters often complete their ballots by relying on sample ballots distributed by local leaders, a practice which Landé argues strengthens subnational politicians in their dealings with national political figures. The write-in ballot also advantages national candidates with public visibility, such as media and entertainment personalities. The author also suggests that the holding of simultaneous local, provincial, and national elections in 1992 had a negative effect on party loyalty, as it undermined reciprocity between local and national candidates. He finds little connection between the electoral success of political parties at the local or provincial level and the relative success of the parties' candidates in the presidential contest. This is unsurprising, given the weak history of party discipline in the Philippines. More striking is Landé's failure to reference the growing literature on the institutional and policy consequences of various electoral systems (e.g., Matthew S. Shugart and John M. Carey, *Presidents and Assemblies: Constitutional Design and Electoral Dynamics*, 1992). Even the debates over the relative strengths and weaknesses of presidential versus parliamentary systems receive modest attention. This is particularly notable given that proposals to switch to a parliamentary form of government have been very actively debated in recent years in the Philippines, as they were shortly before Marcos declared martial law in 1972. These debates have made little reference to comparative experience in other countries or to the scholarly literature. In ignoring this literature, Landé missed an important opportunity to inform these discussions.

On a minor note, Landé relies heavily on map presentations of the electoral data. In following the accompanying narrative, readers unfamiliar with the Philippines would benefit from a map of all the provinces, appropriately labelled.

These shortcomings notwithstanding, Landé's study represents a significant advance in the scholarship on Philippine electoral politics. Although well suited for use in advanced Asian politics courses, it should be complemented by more theoretical texts on electoral institutions.

Capital Ungoverned: Liberalizing Finance in Interventionist States. By Michael Loriaux, Meredith Woo-Cumings, Kent E. Calder, Sylvia Maxfield, and Sofia Perez. Ithaca, NY: Cornell University Press, 1997. 234p. $39.95 cloth, $15.95 paper.

Ethan B. Kapstein, *University of Minnesota*

Now that reregulation of international capital flows is once again an acceptable topic of policy debate, we are led to ask why many states around the world took the decision, during the past two decades, to abandon their interventionist policies of domestic credit allocation. This book provides an excellent place to look for answers. Loriaux and his col-

leagues examine "the end of credit activism" in Japan, South Korea, Mexico, France, and Spain, and in the opening and concluding chapters Loriaux situates these case studies in international perspective.

Inspired by Peter Katzenstein's *Between Power and Plenty* (1978), Loriaux seeks to establish the linkage between systemic events and national policies. Unlike Katzenstein, however, who asked why the 1973 oil crisis produced such different policy responses in the industrial countries, Loriaux seeks to induce a systemic story from the common threads found in each case. Examining the policy shift from interventionist to more market-oriented credit allocation schemes in countries as different as Korea and Spain, Loriaux infers "that the force driving that process . . . must have had roots in the evolution of the broader global political economy" (p. 2).

That proposition, in turn, suggests to Loriaux two possible explanations. The first is the neoliberal study of autonomous international market forces constraining the policymaking capacity of domestic financial institutions. The second is the hegemonic story of a heavily indebted United States seeking to promote "free market" financial policies to serve its own needs for deficit financing. It is this latter possibility of "predatory hegemony" that Loriaux finds most compelling.

Do the country cases support this particular systemic argument? The answer is: not completely. To begin with, the methods adopted here may be suspected of selection bias, so that in the best of circumstances we would have five studies that confirm the Loriaux hypothesis. But this is not the empirical result. Indeed, with the exception of the chapter on South Korea by Woo-Cumings, and to a lesser extent Calder's chapter on Japan, the U.S. government does not figure very prominently here at all. In the cases of Mexico, France, and Spain, other factors, including domestic and regional politics, are of equal if not greater weight in explaining the decision for liberalization.

Furthermore, when we extend the data set, the hegemonic argument must be qualified by the fact that many countries around the world, including Brazil, Chile, China, and India, continued to use capital and credit controls during the period under study (roughly the mid-1970s to mid-1990s) to meet their economic policy objectives. If America was so concerned by its relative decline and so intent on developing international policies that would stem that fall, then it might have been expected to focus its power on its principal systemic and regional challengers.

What, then, do these case studies tell us? Calder's analysis of Japan reveals a country that within one generation moved from capital scarcity to tremendous liquidity. With that liquidity, industries and entrepreneurs were no longer dependent on the state for finance, and they began to engage in independent investment programs. The plenitude of credit, in turn, led to Japan's speculative bubble, which burst at the end of the 1980s, leading to new pressures on the state to reregulate the financial markets. That intervention, as we have seen, has been fitful at best, but it has certainly been associated with liberalization, encouraged by a United States that wanted access to Japanese financial markets.

In her chapter on South Korea, Meredith Woo-Cumings makes the strongest of the hegemonic arguments. She writes that, given South Korea's position as a front-line state in the Cold War, it is "not surprising" that the country's political economy "is critically influenced by events that occur in New York and Washington" (p. 91). She describes in detail how U.S. officials applied pressure on Seoul to open its markets in trade and finance to American firms. Even in this case, however, she admits that Washington has only been partially successful and that the reform process is far from complete. That would seem to be the case with much of East Asia, even following the financial turmoil of 1997–98.

The limits of American hegemony are most powerfully seen in Sylvia Maxfield's study of Mexico. There, despite pressures that must have been no less strong than those faced by Seoul, Mexican officials successfully negotiated financial market opening in a way that satisfied the demands of domestic bankers. The impetus to that opening was driven in the first instance by the debt crisis which began in 1982, rather than by any particular directives coming from Washington, and over the following decade Mexico's first priority was to reestablish its credibility with foreign investors. Remarkably, despite a substantial relaxation of barriers to entry, and the negotiation of the North American Free Trade Agreement, U.S. banks have been "disappointed" by the degree of opening achieved (p. 117).

Loriaux traces French financial market liberalization to the 1970s, with the collapse of Bretton Woods and the oil crisis placing severe inflationary pressures on the economy. But this is curious, since he points out that the response of the Giscard d'Estaing government was decidedly interventionist. In fact, it was not until Mitterand's failed experiment with financial sector nationalization that the Socialists reversed the interventionist course and began to liberalize. This, however, was as much a consequence of European political economy, and especially the single currency project, as it was of American hegemonic demands for open financial markets (though one might, of course, trace European monetary integration to America's "predatory hegemony").

With respect to Spain, the systemic case is even weaker. Perez does not ignore international factors in promoting Spanish liberalization, but she emphasizes the ongoing domestic debate between political reformers and bankers in advancing this project. Profiting from the inflationary environment, Spanish bankers had little interest in the real economy, and they were generally capable of frustrating most reform measures. It was not until Franco's fall from power that serious efforts at liberalization could proceed, and these were subsequently aided and abetted by European Community membership.

As with Katzenstein's *Between Power and Plenty*, we are thus left to ask: How important are international pressures on nation-states after all? To be sure, we do seem to have a significant degree of policy convergence at work here: Increasing capital liberalization is certainly one of the major recent developments in the international political economy. But whether this can be traced to predatory hegemony is a different story. A contrasting view might focus instead on what Waltz called the "socialization" function of the international system. Observing the success of the Thatcher-Reagan reforms in generating economic growth following a period of stagnation, a growing number of states may have sought to mimic that liberalization strategy to the extent possible within their own domestic political constraints.

Russia's Politics of Uncertainty. By Mary McAuley. Cambridge: Cambridge University Press, 1997. 351p. $64.95 cloth, $24.95 paper.

Alfred B. Evans, Jr., *California State University, Fresno*

McAuley has undertaken an ambitious enterprise, since her study delineates changes in politics that have occurred in six widely separated locations in Russia after the disintegration of the USSR. This is one of the first works by a single author comparing simultaneous trends in several regions of the Russian Federation. The six locations on which she focuses

are the republics of Tatarstan and Sakha (Yakutia), Krasnodar territory, the regions of Perm and Tomsk, and the City of St. Petersburg.

In the early 1990s, with the undermining of Communist Party authority and the breakup of the Soviet Union, a large number of studies described changes at the level of the central government of Russia, while a few writers explored trends in Moscow and St. Petersburg. More recently, a small but growing number of researchers have analyzed changes in authority and policy at the regional level in Russia. McAuley's book must be considered a major addition to that body of scholarly literature. She should be commended for her diligence in traveling widely within Russia, carrying out countless interviews of local officials, and reading a large volume of articles published in the newspapers of various cities. The result is a richly detailed and perceptive study, which demonstrates that many regional leaders have been adept at defending their interests as the authority of the central government weakened and the rules of the game of politics changed rapidly.

The paradox revealed by McAuley's investigation is that while much has changed in Russian politics during recent years, much continuity with the practices of the Soviet period is also evident. Politicians who have retained power at the regional level, as well as the few challengers who have unseated incumbents, have "relied heavily upon closely knit local and personal networks" (p. 18; see also pp. 243–4) in order to gain their objectives. Because of the collapse of the authority of the Communist Party of the Soviet Union, which encouraged the cultivation of personal linkages but maintained some institutionalization of the transfer and exercise of power, and also because neither parties nor other institutions are strong enough in the post-Soviet environment to share or limit the power of government leaders, the importance of patron-client ties has been heightened. In the absence of the cultural and institutional infrastructure of democracy, the adoption of new constitutional rules ostensibly designed to secure democracy has had the unintended effect of bringing the competition among political clans and machines into the open. One of McAuley's most important contributions is her convincing description of the manner in which the privatization of economic enterprises, by assigning regional government executives "responsibility for the distribution of valuable resources" (p. 247), enhanced the role of local political elites rather than furnished a base for more energetic competition among independent political interests. A valuable insight is McAuley's appreciation of the degree to which the post-Communist Russian state still penetrates society, influences the pattern of its growth, and shapes the expression of its political interests.

McAuley reports that at the level of the region and the large city in post-Communist Russia, power has gravitated to the government's executive leadership; that much of the burden of providing services to the population has shifted down to the regions and the local governments; that regional leaders spend a great deal of time in Moscow, making deals with central leaders; that the regional press has been largely tamed by its lack of financial independence; that regional legislative bodies, whether they struggle with the regional executive or remain docile, are highly ineffectual; that industrial enterprises are the bases of the most influential lobbies in regional politics; and that political party identification and ideology of candidates have little effect on the voters in regional elections. Her study shows that regional elites proved able to bend new rules and adapt new institutions to their advantage. She concludes that by the middle of the 1990s in each locale she studied, regardless of whether the politics of the region had been competitive or consensual at the end of the Soviet period, "the party of power, of the administration, emerged as the dominant authority" (p. 312). She summarizes her study with the statement that institutional changes aimed at bringing democratic processes "will not in themselves create an open and competitive politics. On the contrary, they may well allow an existing elite to secure its position, and, at a time when rule enforcement breaks down and the future is uncertain, inherited conventions of behavior will play a crucial role" (pp. 312–3).

McAuley's study must be welcomed as a highly informative addition to the literature on post-Soviet Russian politics. Yet, the Introduction does not set forth a comparative framework of generalizations concerning her area of research or present clear hypotheses to be tested. The fact that McAuley began her research when there was virtually no scholarly literature on change in regional and local politics in post-Communist political systems helps explain her relatively open-ended approach to the subject. Even so, her intriguing concept of "semi-corporatist politics" (pp. 175, 219) deserves fuller explanation. It must also be pointed out that although her statements about regional executives, legislatures, parties, and voters touch on a number of points dealt with by Adrian Campbell, Jeffrey Hahn, Peter Kirkow, Blair Ruble, Darrell Slider, Kathryn Stoner-Weiss, Marc Zlotnik, and others during the last few years, McAuley does not adequately show whether her findings confirm those scholars' conclusions. Though the book appeared in print in 1997, McAuley does not take into account most of the writing on regional politics in Russia published after 1994. Nevertheless, her work must be acknowledged as a pioneering study of indispensable value to those who wish to understand politics below the national level in the Russian Federation. The book is of broad significance because McAuley helps answer the perplexing question of why the nascent, vigorous social pluralism in the Russian republics during the late 1980s was replaced by the feeble political pluralism of the Russian Federation during the middle and late 1990s. McAuley sees the answer to that question in the weakness of the institutions of "political society" and the persistence of the state's domination of civil society.

Peronism without Perón: Unions, Parties, and Democracy in Argentina. By James W. McGuire. Stanford, CA: Stanford University Press, 1997. 388p. $55.00.

J. Patrice McSherry, *Long Island University/Brooklyn*

What explains Argentina's recurrent failure to consolidate a democratic political system? This political puzzle has engaged scholars for decades, producing a number of explanations. James W. McGuire suggests that Peronism's self-definition as a movement rather than a party has been a key source of Argentina's instability. He sets out to show that Juan Perón's legacy of "movementism" and indifference to party building led to weak institutionalization of the Peronist party, with negative implications for consolidation of Argentine democracy. McGuire presents a detailed analysis of the intricacies of politics in the Peronist trade unions and in several incarnations of the Peronist party within the broader tapestry of Argentine twentieth-century history. Perón and today Carlos Menem, McGuire asserts, have preferred to use personalistic control, "balancing" strategies, and the resources of loyal unions and the state rather than party structures to carry out political projects and dispense patronage, in order to solidify their own political dominance and prevent opportunities for rivals to gain independent power.

Only when Perón was abroad, between the mid-1950s and early 1970s, did party-building projects by powerful new figures (such as Augusto Vandor of the Metalworkers Union) commence, producing the beginnings of routinization and institutionalization of Peronism.

The author concentrates on two historical periods, 1962–66 and 1984–88, when sectors of Peronism tried, but failed, to better institutionalize Peronist party structures. In the first period, these failures meant that large sectors of Argentine labor had little stake in a democratic framework and legislative institutions—one factor that set the stage for military coups, McGuire argues. Another such factor was the "veto politics" of Peronism and its attendant use of nonparty channels to articulate interests, including mass demonstrations, strikes, factory occupations, and direct negotiations with other corporatist actors. In the second period, attempts by the renewal wing of Peronism to routinize and democratize the party were thwarted by internal power struggles as Menem, Lorenzo Miguel, and Antonio Cafiero jockeyed for leadership. McGuire's basic theme is that Argentina's history might have been quite different if Peronism had built an Argentine version of Britain's Labour Party. Incentives for supporting military coups might have decreased, incentives for supporting democracy increased. The argument is reasonable, though not without limitations.

McGuire calls his model for understanding Argentina's democratic instability a distributive conflict-party institutionalization approach. His argument clearly evokes Samuel Huntington's, although McGuire defines institutionalization differently. He specifies three components: value infusion, taken-for-grantedness, and behavior-shaping capacity, and he finds that the first has causal priority. Institutions must be valued before they can be taken for granted; and until they are taken for granted, they do little to shape behavior. Peronist leaders have rarely infused the party with value, McGuire argues. His definition of democracy is explicitly disaggregated from social justice; his minimum requirements are that leaders be chosen in fair, competitive elections in which all citizens may participate; that nonelected agents are not able to veto policy decisions; and that citizens have civil and political rights (p. 13). In McGuire's conceptualization, democracy may gradually improve in breadth, depth, and range over time. McGuire posits that a democracy is consolidated when its quality exceeds the procedural minimum and when it has achieved a fair degree of institutionalization. Following Juan Linz, McGuire identifies consolidation with taken-for-grantedness and behavioral influence; democracy becomes "the only game in town."

McGuire's depiction of Peronist party and union politics is authoritative and lively, with much useful and original data and a good intuitive sense of Argentine unionism. *Peronism without Perón* is doubtless one of the best recent analyses of the subject, and it is especially good on Perón's legacy and on the politics of Peronism in the 1980s. The explanation of Menem's successful efforts to weaken the Peronist party adds an important element to understanding the erosion of some democratic institutions and practices that has accompanied the current president's economic restructuring.

Despite the important contributions the book makes to our understanding of Peronism, some of McGuire's conceptual arguments are less persuasive. First, McGuire more than once implies that unmediated expressions of labor mobilization provoked or set the conditions for coups (although he backs away from this view in the concluding chapter). He argues that the failure of Vandor's party-building project helped to precipitate the 1966 coup (pp. 112, 147). McGuire draws a distinction between Chile and Uruguay, countries in which parties and parliaments were more institutionalized, and Argentina (p. 33). Yet, the former countries also succumbed to military dictatorship, at nearly the same time as Argentina (1970s), weakening the argument that lack of party institutionalization was a central determinant for democratic breakdown.

By focusing on the institutional and state levels of analysis to the near exclusion of the international level, McGuire underanalyzes crucial factors that contributed to the wave of military coups in the 1960s and 1970s, especially the Cold War context, the influence of U.S. counterinsurgency strategies, and the rise of politicized national security doctrines throughout Latin America. Moreover, by giving causal primacy to the Peronist movement's role in producing democratic instability, McGuire tends to underestimate the fact that other powerful actors were arguably even less committed to democracy. The messianic political projects of the military were more ambitious than simply to repress labor and leftist mobilization. General Onganía's West Point speech in 1964 was a harbinger of the military state to come, which institutionalized the Argentine military's priorities of "security and development," key pillars of national security doctrine. Important sectors of the military had their own motives for a "modernizing coup" and had little faith in liberal democracy. Thus, they abolished not only Peronism after the 1966 coup, as before, but all parties; they dismantled all constitutional institutions and channels of representation in the country. The behavior of the Argentine military was proactive, not merely reactive (even more so in 1976), and its political projects and ideologies are crucial independent variables in the explanation of democratic instability in Argentina.

On a related issue, I find the implication that strikes or protests provoke or set the stage for military coups a disturbing formulation. The Huntingtonian (or Schumpeterian) thesis that mass action and popular contestation undermine democratization has been challenged in much literature (recently Nancy Bermeo, "Myths of Moderation: Confrontation and Conflict during Democratic Transitions," *Comparative Politics* 29 [April 1997]:305–22).

To make his argument about the centrality of parties to democratic stability, McGuire shows that big landowners and other elites in Chile had a stake in competitive politics via conservative parties that controlled peasant and worker votes until electoral reforms in the late 1950s allowed new social groups to gain a voice. Then, elites began to opt for antiparty politics, a period McGuire compares to Argentina's 1912 electoral reform (p. 39). But there is a paradox here. If elites respect competitive politics only so long as they dominate the system and exclude other sectors, then in what sense can this be called democracy? Competition restricted to elites seems to fail McGuire's definitional requirements, and furthermore, the problem of democratic instability then must be traced not to lack of party institutionalization but to elite unwillingness to accommodate the interests of new social groups. Indeed, the case of Chile in 1973 suggests that elites also may be alarmed by strong institutionalized parties of the Left or of workers (not only movements), prompting them to jettison the democratic framework.

The amorphous nature of the concepts of democratic quality and consolidation leave this reviewer unsatisfied. Taken-for-grantedness relies on attitudes, and we are not told *whose* attitudes are significant—those of elites? the military? everyone? Attitudes also are subject to change. Institutions might lose their value-infusion in the eyes of former supporters; democracy might be deconsolidated. Yet, the appearance of consolidation might exist simply because several elections have occurred, but elections may take place

in nondemocratic conditions (Indonesia, Nigeria, and Central America come to mind); stability is not the same as democracy.

Finally, much contemporary literature divorces the concept of democracy from ideas of social and economic equality long associated with democracy. But how much inequality can democracy tolerate? The author acknowledges the link between inequality and distributive conflict, which leads to instability, thus implicitly noting the interrelationships (e.g., p. 260). One wishes for more theoretical exploration of the linkages between democracy and equality.

These critiques should not detract from the importance of James McGuire's book, which makes a significant contribution to key debates in the discipline. The book is well researched and readable, and it is an important resource for scholars of Argentina, labor history, parties, and political regimes.

Democracy and Dictatorship in Ghana and Tanzania. By Robert Pinkney. New York: St. Martin's, 1997. 230p. $59.95.

James R. Scarritt, *University of Colorado at Boulder*

Using a conceptual framework developed in his 1994 book, *Democracy in the Third World*, Pinkney compares Ghana and Tanzania with regard to their civil societies, prior regimes, democratic transition processes and outcomes, and potential for democratic consolidation. This framework focuses on types of democracy; the influence of political culture, civil society, economic success, and political ideologies and institutions on democratic prospects; the seeds of challenge to authoritarianism, the configuration of interests, and the resolution of conflicts as elements in the transition process; and the nature of that process as a key influence on the prospects for consolidation. Pinkney believes that it is interesting to compare these countries because both of them have an intermediate level of potential for successfully democratizing despite their widely different political histories; both their similarities and their differences help explain why they are at this level. An explanation based on such a broad and nonfalsifiable framework is less satisfying than one based on a tight theoretical model, but Pinkney uses his framework very effectively, given its inherent limitations. His extensive knowledge of the two countries, based on both first-hand experience and reading extensively in the relevant literature, allows him to do so.

Ghana is portrayed, accurately in my view, as having three ideological tendencies: state socialist, populist, and liberal. These have competed for power over a long history of civilian and military regimes supplanting one another by coups and returns to civilian rule. No system has worked well for a variety of reasons that Pinkney discusses, and Ghanaian society has been marked by several types of rather intense conflict stemming in large part from this ideological competition. The current Rawlings–National Democratic Congress regime, despite its conversions from a radical populist to a capitalist economic ideology and from a junta to a majority party in a competitive party system, remains within the populist tendency. It has weak roots in society, even in comparison to its opposition, which is an unstable combination of the other two ideological tendencies. Pinkney believes that the current party system, excessive presidential powers granted under the present constitution, and the antidemocratic, interventionist tendencies of the Ghanaian military are the clearest obstacles to democratic consolidation, but the potential effects of political culture, civil society, and most other political institutions in this regard are ambiguous. He thinks that competition between parties representing the other two ideological tendencies led to instability under economic decline but could provide a better basis for consolidation under the current favorable economic conditions.

In contrast, again accurately in my view, Tanzania is portrayed as having been dominated by a single socialist ideological tendency since independence (although it has had different interpretations), and as having experienced a high level of stability under a single-party system that allowed limited but significant internal competition and tried to combine the procedures of what Pinkney calls liberal democracy with the policies of popular democracy. Compared to Ghana, political culture is more pliant and egalitarian, and there has been less conflict, although civil society is weaker. The ruling party, despite its recent decline, is still much better organized than the newly created and fragmented opposition, which has limited regional and class (new bourgeoisie and disaffected workers) support. Civilian supremacy over the military is strongly entrenched by African standards, which is good for consolidation, although the centralization of other political institutions is not. A reorientation of economic policies has also occurred under externally imposed structural adjustment, but this has not reversed economic decline as it has in Ghana.

Pinkney also points out important similarities between the two countries. Ruling parties or groups dominated both transitions despite their own internal problems because social movements supporting democracy were so weak. He suggests that these countries differed sharply in this regard from earlier "third-wave" democratizers. In *Democratic Experiments in Africa* (1997), Bratton and van de Walle show that Ghana and Tanzania also differed significantly from other African countries, many of which experienced considerably more protest. Partly because they had so little voice in the transition process, oppositions in both countries strongly disputed the fairness of the posttransition elections. Another crucial similarity that Pinkney finds between these two countries, and one that I believe is more widespread, is the absence of viable alternatives to democracy, imperfect though it may be. For almost all groups in the complex configurations of interest that Pinkney lays out for both countries, political life is sufficiently better after transition to prevent a significant amount of what Huntington, in *The Third Wave* (1991), calls authoritarian nostalgia. Thus, Pinkney believes that both countries could remain unconsolidated democracies for a long time.

The facts that Pinkney lays out, analyzed within his framework, support a tentative conclusion that Tanzania has better prospects for democratic consolidation than does Ghana. My conclusion is probably based on a different weighing of the factors that Pinkney discusses, although it is impossible to be certain, because he does not weigh them. Perhaps we differ on the significance of internal competition within the Tanzanian ruling party in comparison to the significance of Ghana's short periods of multiparty democracy. Or perhaps we disagree about the significance of the organizational strength of ruling parties. These differences, if they exist, can only be resolved by formulating and testing a precise theory of the relation between the nature and extent of past democratic experience and consolidation prospects. We may also disagree about the significance of economic success or about the probable duration of Ghana's economic success. But resolution of these possible difference would also require theory formulation and testing. Pinkney's book should be read by those who are interested in theory building—as well as by those who just want to learn more about the two countries—

because it provides important clues for the development of relevant theories.

Political Participation in Beijing. By Tianjian Shi. Cambridge, MA: Harvard University Press, 1997. 334p. $49.95 cloth, $22.95 paper.

Yang Zhong, *University of Tennessee, Knoxville*

Empirical and systematic study of mass political culture and behavior in communist countries has eluded social scientists for decades. Despite the tremendous constraints, students of the former Soviet Union and communist Eastern Europe were already attempting to gauge political culture and behavior of ordinary citizens in those countries in the late 1970s and the 1980s through survey techniques. Empirical study of mass political culture and behavior, especially through survey techniques, has been a weak spot in the Western scholarship on China as well. As Allan Liu recently noted in *Mass Politics in the People's Republic* (1996, p. 2): "Until recently, U.S. scholars specializing in China paid episodic attention to Chinese mass sentiments. The concept of public opinion did not appear in the lexicon of contemporary U.S. sinology until the 1980s." Indeed, much attention in the study of Chinese politics has been given to the Chinese elite and formal political institutions and processes. Thanks to the relaxation of political control in the People's Republic of China (PRC) since the 1980s, scholars in the West have shown noticeable interest in political attitude and behavior at the mass level. Even though using survey research methods is still a novel enterprise in China studies, the limited number of such endeavors have already yielded important information and empirical findings about patterns of political culture and behavior in the PRC.

Tianjian Shi's *Political Participation in Beijing*, which uses survey techniques in an extensive and intensive study of the forms and patterns of citizen political participation in Beijing, is a breakthrough in the field and points out a new direction in the investigation of Chinese politics. A central argument in the book is that the institutional setting, most noticeably the work unit system, in China shapes the forms and determines the nature of political participation. Specifically, work units in urban China are the center of citizens' life. The Chinese work unit is charged with political, economic, and social functions that range from serving as a political control tool to a government propaganda machine, from distributing apartments to providing education, and from solving marital problems to maintaining social order. This feature of institutional setting determines that most political participatory acts in the PRC occur in the work units, which is different from most other societies. Another unique aspect of political participation in the PRC, according to Shi, is that it focuses mainly on the policy implementation stage rather than the policymaking stage. Chinese work units are primarily policy implementation bodies. Since many of the policies are vaguely worded (often on purpose) and lack clarity, there is much leeway for work unit officials to interpret and change the policies.

Shi is obviously not the first to make these observations. The most valuable contribution of this book is that Shi, by using sophisticated and vigorous social science methods, empirically documents the forms and degrees of political participation in urban China and presents many interesting and intriguing findings. Specifically, he identifies 28 political participatory acts and groups them into various categories: voting, campaign activities, appeals, adversarial activities, cronyism, resistance, and boycotts. Shi found that in the late 1980s only one-tenth of the people in urban Beijing were totally apathetic and never engaged in any political activity to articulate their interests. In contrast to participatory behavior in liberal democracies, people in urban Beijing, due to structural constraints of the political system, rely primarily on appeals, adversarial activities, resistance, and cronyism to get what they want. Shi further found that, contrary to situations in other countries, engagement by urban Beijing residents in resistance and cronyism negatively correlate with efficacy and regime-based trust. This finding suggests that engagement in these activities in Beijing makes people feel alienated from rather than identify with the political community.

Another strength of the book is its comparative perspective. Not only did Shi use standard political science theories to study political participatory behavior in Beijing, but also he constantly compares China with findings from liberal democracies and the former Soviet Union to illustrate the differences and similarities. Still another strength of the book is its insights on participatory political acts by ordinary Beijing citizens. Shi grew up in China. He was a "sent-down" youth during the Cultural Revolution and later became a truck driver in Beijing. His life experiences in work units enable him to know such concepts or practices as *zuzhi dui zuzhi* (organization to organization) and *chuanxiaoxie* (giving one tight shoes to wear). Chinese scholars are often accused of seeing the trees not the forest, while Western scholars are often accused of the reverse. Shi can see both the trees and the forest.

As with any good book, there are places for improvement. I have a problem with Shi's definition of political participation: "activities by private citizens aimed at influencing the actual results of governmental policy" (p. 21). Many issues involved in political participation do not have much to do with governmental policies. Allocation of apartments, allowing children to succeed to their parents' jobs, and even awarding bonuses are policies made by work units. If the author considers the work unit a basic level of government, there are still many civil matters handled by work unit officials that are not related to official policies, such as marital disputes, family problems, and disputes between neighbors. There is no question that many acts of interest articulation at the work unit level involve these issues in China. I am not sure how the questions were phrased regarding political participation in the survey questionnaire. What scenarios were covered and what scenarios were excluded? (The author should have provided a complete questionnaire as an appendix in the book.) Shi apparently agonized over the issue of what constitutes political activities (pp. 89–91). The solution was not a perfect one, and I am sure there is no perfect solution.

Another issue is the case study of Beijing and the justification of generalizing the findings to the rest of China. Shi did not fully explain why he chose Beijing as the site of his study of political participation. My guess is that it was chosen for convenience. Still, the author should have discussed why the findings can and should be generalized to the rest of *urban* China, since Shi often refers to China instead of urban Beijing (the rural counties in Beijing were not included in this study) in reporting, analyzing, and interpreting the findings. Some people believe that Beijing urban residents are more politically active than the rest of the population. Another issue that should be addressed is the implications of this study for rural China, where people are organized quite differently from urban China, especially after the early 1980s. After all, 70–80% of Chinese live in the countryside.

Shortcomings aside, I believe *Political Participation in*

Beijing is a major scholarly contribution and brings a breath of fresh air into the field of China studies.

Remembering Stalin's Victims: Popular Memory and the End of the USSR. By Kathleen E. Smith. Ithaca, NY: Cornell University Press, 1996. 219p. $29.95.

Jeffrey W. Hahn, *Villanova University*

One of the most troubling questions for societies making a transition from authoritarian rule is how those in power will come to terms with acts of repression committed by the past regime. To ignore the question is impossible and risks the credibility and legitimacy of those who would liberalize the system; to seek to settle scores with those who committed the crimes threatens members of the old regime with whom one must negotiate a peaceful transfer of power. It may also raise unsettling questions about the extent of complicity in those crimes on the part of those who cooperated, even unwillingly, with the old regime. Kathleen Smith has produced a valuable study of how this issue has played a role in the transformation of the Soviet system. Drawing on research from the literature on transitions elsewhere, the author argues that the Russian experience in coming to grips with its Stalinist past is distinctive. Her argument is rooted in the contrast between authoritarian and totalitarian systems. Both may be equally repressive, but the former accommodates elements of civil society while the latter seeks to eradicate any pluralism; the Leninist party-state system claimed for itself a monopoly of truth. This distinction lies at the heart of the book's major thesis: What made Soviet leaders' efforts to come to grips with the past so difficult was reconciling their putative infallibility with the clear fact that serious mistakes had been made. To acknowledge the mistakes threatened the legitimacy of their claim to power and the privileges that went with it. The existence of elements of a civil society, such as the Catholic church or Solidarity in Poland, might have helped liberalization in the Soviet Union, but none were permitted.

Two chapters of the book reveal how liberalizing Soviet leaders attempted to deal with the dilemma of acknowledging Stalin's crimes while at the same time preserving the party's claim that it alone had the right to decide what was best for society. Smith shows how both Khrushchev and Gorbachev tried, in the end unsuccessfully, to circumscribe investigations into the past so as not to jeopardize the party's authority in the present. The best parts of the book are the chapters demonstrating how important the issue of anti-Stalinism was as a catalyst for the development of dissent under Khrushchev and Brezhnev and for the genesis of a civil society in Russia under Gorbachev. Smith traces how anti-Stalinist dissent was institutionalized in the period of perestroika and led to the creation of Memorial, the first truly independent social movement in Russia with wide popular appeal. Her description of how it came to be is fascinating and represents some of the best writing and research in this extremely well-written book. Equally important, if less uplifting, are the chapters that describe the problems, both internal and external, which Memorial faced and which contributed to the decline of anti-Stalinism as opposition forces moved from participation in social movements to participation in political parties. The book concludes with speculation on what the end of Soviet rule means for settling accounts with Russia's Stalinist past.

Remembering Stalin's Victims contributes both to our knowledge of transition politics and to our knowledge of Soviet and Russian politics. The author's excellent command of the literature on transitions enables her to link explicitly and compare what she finds happening in Russia with issues of extrication and legitimacy in other transforming societies. She has something new and important to say about how civil society develops and why its absence in Leninist systems is an impediment to democratic development. Her research also offers insight into the dynamics of social movements and the factors that determine their success as political actors. Those interested in Russia's transitional politics will be informed by her discussion of Memorial and the difficulties it faced in its battle for official recognition. Gorbachev's role in this appears highly ambivalent. What makes Smith's narrative so compelling are her interviews with the leaders of Memorial and her access to the organization's internal documents during the six months of field research she conducted in 1991 and in shorter subsequent visits to Russia, Belarus, and Ukraine. Not only is the research solid, but also the author's analysis is presented in as scholarly a manner as one could reasonably expect when dealing with a subject as emotionally charged as remembering the history of Stalin's depredations. The text is enlivened by occasional photographs related to the anti-Stalinist movement in Russia.

This fine piece of scholarship is not without shortcomings. One is a certain lack of clarity in focus. On the one hand, the book is about how Russia has dealt with its Stalinist history. It is about destalinization and human rights. Here the focus is on how regimes in transition grapple with memories of injustice. On the other hand, it is about Memorial as a political organization, and here the focus is on the emergence of civil society in Russia and the impediments it faced. While these themes are obviously not unrelated, it was not always clear whether I was reading a history of the human rights movement in Russia or an analysis of the evolution of group politics during perestroika. Of the two foci, the stronger sections are those that deal with the latter theme. Those on the history of destalinization do not offer much that is new. There is also something of a time problem with this book. When the author does focus on Memorial, she covers well what happened in 1988–91, but the account of what happened to the organization after 1991 is weak, if nonexistent, despite visits by Smith to Russia in 1992, 1993, and 1994. Finally, there is a tendency to overstate the importance of the issue of anti-Stalinism in the political transformation of Russia. The issue needs to be seen in the context of the important institutional changes going on at the same time. Of these, none was more important than the introduction of competitive elections, which not only released the centrifugal forces of nationalism in the republican legislatures but also changed the nature of local Russian politics. To argue that the local party-state apparatus was a monolith "unified" in opposition to unofficial organizations ("informals") (see p. 118ff.) is possible in 1988 but not by 1990–91. By then the local party organization in many areas was disintegrating, creating a new climate for Memorial's activities, as evidence from Yaroslavl' (see p. 128) provided by the author herself suggests.

The Clash of Rights: Liberty, Equality, and Legitimacy in Pluralist Democracy. By Paul M. Sniderman, Joseph F. Fletcher, Peter H. Russell, and Phillip E. Tetlock. New Haven, CT: Yale University Press, 1996. 291p. $35.00 cloth, $18.00 paper.

Patrick Malcolmson, *St. Thomas University*

Perhaps it is less the case now than a few decades ago that a political science devoted to helping democracy is seen as degrading itself into a form of civic education. This book is an

excellent case in point. The authors clearly are as concerned about what the values of democracy ought to be as with describing what they presently are. They begin with the announcement that the book is "an essay in democratic theory, albeit in an empirical key." Democratic theory has long relied on, and continued to verify, the thesis "that political elites, by virtue of their involvement in politics, outstrip ordinary citizens in commitment to civil liberties and democratic rights" (p. 47). When push comes to shove over the right to free speech, for example, it is the elite who are more likely to stand on principle and defend the rights of those who would express offensive and repugnant opinions.

Sniderman and company challenge this thesis. The book demonstrates that "on a wide array of issues requiring political elites to balance competing considerations of individual rights and social order, their responses are indistinguishable from those of the public at large" (p. 24). When it comes to classic freedom of expression issues, the elite are more likely than the general public to hold their ground on democratic rights; but apart from these issues, the authors find that elite opinion and mass opinion converge. Their findings should cause most readers to sit up and take notice. So much for the old view that the elite are the guardians of liberal democracy.

According to the authors, a major shortcoming in previous research is that it failed to take into account the effects of the party system. It presupposed that the decisive contrast is between the elite and the mass public. Using the conceptual framework of Schumpeter to reexamine the work of McCloskey, Brill, and many others, the authors provide a new interpretation. The research in this book concludes that the electoral system operates to provide a choice between competing sets of elites. The deeper flaw was that, beginning with the "groundbreaking work" of V. O. Key, which in turn inspired so much of the scholarship on democratic elitism, the concept of the elite was "understood in curiously apolitical terms"—outside the institutional context of politics and independent of the party system (p. 242).

This argument is both interesting and valuable in illuminating the present condition of democracy in North America. But the argument so far described is only the starting point for the real thesis of the book: The reason consensus cannot be easily achieved by the elite or the mass public when it comes to issues of democratic rights is that rights are "inherently contestable."

The notion of democratic elitism was in fact based upon what the authors term the "error thesis." As they point out, most people support fundamental democratic rights in the abstract; but when it comes to a particular issue of free speech, they often desert their post. One can only conclude that this reflects "a failure to understand how an abstract right of which they approve applies to a specific issue" (p. 53). The connection between the error thesis and the thesis of democratic elitism is fairly obvious: The elite are less prone to such failures in comprehension.

The authors argue against the error thesis and in favor of the "inherent contestability" thesis. There is no one correct theory of rights, no right answer, so to speak. Hence, there can be no error. Rights are inherently contestable concepts. Democracy must, therefore, find its foundation on a theory of "value pluralism" or democratic relativism.

Using sophisticated polling techniques that attempt to measure how "pliable" people are in holding their opinions, the authors survey public opinion on issues involving racism and hate speech, censorship, pornography, feminism, an array of equality rights, aboriginal rights, and issues of identity politics. The public is, for the most part, the Canadian public, and the issues are identifiably Canadian.

Because this is a work of empirical theory, it should go without saying that the philosophical foundation of the argument—the inherent contestability thesis—can only be supported by but not deduced from the empirical evidence the authors provide. This evidence is presented in a clear and straightforward manner in the form of bar charts, and it is indeed illuminating. If the error thesis is correct, then it suggests that the elite are now as prone to error as the general public; if the inherent contestability thesis is correct, then it suggests that the distinction between the elite and public cannot be based on the profundity of their understanding of rights issues.

This book raises a number of deeply troubling questions for those who study and care about contemporary democracy. It challenges the received scholarship, puts forward a new interpretation of rights politics, and suggests how political institutions might be designed to take democratic reality into better account. Regardless of the focus on Canada, the readers of this journal will, upon examination, find this to be a challenging but rewarding new work of scholarship in empirical democratic theory.

Post-Communist Presidents. Edited by Ray Taras. Cambridge: Cambridge University Press, 1997. 250p. $54.95 cloth, $18.95 paper.

Timothy Frye, *The Ohio State University*

Ray Taras and his coauthors have produced a useful work that captures the intricacy of creating presidential power in the early years of the postcommunist transformation. Using studies of the definition and exercise of presidential power in Russia, Ukraine, Kazakstan, Hungary, Poland, and the Czech Republic, the cases provide rich empirical accounts that remind us of the great contingency of institutional choice during the early years of the postcommunist transformation.

Juan Linz and Jon Elster provide relatively brief introductory and concluding chapters that echo arguments for which they are well known. In the introduction, Linz draws evidence from the postcommunist cases to reinforce his belief in the perils of presidentialism. His defense here is more qualified than in past works but remains strong. In the conclusion, Elster argues that bargaining among self-interested politicians, rather than considered debates about the merits of different institutional arrangements, has driven the process. He concludes that for the institutional designers of the postcommunist world, reason has been crowded out by interest and passion.

Ray Taras introduces the cases studies with a review of debates on presidentialism/parliamentarism, then detours to useful discussion of the historical roots of the concept of separation of powers. Drawing on historical perspectives from Bagehot to Laski, Taras reminds us that debates over the effects of institutions have a long tradition in the field. He also provides a thoughtful parsing of the concept of separation of powers into four different usages: the rule of law, accountability, balancing, and efficiency. Taras's brief treatment of separation of powers provides a clear statement of a complex concept.

Taras notes that the chapters are a means to examine two issues: the exercise of presidential power and the effect of different arrangements of presidential power on the consolidation of democracy. By this measure, the volume falls short of the mark. The notoriously slippery concepts of presidential

power and democratic consolidation are often discussed but not analyzed systematically. The chapters are largely descriptive and make little attempt to generate or test theoretical propositions, particularly across cases. Rather than build or test theories, the authors use theoretical insights from the institutional literature to inform their cases studies. By this measure, the volume fares much better.

The case studies stand on their own as useful works. They are well researched and thorough. The authors resist the tendency to ascribe all political events within a country to its particular institutional arrangement and recognize the difficulty of separating the powers of the office from the powers of the individual officeholder.

The most notable chapters try to place the case study within the broader framework of presidential power in the region. Andrew Wilson provides a sound treatment of struggles for power between the Ukrainian parliament and successive presidents, Leonid Kravchuk and Leonid Kuchma. In Wilson's view, legislative gridlock was not determined solely by institutions but also by the personalities of the two Leonids. Kravchuk had little taste for conflict, while Kuchma relished doing battle with the parliament. Wilson deftly traces how Kuchma expanded the powers of the Ukrainian presidency but fell short of creating a Yeltsinesque superpresidential regime. These points give the chapter a helpful comparative perspective.

Krzysztof Jasiewicz's chapter on the Polish presidency is also quite well done. He argues that Walesa, by pushing to expand his powers so aggressively, helped consolidate parties within the fractured Polish parliament to oppose the creation of a strong president. Jasiewicz also provides a valuable comparison of changes in presidential powers over time.

Patrick O'Neill's discussion of the complex negotiations that created the Hungarian presidency shows considerable skill, and Sharon Wolchik's treatment of the Czech Republic's "philosopher-president" Waclav Havel is equally deft. These cases provide a nice contrast on how weak presidents can sometimes expand their formal powers. O'Neill describes how Arpad Goncz used vagaries in the formal rules of the Hungarian presidency to expand his formal powers, while Wolchik depicts how Havel failed to translate his moral authority into stronger fòrmal powers, despite expending much effort toward this goal.

Writing on the presidency in Russia, Stephen White is doubly challenged. The case is well known. In addition, the chapter ends just after the 1996 presidential election, giving the author little time for reflection on this event. As if to offset these disadvantages, White devotes more attention to lesser-known aspects of the Gorbachev presidency than to central issues in the Yeltsin presidency, such as the dissolution of parliament in October 1993. This makes for a somewhat unbalanced chapter. In contrast, Martha Brill Olcott's work on the Nazarbaev presidency in Kazakstan is somewhat more balanced and devotes more attention to central political issues, such as the dissolution of the Kazak parliament in 1994.

The chapters work better as individual pieces than as a whole. This is unfortunate, given the tremendous opportunity for comparative work on presidential power in the postcommunist world. As a volume, the work suffers from two shortcomings. First, the chapters present little data that is comparable across cases. The authors use a variety of measures for presidential power, including indices by Shugart and Carey and by James McGregor as well as idiosyncratic judgments by the authors. This makes it difficult to judge the relative strengths of the presidents across cases. Measuring the formal or informal powers of the presidency is admittedly difficult, but such measures are needed to generate a deeper understanding of the cases.

Second, the empirically rich case studies capture much of the context in which decisions were made and emphasize the unique aspects of each case, but that makes comparison across cases difficult. By taking a holistic approach to the development of presidential powers, the authors are forced to give less treatment to important questions that could have been better served by a more narrow focus. For example, more explicit comparisons could have been made of the causes of the dissolution of the Russian and Kazak parliaments and the nondissolution of the Ukrainian parliament. Or attention could have been devoted to variation in formal and informal powers across cases. By focusing on a specific aspect of presidential power—such as decree making, government formation, the choice of presidential versus parliamentary systems, or the role of constitutional courts—the authors could have made a larger theoretical contribution.

The case studies tell us much about particular presidencies and create a solid foundation for future research. The task for scholars working on postcommunist political institutions now is to move beyond rich political histories to directed studies that test and build theory. As scholars seek to create more general theories about institutional choice, electoral rules, and presidential power across the postcommunist cases, the chapters in this volume remind us of the great contingency of institutional developments in the early years of the postcommunist transformation. Taras and his coauthors have provided a valuable service by tracing how small details and nuances shaped the process of institutional choice in idiosyncratic ways that will be difficult to build into theories. Perhaps the main lesson from the case studies themselves is that building powerful and parsimonious theories about presidential power in the postcommunist world will be a difficult task.

Demanding Democracy: Reform and Reaction in Costa Rica and Guatemala, 1870s–1950s. Deborah J. Yashar. Stanford, CA: Stanford University Press, 1997. 319p. $49.50 cloth, $17.95 paper.

Mitchell A. Seligson, *University of Pittsburgh*

Deborah Yashar's richly detailed and nuanced case studies of Guatemala and Costa Rica should give pause to those who have been disparaging the utility of area studies research in political science. Yashar asks two major theoretical questions: What are the conditions under which democracies are founded? What are the conditions under which democracies can be sustained? Many democracies have been initiated, but relatively few have survived over long periods. In Latin America, only Costa Rica has avoided breakdown, while once promising cases, such as Chile and Uruguay, each succumbed to authoritarian assaults. It is with good reason, therefore, that Yashar selects Costa Rica for detailed analysis. The resilience of its democracy, in a region characterized by instability, revolution, coups, and brutally authoritarian regimes, has puzzled social scientists for many years. Guatemala serves as the counterpoint, a country in which only very recently has there been a continuous series of democratic elections, but in which civilian supremacy over the military and the prospects of executive coups are issues that are far from being resolved.

While making no claim to universality (p. 2), the author presents a theory which attempts to go beyond approaches that focus exclusively either on the role of key actors in the founding of democracy or on structural conditions that

determine the ability of democracy to survive. The core of the theory rests on the conditions which help build coalitions that will both found and maintain democracies. While agreeing that the role of markets and states matters, Yashar believes they are not determinant. Therefore, approaches that rely primarily on underlying structural conditions are subsumed by Yashar under the rubric of coalitions. Following Dahl, she argues that when economic and political resources are highly concentrated, it is more difficult to build a democratizing coalition. To found and sustain democracy, it is necessary to break the stranglehold of old elites on resources and create new transclass alliances that incorporate at least components of the masses. In developing countries, rural allies need to be sought, since much of the population and production remain in those areas. The key for a democratic breakthrough is a division within the elite coupled with an organized civil society making demands for citizenship rights. Those breakthroughs occur at "critical junctures," a thesis that draws upon the concept developed by Ruth Berins Collier and David Collier. When there are no divisions within the elite, but the popular sectors demand democracy, the result is either the marginalization or repression of the masses. If the elites are divided, but there is no popular pressure, all that one can expect is for one form of authoritarian rule to be replaced by another. For democracy to emerge and be sustained, argues Yashar, both elite division and civil society pressure must be combined simultaneously.

This is certainly an interesting thesis, one that moves us away from glorifying the heroes who found democracy, or from assuming a tight nexus between certain economic forms and democratic emergence. It also minimizes the importance of foreign intervention, a factor that has often been seen as central in Latin America, and fundamental in the case of Guatemala in particular. The thesis is supported in the book by a very detailed look at the two countries, covering the 1870s through the early 1950s. The theoretical unity of the work is greatly enhanced by the treatment within each chapter of both cases, so that the reader can easily draw comparisons. This is certainly an important advance over comparative politics studies that devote separate chapters to each country, leaving it up to the reader to make the connections.

The two case studies are presented within the framework of a most similar systems design, and it is here that some readers may quibble with the conclusions. Early in the book (Figure 1, p. 8), the author depicts the parallels in the political histories of the two countries, showing that they both went through a long undemocratic period, which was radically altered in the 1940s, but that only in Costa Rica did that period of democratization last. In Guatemala, in contrast, "military authoritarianism" replaced democracy as a result of the coup of 1954. Yashar makes the claim that, before the 1950s, "Costa Rica and Guatemala experienced variations *within* broadly parallel regime trajectories of authoritarianism and democracy through the middle of the twentieth century" (p. 7). She presents data to show that up until the early 1950s, when the two countries began to follow sharply different trajectories, they were similar both politically and economically (pp. 5–7).

Many scholars of Central America would argue that those parallels are overdrawn. They would take sharpest issue with Yashar's statement that "Guatemala founded authoritarian rule in 1954," whereas "historical and conjunctural factors interacted to found political democracy in Costa Rica in 1948" (p. 211). Brutal authoritarian rule has prevailed for almost all of Guatemala's history since independence, whereas incipient democratic traditions can be found in Costa Rica for at least the last 100 years. Therefore, to argue that authoritarian and democratic rule were *founded* in the 1948–1954 period in these two countries is not consistent with the facts as presented in the book itself.

One of the most important differences between the two countries, which Yashar details in her book, is the presence of a large, subordinate Indian population subject to repressive labor conditions in Guatemala, but there is no such sizeable population in Costa Rica. There the indigenous population was small from the outset and largely disappeared in the early years of Spanish colonization. The historical shortage of labor, coupled with ethnic homogeneity, seems to have rendered impossible the widespread emergence of a serf-like labor force in Costa Rica. Perhaps out of these important differences emerged many others, including a stronger civil society. One factor contributing to that strength was the establishment of universal primary education in Costa Rica in the 1880s. By the late 1940s, nearly four-fifths of the population was literate, compared to about one-quarter in Guatemala. Indeed, Guatemalan literacy rates in the early 1990s were about the same as Costa Rican rates in 1915.

While it is true that before the 1950s political rule was oligarchical in Costa Rica, Yashar does not sufficiently stress the strong civic traditions and respect for human rights that were (and largely still are) absent in Guatemala. Yashar also minimizes the importance of the role of external factors; the United States, for example, played a pivotal role in the coup that extinguished Guatemalan democracy in 1954, whereas it played a small but positive role in trying to bring about a negotiated conclusion to the Costa Rican Civil War of 1948.

Yashar correctly emphasizes that the survival of democracy depends on state penetration of the countryside and the state's ability to "subvert traditional elites" (p. 231). Yet, traditional rural elites in Costa Rica were very different from those in Guatemala, as is shown on pages 30–5; the yawning ethnic gap that separated large landowners from their debt-peons in Guatemala was virtually nowhere to be found in Costa Rica. Thus, in order for democratization to take hold, there was much less need to break the backs of the rural elite in Costa Rica than in Guatemala, where a massive land reform implemented in the early 1950s proved central to engendering the conflict that eventually culminated in the 1954 coup.

This is a well-written and carefully researched book. It rests its scholarship on a wealth of secondary and archival sources, coupled with transcriptions of the author's illuminating interviews with key political figures who participated in many of the events described. The theoretical advance of an explicit link between the conditions fostering democracy's founding with those accounting for its endurance will help us better project the prospects for survival of the dozens of democracies that have emerged in recent years. Students of those cases would do well to pay close attention to Yashar's theory.

International Relations

Post-Realism: The Rhetorical Turn in International Relations. Edited by Francis A. Beer and Robert Hariman. East Lansing: Michigan State University Press, 1996. 429p. $35.95 cloth, $24.95 paper.

Ronald J. Deibert, *University of Toronto*

It is now commonplace to note that we live in a world of increasingly intense media encounters, where the line between the "image" and the "reality" are all but dissolved. Perhaps nowhere is this phenomenon more intense than in those focused moments of high global hostility, such as that which has characterized recent events in the Persian Gulf. Here, the familiar talking heads offer strategic mediations on CNN that draw upon a rich and diverse well of realist maxims, rhetorical devices, and allegories. Percolating through generations of academics and foreign policy practitioners acculturated into the *weltanschauung* of realpolitik, these maxims, devices, and allegories at once precede and shape the "reality" to be apprehended "objectively." We can pretend that the two are distinct—the world "out there" and the academic study of it, that is—but of course they are not. In the hypermedia environment, more than ever before, theories of world politics are inextricably intertwined with and help reproduce the world they set out to represent.

The chapters collected in *Post-Realism* take aim at understanding more fully the processes by which this interaction takes place. Specifically, the volume is oriented around the rhetorical power and persuasive appeal of realism, the most dominant theoretical approach in the international relations field over the last century. Collectively, the authors explore realism not so much as a positive theory on its own terms but as a form of "persuasive discourse"—a powerful set of stories about how the world should and does operate, who are its main actors, and what are the main lessons of history. As many of the chapters note, the role of rhetoric and metaphor has been underappreciated in international relations theory for a variety of mostly epistemological reasons. The authors seek to rectify this oversight by focusing systematically on such issues, thus falling alongside other recent submissions into what has been called "the linguistic" or "rhetorical turn" in the social sciences.

This is a tightly edited and cogently organized volume with a high level of writing throughout. The editors, Francis Beer and Robert Hariman, begin with a clear introductory piece that nicely sets the backdrop and capsulates many of the precise ways a "post-realist" approach differs from a traditional realist one. The chapters split into three major sections. The first, "Rereading Realist Writers," includes essays by Hariman, Ivie, Aune, Jones, Epp, and Goodnight on the writings of Kissinger, Kennan, Niebuhr, Carr, Wight, and Morgenthau, respectively. All are very strong chapters that provide detailed and insightful analyses blending the personal and political convictions, historical contexts, and intended audiences of the authors concerned. Roger Epp's thorough excavation of Martin Wight is particularly nuanced and refreshing, and it goes far beyond the standard aphorism for which Wight is most known, that there can be no "international theory," to reveal a complex and multifaceted character struggling with numerous and overlapping debates and doctrines.

The second section, "Rewriting Realist Concepts," includes chapters, most of which are intellectually provocative and cast in broad theoretical and historical terms, that critique and reformulate realist concepts and categories. Jean Elshtain provides rich relief on the grip and almost irresistible attraction of the notion of sovereignty while at the same time pointing to other possible formulations beyond its horizons. Paul Chilton's essay, "The Meaning of Security," catalogs the powerful set of symbols activated by this normatively loaded and centrally important realist term. Drawing on the metaphorical analyses of George Lakoff and others, Jennifer Milliken shows how the metaphors surrounding "prestige" and "reputation" have long animated U.S. foreign policy with clear political consequences. In "Nationalism and Realist Discourses," Yosef Lapid investigates the "neglect-of-nationalism-problem" in neorealism, tracing it to its positivist-scientific proclivities and, to a somewhat lesser extent, to those of earlier realist formulations as well. V. Spike Peterson hammers into pieces the hard masculine core of realism that was sculpted out of the gender-biased legacies of Western rationality and then begins to weave the fabric for a "feminist post-realism" patterned on a more holistic design. Easily the most flamboyant and provocative of the chapters is James Der Derian's reinterpretation of realism, which begins with a forty-plus listing of the many different senses in which "realism" surfaces, from magical and minimalist to super- and hyper-. Why? Because far too often realists in international relations theory present only one "flavor": a "uniform, self-evident, and transparent . . . vanilla" (p. 277). This is an essay that does not talk about metaphors and rhetoric—it *employs* them with devastating finesse and wit to unsettle established ways of seeing the world.

The final major section, "Rewriting Foreign Policy," includes chapters by Sylvan and Majeski, Doty, Wilmer, and Beer and Boynton that put forth several examples of what would constitute a "post-realist" research program in the area of foreign policy. Although each obviously differs in range and nuance, these chapters present convincing evidence that a postrealist approach, with its emphasis on metaphor and rhetoric, can produce interesting, insightful, and useful knowledge for those interested in foreign policy and world politics.

All "posts-"—by definition—seek to move beyond *something*, even while retaining elements of that which they leave behind. Although certainly diverse, the chapters nonetheless share enough unity that it is fair to ask what elements of the old realism does this new version leave behind or move beyond? Perhaps twice removed from these rhetorical analyses are any notions of natural or material factors, whether of the "human," "technological," or "environmental" kind. Certainly, the *animus dominandi* of Morgenthau and the "Fallen Man" of Niebuhr are analyzed for their rhetorical or discursive weight, but they are blended out of the postrealist approach itself. Of course, human nature (indeed, all forms of nature) is out of style in these antiessentialist times and has been abandoned by realists since the structural formulations of Waltz. But one wonders whether some vitally important factors are being obscured and marginalized by a pure focus on words, language, discourse, and rhetoric. The natural impulses, desires, and fears that connect us to our "animal" side, the cognitive traits and adaptations that have evolved over millennia, the material instruments of technology, the "forces" of production and destruction, the influence of such environmental contingencies as climate, geography, resources, and disease, are all sopped up and absorbed by postrealist constructivism. (One exception is Peterson's "feminist post-realism," which makes reference to the biologically based importance of "nurturance.") Perhaps this is a component of at least some varieties of classical realism (not to mention Marxist and other historical materialist approaches) that would be better off revised and reformulated, rather than left behind?

What else do postrealists move beyond? One element for which neorealists certainly strive is a clear basis upon which to adjudicate competing theories. However spurious their positivism may appear to many, at least explicit criteria are formulated and adopted. Yet, with their acknowledgement of the constitutive role of language, postrealists must, by definition, move beyond representationalist epistemologies of this sort. What is less clear, however, is what they propose to take its place. On what basis, for example, are we to judge whether Charles Jones's fine interpretation of E.H. Carr is better than a possible competing one? In the introductory chapter, the editors allude to some other works that address these issues and hint at a possible accommodation with positivism, but the analysis stops short of saying precisely how this accommodation would manifest itself (pp. 10–3). In the conclusion, there are some criteria set forth by which a practitioner might formulate critical foreign policy, but these are directed at evaluating possible courses of state action and not alternative explanations. And in useful passages written by the editors as introductions to each major section there are hints of a pragmatic approach that would see interpretations judged on the ethical and normative consequences that follow from their being adopted. But a sustained and direct confrontation with the issue of adjudication between competing postrealist interpretations would have strengthened the volume and addressed a critical issue at the heart of the "linguistic turn."

Nonetheless, this is a fine edited volume that makes a serious contribution to recent scholarship in foreign policy, rhetoric studies, and international relations theory. The chapters are well integrated, intelligently written, and provocative. Moreover, the book addresses a topic that will only become more important as we enter deeper into a world of swirling images and digitally mediated realities—the persuasive power of rhetoric, metaphor, symbols, and signs.

Friends in Need: Burden Sharing in the Gulf War. Edited by Andrew Bennett, Joseph Lepgold, and Danny Unger. New York: St. Martin's, 1997. 362p. $55.00.

Gerald Steinberg, *BESA Center, Bar Ilan University*

In 1991, the Bush administration went to war against Saddam Hussein, claiming to be leading a broad coalition. In early 1998, however, when President Clinton prepared another attack following continued Iraqi interference with the U.N. arms inspectors, there was no pretense of a coalition. What was the nature of the initial alliance against Iraq, and what happened between 1991 and 1998?

The authors of *Friends in Need* examine the 1991 coalition from the perspective of alliance theory and burden sharing. After the editors establish a systematic framework in the opening chapter, the contributors of the individual cases use this framework to analyze the behavior of some of the countries in the coalition. The model, which extends the burden-sharing literature from the context of NATO during the Cold War, consists of five externally and internally derived hypotheses. The issue of collective action and the problem of free riders, dependence on the alliance leader, and domestic politics emerge as the central factors.

If Iraq was a threat to all members of the coalition (militarily in the region, and in terms of oil market stability for the other states), and if the United States was prepared to shoulder most of the military and economic burden to protect its own interests, then what motive did the other threatened states have to contribute to the confrontation with Iraq? For most of the states, the answers are not surprising, and the use of the heavy weapons of social science are too powerful for the issues at hand. Yet, the effort and systematic case studies are important contributions to alliance literature. After emerging as the victor in the Cold War, the United States was (and is) the only potential leader of the posse, and the elaborate research design used to explain U.S. leadership is unnecessary. More interestingly, since the United States had the military power to act independently, and "most of the coalition partners were more trouble than they were worth in terms of actual military effectiveness" (p. 45), why did the Bush administration twist the arms of so many supporting actors?

In part, American domestic politics played a role. To gain support from the Democratic majority in Congress, which was hesitant to take risks to restore Kuwait's decadent monarchy, Bush thought he needed the facade of a broad international coalition. Once approached, many leaders feared that by refusing the American request, they would lose points in other areas. Korea and Japan depend on U.S. forces for stability in Asia, Germany needed diplomatic support for reunification, and the crumbling Soviet Union (although a reluctant ally whose envoys still sought to play an independent role) sought American aid to prevent the descent into anarchy.

In addition, American officials perceived U.S. allies "to be 'free riding' to some extent in NATO, and they were determined not to let this happen in the Gulf" (p. 60). The chapters on Great Britain and France demonstrate the degree to which the NATO pattern extended to this case. Under Thatcher, the British played the role of the experienced sibling, urging a strong stand in the battle of good versus evil and providing the largest deployment of British forces since 1945. According to Andrew Cooper and Kim Nossal, the "middle powers" (Australia, Canada, and the Nordic countries) joined in the idealist expectation that the coalition was "a harbinger of a new world order, in which the . . . UN would play an increasingly important role, and the U.S. would pursue a policy of multilateralism" (p. 271). They stayed even when realpolitik returned to center stage, because "the costs of leaving the coalition, both domestic and external, were too high" (p. 272).

Mitterand's France was the reluctant ally, attempting to carve out an independent role far beyond its significance and often at the expense of the collective good. Isabelle Grunberg's case study shows how French participation was "marked by a pattern of hesitations, delays, contradictory statements, ambiguous diplomatic moves, and uncertain military alignments" (p. 113). The Arabist French elite preferred to maintain its ties with Iraq, but a combination of public disdain for Saddam's brutality and the perceived need for strengthened ties with the United States to offset the effect of German unification led to limited cooperation.

The biggest problems for the United States were posed by the Arab states that Washington claimed to be protecting from Iraq. In the period after the invasion of Kuwait and before the war began, Bush voiced the fear that the Saudis are "going to be the ones who are going to bug out at the last minute and accept a puppet regime in Kuwait" (p. 46). Yet, once the Americans demonstrated their commitment to attack Iraq, and Saddam Hussein was likely to be ousted, Saudi commitment increased.

Egypt was also a central Arab participant, but as Daniel Brumberg shows, Cairo's policies were based on regional goals, not on a shared threat perception. Egypt's aspirations "required that it join the coalition rather than cede its role to the West—or to other Arab states" (p. 92). The 35,000 Egyptian troops dispatched to Saudi Arabia were not wel-

comed in Riyadh, reflecting the fear that instead of Iraqi dominance, the Gulf would end up subjugated by Egypt. As soon as the war ended, the Saudis rejected the Egyptian offer of longer term "protection," and the troops were expelled. (For its trouble, Egypt obtained an economic package worth $25 billion, largely in terms of debt forgiveness.)

With Egypt involved, other Arab states, such as Syria, followed in order to gain substantial aid, avoid the wrath of the United States, and also check Egypt's power. For the United States, the deployment of Syrian forces, however symbolic, blocked any plans that Saddam might have had to create an Arab alliance on his side. As Haleh Vaziri demonstrates, Iran's behavior as a "free rider" was determined not only by national interest but also by the absence of any viable option.

The greatest weakness in this volume is the absence of case studies of two of the most important actors in the events—Saudi Arabia and Israel. Saudi Arabia is considered in some detail in the chapters on the United States and the Arab states, and primary sources on decision making in Riyadh are difficult to obtain, but no such limits exist with respect to Israel. In addition to providing a "silent partner" and backstop for the combat troops and intelligence activities, Israel's major contribution came when the Shamir government stayed out of the war, despite the Iraqi attacks, and in the face of intense pressure and decades of policy based on preemption and retaliation. Israel was not a free rider and made a major contribution to the coherence of the coalition. The decision against responding was largely the result of Israel's unique alliance relationship with the United States and the expectation of improved security as a result of the destruction of Saddam's arsenal. The inclusion of this case would have added an important dimension to the study.

The authors of this study recognize, long before the latest round in 1998, that the situation in 1991 was an exception, not the beginning of a "new world order." "Because the forces that shaped the Gulf coalition were unique, it is unlikely that the US will have the means to create such a coalition in the future. Instead, it might be obliged to act unilaterally" (p. 109). As this study demonstrates, however, in a unipolar world, once the United States has decided to use its power, even the most reluctant states recognize the high costs of opposition.

Allies Divided: Transatlantic Policies for the Greater Middle East. Edited by Robert D. Blackwill and Michael Sturmer. Cambridge, MA: MIT Press, 1997. 327p. $22.50.

Sara Roy, *Harvard University*

The greater Middle East, an area stretching from the Maghreb to the Caspian Basin, is a flashpoint of world politics and likely to remain so well into the future. For the 15 contributors to this highly informative and extremely well-done work, this increasingly precarious region remains vital to Western interests for three predominant reasons: oil, the proliferation of weapons of mass destruction, and rising Islamic extremism. Yet, shared interests in the Middle East region, more often than not, have failed to produce shared policy approaches, with potentially damaging geopolitical consequences for the nations of the transatlantic alliance.

This failure to agree (i.e., for Western European nations and the European Union [EU] to accept U.S. leadership) became most pronounced with the end of the Cold War and the demise of the Soviet threat, which not only diffused transatlantic solidarity but also challenged the need for unquestioned U.S. political and economic dominance in the greater Middle East and elsewhere. Hence, U.S.-European commercial rivalries in the region, easily managed under the exigencies of the Cold War, have become increasingly difficult to contain. Similarly, the competition over arms sales, needed to support domestic arms industries that were thrown into crisis with the end of the Cold War, grows ferocious. These transatlantic rivalries, coupled with Europe's own effort to develop a distinct identity based on a common foreign and security policy, have further enhanced emerging divisions across the Atlantic.

Allies Divided contains essays by American and European analysts on issues central to regional stability and Western security in the greater Middle East: Israel and the Middle East peace process, the Persian Gulf (with particular attention on Iran and Iraq), Turkey and the Caspian Basin, weapons of mass destruction, and military force projection. The juxtaposition of U.S. and European views on each of these issues reveals a striking discord in policy and strategy among the allies that could, in the collective view of the contributors, damage Western interests in the region as well as the transatlantic partnership itself. Through a systematic and detailed examination of these differences and the reasons for them, which are accompanied by specific policy prescriptions, the volume manages to distill coherently a considerable amount of information and makes a convincing argument for greater cooperation among the Western alliance.

Disagreements between the United States and the European allies are wide and deep and span the entire region as defined. America's commitment to Israel, for example, which is far greater than that of Europe, has shaped the Middle East peace process in ways not always acceptable to the nations of Western Europe and the EU, who feel they have far more at stake given their geographical proximity to the region, heavier energy dependence, stronger commercial ties, and contrasting political views. Moreover, the consistent American refusal to allow the Europeans, particularly France and Great Britain, other than a complementary role in the peace process despite the fact that the EU has been a major financier of that process, has further strained relations. Indeed, the March 1998 visit by British Foreign Secretary Robin Cook to Har Homa, the highly controversial Israeli settlement, rankled not only the Israeli government but also the U.S. government.

Two other critical areas of discord, Iran and Iraq, will also pose major challenges to future U.S.-European cooperation. America's policy of dual containment as applied to these countries has found little if any support among the majority of European nations (with notable exceptions, such as Great Britain), who reject the use of coercion as a method of modifying state behavior. Yet, Europe's own attempt at critical dialogue, a form of engagement that is political as well as mercantile in character, has similarly enjoyed little success. Moreover, the Iran and Libya Sanctions Act, congressional legislation that would penalize European businesses trading with Iran, will do little to heal growing U.S.-European fractures.

Despite its clear merits, the volume may prove irksome to many scholars and analysts. Some key assumptions expressed in the chapters remain unquestioned and are presented as absolute truths when in fact many have been challenged or should be. For example, the Arab-Israeli mideast peace process, as constructed, is seen as a positive formula for peace despite mounting and compelling evidence to the contrary, evidence (which even the U.S. government has been forced to concede publicly) that remains largely unaddressed and that would certainly affect any policy prescriptions for the region.

The volume also maintains that one goal, albeit unstated, of

U.S. policy toward Iraq is the removal of Saddam Hussein from power, when in fact U.S. policy may be committed to just the opposite. Why, then, during the Gulf War did the United States allow Saddam Hussein to crush the Shi'ite rebellion in the south (with the United States refusing to allow any military equipment to reach the regions of southern Iraq) and the Kurdish uprising in the north? If, in fact, U.S. policy is committed to Saddam's presence (in lieu of an "acceptable" alternative) and not his removal, how would this affect transatlantic strategies for the region as well as the region itself?

Another central assumption is the need for far greater convergence in U.S. and European approaches to the region. Yet, why dispense (entirely?) with what, arguably, amounts to a system of (transnational) checks and balances that helps curb the hegemonic nature of U.S. policy in the region in particular? Finally, while the purpose of the volume is to address Western interests in the Middle East and the role of the Western alliance in ensuring those interests, it does so by assuming that the region is the hegemonic domain of the West and that the countries of the region are to be engaged as unequals and often recalcitrant unequals. Although U.S. and European policies are presented as part of the solution for the greater Middle East, they are also part of the problem, having played a very real role in enhancing rather than mitigating extant instability and disorder. At issue is how to address this reality, the solutions to which would contribute mightily to regional stability and Western security as well.

Emergent Actors in World Politics: How States and Nations Develop and Dissolve. By Lars-Erik Cederman. Princeton, NJ: Princeton University Press, 1997. 258p. $55.00 cloth, $19.95 paper.

Miriam Fendius Elman, *Arizona State University*

Although the title may suggest that this is an undergraduate textbook on international relations (IR), Cederman's book is not for the novice. Writing for those already well versed in contemporary IR debates, Cederman provides a sophisticated treatment of nationalism (chaps. 6–8) and the balance of power (chaps. 4–5), and he contributes to a growing IR literature on the origins of the state (e.g., Hendrik Spruyt, *The Sovereign State and Its Competitors: An Analysis of Systems Change*, 1994). Like many of these new studies, Cederman considers states and nations as separate dependent variables, and he criticizes traditional realist and liberal perspectives for treating these actors as "exogenously given" (pp. 4, 214). Also consistent with the literature, Cederman grapples with the extent to which rational choice and materialist explanations can account for institutional outcomes at both the domestic and international levels of analysis. But in terms of method, Cederman departs substantially from other IR studies on the state. *Emergent Actors in World Politics* presents a series of models based on computerized simulations to address the "fundamental question of why states and nations form and disintegrate" (p. 3). This complex adaptive systems (CAS) approach lends a dynamic perspective to the study of nationalist coordination and mobilization in multiethnic states and to balance-of-power theory. In an original twist that distinguishes the book from most current formal applications in IR (pp. 32–6, 48–9), Cederman's models are designed to incorporate features more often considered the bailiwick of qualitative analysis: path dependence, nonlinear and positive-feedback processes, multiple equilibria, historical accidents and inefficiencies, and group identity formation. The models yield a number of counterintuitive results, most important the finding that balance-of-power systems are more stable when offensive technology has the advantage and defensive alliances are jettisoned (pp. 96, 99–100, 105, 109, 112, 215).

The point of departure is the neorealist research program, which Cederman criticizes for ignoring the origins, development, and disappearance of states and nations. To be sure, neorealists usually presuppose the existence of actors and prefer to concentrate on the explanation of international cooperation and conflict (p. 20), but Cederman goes too far when he claims that neorealism lacks a theory of the state (pp. 16–17, 23, 74). For instance, according to Kenneth Waltz (*Theory of International Politics*, 1979, pp. 127–8) the structure of the international system is the generative element that accounts for domestic political development. Domestic organizational features tend to converge because states coact in an anarchical environment. Those states that do not match domestic developments in others will become vulnerable and fall behind. Thus, Waltz expects growing homogenization and uniformity among states. His rudimentary theory of the state can be pitted against an alternative that predicts functional differentiation and the coexistence of radically different political organizations (see Barry Buzan, Charles Jones, and Richard Little, *The Logic of Anarchy: Neorealism to Structural Realism*, 1993, pp. 116–21). Another type of neorealist theory of the state would focus on the effect of malign and benign external security environments on domestic institutional choices. A long tradition in both IR and comparative politics suggests that democratization is precluded when states face severe external threats but becomes a more attractive option when threats subside. By suggesting that decision makers weigh their options based primarily on an assessment of international constraints and opportunities, this "second image reversed" proposition is also consistent with neorealism's hard core. Cederman fails to address either of these arguments.

In contrast to neorealism, Cederman views state actors as Janus-faced, confronted with both international and domestic constraints (pp. 122, 134, 148, 151–2, 217). Specifically, he argues that leaders of multinational states must respond appropriately to external and internal challenges. State-building strategies that devote too much energy to one set of pressures increase the state's vulnerability to external intervention or internal secession (pp. 217, 228). Because state agents need to consider international and domestic consequences, a cautious approach to nationalist mobilization is preferable. Cederman's case studies persuasively demonstrate that state agents who push mobilization too far or too fast provoke national unrest; instead of advocating instant democratization, reformers should use "both the gas and the break" (p. 176). Nonetheless, much of Cederman's argument is consistent with a neorealist reading of international politics, which suggests that decision makers take their cues more from the international environment than from domestic politics. Consider Cederman's explanation for nationalist coordination. Although he argues that in multinational, culturally fragmented states the balance of power between the center and the periphery goes far in explaining the emergence of national identities, he also suggests that the severity of external threats affects whether peripheral identities will become encompassing and inclusive (p. 219). Thus, for instance, Yugoslavism gained more support in Croatia and Serbia as external pressure from Italy intensified. The perception of weakness and fears of external intervention made it easier to "paper over" Croat-Serbian differences and accept a more inclusive identity (pp. 207–8). In addition, Cederman makes the common mistake of equating balance-of-power theory with neorealism (p. 215), thus overlooking

the extent to which his findings are compatible with alternative neorealist theories. In particular, the variability and historical contingency of balance-of-power politics and the prevalence of "hegemonic takeoffs" (chap. 4) is inconsistent with what Waltz's theory would lead us to expect but is perfectly compatible with offensive neorealism and power transition theory.

While Cederman sees his study as a challenge to neorealism, he places it firmly within the constructivist camp. Indeed, his objective is to "combine formal modeling techniques with the insights of sociologically oriented perspectives outside of mainstream IR theory" (p. 6). But despite the insistence that states and nations are socially constructed (p. 19), nonmaterial variables and processes do not figure prominently in Cederman's analysis. In explaining whether the international system will expand or contract, for example, the Emergent Polarity Model contains three independent variables (the proportion of predators, defense dominance, and defensive regional alliances), none of which includes a nonmaterial, normative, or sociological dimension (p. 78). Nor is there any discussion of how new norms of sovereignty and self-determination increase the number of actors in the international system. For example, one possible mechanism is that by reducing the legitimacy of predatory behavior and giving state preservation the status of an international principle, sovereignty becomes a nonmaterial resource for otherwise weak states. Moreover, in Cederman's models, socially constructed beliefs do not play much role in giving internal and external enemies clues about when military action might pay.

Because it aims to convince readers of the merits of CAS modeling, *Emergent Actors in World Politics* adopts a method-driven rather than a problem-oriented position. Cederman focuses as much on the framework as on what is to be studied. One quickly gets the impression that he is at least as interested in showing readers what the CAS model can do—and the variety of IR topics to which it can be applied—as in the problem of nationalism, state disintegration, or balance-of-power politics per se. The result is a book packed with important but somewhat disconnected findings on a wide range of issues, many of which do not receive the attention they deserve (e.g., competition often generates unipolar systems instead of balances of power, p. 106). In particular, the chapters on polarity—a structural variable—appear out of place in a book devoted to the domestic processes of state/nation formation and disappearance.

These points aside, *Emergent Actors in World Politics* is an impressive book that nicely weaves cutting-edge formal modeling and counterfactual thought experiments with a diverse set of real-world examples. Cederman's empirical knowledge is particularly impressive. He effortlessly shifts from historical to contemporary cases, providing insights into nationality formation and nationalist reactions within the Habsburg Empire, the European Union, Yugoslavia, and the Soviet Union. Although not for the IR beginner, experts looking for an intelligent study that goes beyond the typical should find Cederman's book good value for the money.

Promoting Asia-Pacific Economic Cooperation: Perspectives from East Asia. By Yong Deng. New York: St. Martin's, 1997. 194p. $65.00.

Danny Unger, *Georgetown University*

Initiatives aimed at boosting cooperation in the East Asia and Asia-Pacific regions increased in number and significance in the 1980s. Areas previously striking for the relative absence of formal institutional mechanisms to foster cooperation saw a heightened pace of official meetings and the emergence of a host of new institutions culminating in the launch of the Asia-Pacific Economic Cooperation (APEC) group in 1989. These events prompted analyses probing the causes of this sharp upturn in interest in regionalism, its links to Japan's economic rise, the extent to which an incipient regional identity had emerged, and the causal relations between enhanced regional institution building and ongoing economic integration. Deng notes that one result is increasingly sophisticated (Western) analyses of these developments, although most of the more recent contributions fail to give due attention to the perceptions of the Asians who played central roles in driving these processes forward. Hence, Deng centers his analysis on the contributions of the ASEAN countries, China, and Japan.

Deng argues that developing regionalism in East Asia and the Asia-Pacific is striking because of the heterogeneity of the states and the limited roles that dominant states have played in the process. These features pose important theoretical puzzles that international relations theorists have tended to ignore. Deng finds it particularly striking that the outbreak of regime building in the Asia-Pacific coincided with a relative decline in U.S. power. The limited leadership exercised by dominant states underlines the importance, Deng argues, of international regimes and the need to change the ways in which we think about regimes. In particular, we need to give less attention to international structural variables and more to the roles of shared perceptions and common social purpose.

Deng offers an interesting discussion of the regional order under Chinese leadership—the tribute system that waxed and waned over several centuries until overrun by imperialism and the rise of Japan. Consistent with some interpretations of U.S. hegemony after World War II, Deng suggests that whatever the value of trade relations for other participants in the system, in the eyes of Chinese state officials these relations were primarily of cultural, moral, and political significance. The tribute system created an intermeshing of relations among states that featured a flexible assortment of means available to actors within the system in pursuing their diverse ends. These features to some degree foreshadow the nature of contemporary institutions in the region and, in particular, the preferences of most Asian officials in crafting regional institutions.

Chapters on Japan, China, Sino-Japanese relations, and the ASEAN countries comprise the overwhelming bulk of the book. These are clearly presented and offer diplomatic chronologies useful for reference or classroom use. Unfortunately, the material overlaps to a considerable degree, and the intended focus on East Asian actors (or sources) is not particularly strong.

The book has a few weaknesses. In his discussion of APEC, Deng mentions only two (liberalization and economic cooperation) of the institution's three "pillars," ignoring the trade and investment facilitation agenda. Given the limited degree to which the liberalization norm is embraced within East Asia, this last agenda assumes special importance. Deng makes the interesting argument that international structural variables may not offer us the most useful explanation of regime building in the region. More generally, he suggests that the conceptual tools developed to study regime formation in Western Europe may be inadequate to understanding processes in East Asia. Yet, he identifies the literature on regimes closely with the hegemonic stability thesis, despite his frequent use of concepts developed by scholars outside that tradition. Ultimately, Deng distances himself somewhat from the argument that the international structure of power may not be a critical explanatory variable; in his concluding

chapter, he emphasizes the lack of leadership in accounting for the weaknesses of regional regimes. Perhaps the book's greatest weakness is the relative absence of an analysis of domestic politics. Deng argues that state-society relations in East Asian countries vary systematically from those of the West, but he does not develop the potential importance of this assertion as an explanation for distinctive patterns of institutional development. In particular, the Asian emphasis on voluntary (rather than negotiated) liberalization removes the two-level game dynamic from the process. This loss is not great if officials enjoy trade policymaking autonomy. Given the rapid expansion of political participation in most East Asian countries, however, this point seems to merit more attention than it receives here.

Scholars and policymakers have devoted a lot of attention to the alleged tensions within APEC between those ("Western" states) favoring the creation of a formal institution fostering binding, contractual agreements and those (Asian states) opting for flexible and informal arrangements rooted in consensual interaction. However easily subject to caricature, this is a critical issue because it encourages us to consider the relative importance of conceptually distinct (legitimacy and social consensus versus authoritative enforcement) underpinnings of social orders and cooperation. Deng is alert to the issue's importance, but he ultimately does not offer novel insights into the sources of these different emphases, the extent to which such differences in fact exist, or their implications.

This book does not help us much in deciding what kinds of questions we should be asking about the emergence of a social order in the Asia-Pacific region. By what criteria should we decide whether or not it is surprising that cooperation has emerged without significant leadership from dominant states? Has there in fact been enough cooperation or institution building to justify surprise? Is it clear that dominant states have played such modest roles? How should we think about the networks of Japanese firms that have fostered industrialization and expanded investment and trade flows across the region? Do these developments have anything to do with the Japanese state itself? Deng's emphasis on the interaction of economic and security issues in the region encourages us to ask whether the role of the United States has been as limited as his discussion suggests. Similarly, the heavy dependence of the East Asian states on external actors—the International Monetary Fund and the United States—in the wake of financial and foreign exchange crises beginning in 1997 leads us to ask just how much of an anomaly the region presents to us.

Keeping the Peace: Multidimensional UN Operations in Cambodia and El Salvador. Edited by Michael W. Doyle, Ian Johnstone, and Robert C. Orr. Cambridge: Cambridge University Press, 1997. 428p. $64.95 cloth, $24.95 paper.

Conflict Neutralization in the Cambodia War: From Battlefield to Ballot-box. By Sorpong Peou. New York: Oxford University Press, 1997. 358p. $60.00.

Casualties of the New World Order: The Causes of Failure of UN Missions to Civil Wars. By Michael Wesley. New York: St. Martin's, 1997. 200p. $65.00.

Thomas G. Weiss, *Brown University*

Scholarship is no less trendy than the culinary arts, although fads relating to the former tend to take longer to gain momentum and longer to fade away. Despite the dramatic decrease in U.N. operations since the mid-1990s and their falling popularity, literature about them is continuing to proliferate. The initial enthusiasm accompanying the first wave of new U.N. peace operations, beginning in 1987, led to a flood of publications. In an English-language database of pertinent articles, book chapters, books, and reports published from 1989 to 1996, there are 2,200 entries that vary immensely in quality and sophistication (Cindy Collins and Thomas G. Weiss, *An Overview and Assessment of 1989 to 1996 Peace Operations Publications*, 1997; also http://www.brown.edu/Departments/Watson_Institute/peace operations/index.html). Much of the supply of this literature was, and continues to be, driven by demand. Funding for studies has emanated from governments and foundations; and authors (ranging from seasoned analysts to graduate students) who might have been involved in less financially rewarding and unpopular areas of interest have turned their attention and computers to this arena. The ease with which expert status was attained led to a rush to get publications into circulation. A decade later, however, we should expect more reflective, structured, and comprehensive analyses and fewer broadly gauged prescriptions and descriptions.

These three books aim to update our knowledge about completed U.N. operations of the late 1980s and early 1990s; the only case found in all three books is Cambodia, although El Salvador figures prominently in *Keeping the Peace* and *Casualties of the New World Order.* Neither *Conflict Neutralization in the Cambodia War* nor *Casualties of the New World Order* offers a compelling argument for what they purport to discuss. *Keeping the Peace*, however, is carefully crafted and merits serious attention from scholars and practitioners.

Neither *Conflict Neutralization in the Cambodia War* nor *Casualties of the New World Order* specifies clearly enough the basis for judgments about "success" or "failure." The authors do not clarify adequately the inescapable value judgments, differing time frames, and multiple objectives used by various actors, or the hidden agendas that are the essence of world politics. In examining critical peace operations in the last half decade, for instance, one could legitimately ask: Have military efforts in northern Iraq been a success because 1.5 million Kurds were saved and are still alive, or a failure because without a political solution Saddam Hussein is ensconced in Baghdad and flaunting international decisions? Were military operations in Somalia successful because death rates dropped in 1993, or a long-term failure because billions were spent to stop the clock temporarily? Were U.N. military efforts in Bosnia in 1992–95 successful because they saved lives and avoided a wider conflict in Europe, or a failure because the community of states did not stand up to aggression, war crimes, and the forced movement of peoples and chose a type of intervention that may have perpetuated the conflict? Were military and police operations to restore President Aristide in Haiti a success because a precedent against a seizure of power by a junta was set and peaceful elections for his successor held, or a failure because fundamentally skewed economic structures remain in place to exploit the vast majority of the population? Were military efforts in Rwanda in 1994 a success because the carnage was stopped and lives saved, or a failure because the genocide took place and violence spread to new theaters?

Answers to such questions can be affirmative or negative, depending on an analyst's criteria. Readers of the books by Peou and Wesley will have difficulty with the characterizations of success or failure. These published versions of doctoral dissertations add little to contemporary scholarship or policy analysis.

Conflict Neutralization in the Cambodia War is yet another case study about the United Nations Transitional Authority in Cambodia (UNTAC). Peou, of Cambodian origin, mixes first-hand interviews with secondary sources along with un-

derstandable but regrettable passion in his treatment of the war and the subsequent U.N. presence. He develops the notion of a psycho-structural approach called "conflict neutralization," which "is defined as a political process whereby military conflict can be rendered inactive by a legitimate authority (a third party) through democratic means" (p. 10). He is trying to avoid the possible paralysis that would result from taking adequately into account the legitimate criticisms by the two main types of observers who have lamented U.N. failure: the "socio-cultural institutionalists," whose pessimism is based on the idea that outside imposition of western-style democracy on a country like Cambodia is bound to fail because there are no preconditions or cultural affinities; and the "legalists/moralists," whose pessimism is based on a more cogent argument that U.N. efforts did not punish Khmer Rouge war criminals and even included them in the peace process. The author immodestly strives not only to "make the world safe for democracy, but also sow the seeds of democracy in order to make the world more secure for humankind" (p. 10). He seeks a middle ground between consent (which paralyzes the U.N. and also does not address the insecurity dilemma of belligerents) and military coercion (which has the disadvantage not only of reflecting the Permanent 5's foreign policy interests but also often of leading to more violence). Insights are less numerous than platitudes, such as the book's final paragraph (p. 304): "Member-states must consider doing more rather than becoming increasingly cynical and doing less.... Although it will never be in the position to create a strife-free Utopia, the UN is the only global body with the best potential to keep its members thinking collectively about what they can do to prevent hell on earth from breaking loose."

Casualties of the New World Order is an unsuccessful attempt to discover what the book's subtitle describes as *The Causes of Failure of UN Missions to Civil Wars*. Wesley is unpersuasive in explaining why he considers U.N. mediation in El Salvador a success but a failure in Bosnia, peacekeeping is useful in Mozambique but not in Somalia, and election monitoring is sensible in Cambodia but not in Angola. But a more significant shortcoming is the comparison of unlike cases. Conflicts that predated the end of the Cold War are compared with ones that arose afterward, and traditional "peacekeeping" in Mozambique is compared with enforcement in Somalia. From this lack of clarity emerges the unsurprising explanation that "coalitions" (by which the author means "troop contributors" and not the "coalitions of the willing" that have been used in the Gulf or in Liberia on "subcontracts" from the U.N.) determine mandates and the shape of military forces, and the "failings of UN missions derive from significant structural weaknesses that lie within the mission and their method of dispatch by UN member-states" (p. 1). On the same page Wesley sees a particular problem in the present generation of intrastate conflicts because "the failures of UN missions are attributable to common structural weaknesses that are especially vulnerable to resistant civil war conflict dynamics." That the missions, mandates, and force postures of U.N. operations reflect the foreign policy interests of the member states of an intergovernmental organization, particularly those that contribute soldiers, should not come as much surprise. Indeed, such logic is remarkably similar for international or noninternational armed conflicts. Wesley is off the mark in arguing that diverse and contending perceptions of issues and vital interests, along with the desire to minimize costs and commitments, are phenomena that influence state contributions after the Cold War's end. They have always been the essence of international decision making about U.N. operations.

Although publishers and promotion committees dislike edited volumes, books like *Keeping the Peace* should give them pause. This is not a collection of unrelated chapters tied together only by a common cover; rather, it is a carefully designed and persuasively argued comparative analysis of U.N. operations in Cambodia and El Salvador. This book is superior to most single-author volumes because of the specialized knowledge and expertise brought to bear by a combination of academics with practical experience and practitioners with analytical bents. This is crucial because too few scholars go to the field as participants in peace operations, and too few practitioners write about their experiences. The comparison focuses on two examples of "multidimensional peace operations." And despite obvious differences between the cases, the discussion of Cambodia and El Salvador provides comparable data. The editors selected the middle ground between traditional peacekeeping and enforcement largely on prescriptive and policy-relevant grounds—because "traditional peacekeeping and peace enforcement, are less likely to shape the UN agenda in the years ahead" (p. 3). At the same time, this selection has important methodological and conceptual consequences, which make the conclusions comparable across the two cases. Parallel and, for the most part, very high-quality chapters focus on, respectively: the roots and development of the two conflicts; mediation; legal perspectives on peace agreements; maintaining law and order; central civilian administrative functions; and key humanitarian, economic, and social dimensions of peace processes. On the question of success and failure, the editors devote part of their concluding chapter to justifying their "unscientific... [and] eclectic approach" (p. 370) and to examining their justifications for the mixtures of success and failure in both Cambodia and El Salvador. Some of the conclusions are important and straightforward (for example, the importance of an agreed settlement); others are surprising and little documented (for example, the possibilities for enhancing consent through such mechanisms as the "friends" of the country in crisis); and others are counterintuitive (for example, the importance of multidimensionality—or having eggs in several baskets—so that "failures of specific aspects of the program are not enough to destroy the entire mission" [p. 387]). In short, this insightful volume adds to the literature and merits careful perusal.

A final word is in order about a notion that explicitly or implicitly appears in all three volumes as well as in numerous recent conferences. "Lessons learned" has become something of a buzzword in post–Cold War parlance. The rush to create lessons-learned units at the United Nations and elsewhere, however, is not to be confused with the learning of lessons. To the extent that many of the so-called lessons remain relegated to file drawers, coffee tables, or books, the concept of learning is perverted. The assignment is to identify and examine not only key issues and events that have challenged and sometimes paralyzed peace operations but also creative institutional responses that actually have been institutionalized. Organizational learning, like individual learning, occurs in three stages: identification (when problems are observed and data collected); diagnosis (when information is analyzed and discussed, including causes, earlier responses to similar problems, and underlying beliefs and assumptions); and implementation (when revised policies and procedures are institutionalized).

Clearly, well informed is preferable to shallow identification and diagnosis. Whether one agrees with this reviewer's judgments about these volumes, the identification of problems and the formulation of possible solutions are necessary but insufficient without concrete implementation measures

that address identified and diagnosed problems. If the resources or the will to take action are missing, learning has not taken place. In this respect, there has been precious little "learning" by governmental, intergovernmental, or nongovernmental organizations regarding U.N. peace operations, despite the so-called lessons compiled in *Keeping the Peace* and elsewhere.

Taiwan's Security in the Changing International System. By Dennis Van Vranken Hickey. Boulder, CO: Lynne Rienner, 1997. 233p. $49.95.

Wei-chin Lee, *Wake Forest University*

Taiwan has moved by leaps and bounds toward market liberalization and political democratization since the late 1980s, but its security vis-à-vis China has remained at the forefront of international concern. Not only might mischief or miscalculation on either side of the strait cause regional instability in East Asia, but also the consequences would surely pose a daunting challenge to U.S. global security regime design. There is a pressing need to examine the issue of Taiwan's security thoroughly, and Hickey has responded with a broad study of the situation since the end of the Cold War.

In Hickey's view, post–Cold War structural changes mean that the "China card" has lost its magic in containing the defunct Soviet Union. A kinder, gentler world has created opportunities for Taiwan to enhance its security in a radically different strategic environment. Arms acquisitions, primarily from the United States, have become less restricted. Taiwan's pragmatic diplomacy has more maneuvering room and reaches a more receptive audience. Most important, freed from the straitjacket of the Cold War, the United States has shown the courage on several occasions to say "yes" to requests from Taipei which would have received a flat "no" in the past. Nevertheless, the euphoria generated from a friendly international system has been cut short by hostile domestic political developments in China. With its historical claims over Taiwan, staunch nationalistic sentiment, and unending leadership power struggles, China seems determined to achieve its goal of unification with Taiwan at any cost. So, as Hong Kong fever cools, the heat increases on Taiwan. But unlike Hong Kong, which had few chips on the bargaining table, Taiwan is a formidable player.

The strength of this book is its broad conceptual framework of national security. In relation to a face-off with China, Taiwan's national security is examined by incorporating both military and nonmilitary dimensions. In addition to the usual considerations of structure, composition, arms acquisition, and defense planning by Taiwan's military forces, this study treats the ramifications of continuous democratization and growing economic clout, the consequences of diplomatic efforts, and the interplay of triangular relations among Taiwan, the PRC, and the United States. In line with the recent trend of linking domestic politics to international relations, Hickey also provides readers with a brief overview of the Taiwan independence movement and explains how, in its extreme forms, this movement undermines Taiwan's security.

Ultimately, Hickey suggests that the United States should maintain the status quo in keeping with the terms of the Taiwan Relations Act of 1979 by ensuring the island's adequate defense capability. While the United States should actively support Taiwan's membership in nonpolitical intergovernmental organizations to help dampen sentiment for de jure independence from China, "it is *not* in the U.S. interest to promote, assist, or otherwise encourage Taiwanese independence" (p. 196). Furthermore, Hickey argues the United States should avoid becoming a mediator or guarantor of any future agreements between the PRC and Taiwan, except to encourage bilateral dialogues. Hickey points out that it would be a mistake for Taiwan to expect the U.S. cavalry to ride to the rescue in a crisis, although some Taiwanese politicians would disagree with this assertion.

It is undeniable that among players in the delicate U.S.-China-Taiwan triangle, Taiwan exercises the least influence. What might be good for the United States is not necessarily good for Taiwan, meaning that the island is potentially expendable when U.S. security and global structural changes become overriding issues. Both Taiwan and China benefited with the burial of Cold War rhetoric. The move toward a multilateral approach to conflict resolution has given China leverage and a role to play in global affairs. This new status enables it to pressure other nations, including the United States, into accepting China's policy toward Taiwan.

Hickey must be commended for attempting to present a full picture of Taiwan's security. At some points, however, his treatment is not extensive enough, and he falls somewhat short of illuminating the significance and complexity of an issue. For example, security policy options, decisions, and strategies are closely associated with the perceptions, capabilities, and credibilities of all players involved. Thus, while this book treats Taiwan's own perception and assessment of its security and the U.S. evaluation of the balance of power across the strait quite fully, it forgoes a similarly extensive analysis of China's perception and the debate among its domestic elite. In contrast to the copious citations of and quotations from documents and interviews with Taiwanese officials, sources from China are relatively slim. China is, after all, the primary threat to Taiwan. Likewise, given that "Taiwan is probably the noisiest democracy in Asia" (p. 105), readers may wish that Hickey had explored the independence movement at greater length, particularly its internal bickering and the nature and actions of its various parties, factions, and elite groups. On this issue, even President Lee Teng-hui's statements have often generated different readings and responses. More analysis would have helped readers appreciate the complexity of this crucial issue and the challenge it presents for U.S. policy.

Readers will learn a great deal about U.S. efforts to assist Taiwan's military modernization from Hickey's wealth of data and information. Arms transfer has been crucial to Taiwan's security. Yet, one cannot resist asking why Taiwan had to purchase weapons from sources other than the United States when it was fully aware of the U.S. criticism of "operational liability," that is, of weapons incompatibility if arms are procured from different suppliers. If the United States had been actively involved in Taiwan's indigenous defense fighter (IDF) design and production, then why was it described as the world's most expensive jet fighter and the IDF ridiculed as meaning "It Don't Fly"? If Taiwan is really operating in a buyer's market as most people claim, then why did it lease U.S. Knox-class frigates, which are refurbished and updated "old-timers," instead of buying new ships? Why did Taiwan have to pay higher prices than other buyers for French LaFayett-class frigates, complete with unexplained "extra" fees tacked on? The suspicion arises that foreign suppliers are playing one side off against the other, charging Taiwan a premium for arms, while keeping up their cozy, cordial relationship with China. An analysis of these international intrigues might not alter Hickey's conclusions, but it would shed more light on the effect of global structural restrictions upon Taiwan's security vis-à-vis foreign arms suppliers.

Despite such minor quibbles and regrets, it can be said that this book is a nice addition to the study of Taiwan. Viewing Taiwan's security across such a broad spectrum is admirable, and anyone seeking to understand the issue of Taiwan's security and the uneasy tango that China and the United States will dance in the future will find this book a useful reference. The United States has a strong commitment to Taiwan arising from its long-term historical ties, legal obligations, economic incentives, and abiding interests in promoting and preserving democratic values. If Taiwan ever decides to declare formal independence through democratic means, what would the United States do? Hickey's study provides the basis for formulating an answer to this and other provocative questions about Taiwan's future.

Imagining War: French and British Military Doctrine between the Wars. By Elizabeth Kier. Princeton, NJ: Princeton University Press, 1997. 240p. $39.95.

Robert A. Doughty, *U.S. Military Academy*

Despite the title, Elizabeth Kier's book is less about French and British military doctrine between the wars than about the influence of "culture" on the development of doctrine. Since military organizations, she argues, develop strong collective understandings about their role and missions, their cultures influence doctrinal development. To prove this point, she begins by analyzing Barry Posen's and Jack Snyder's studies on doctrine, and she questions whether "balance-of-power theory" adequately explains doctrinal development and whether military organizations inherently prefer offensive doctrines. She then reviews French and British interwar experiences to demonstrate how military cultures influence doctrine, and not surprisingly she concludes that its shaping is best understood from a cultural perspective.

The greatest significance of this book is that its central ideas should replace simplistic notions about doctrine that currently exert great influence. Specifically, some military historians have long understood the complexity of doctrinal development, particularly the uniqueness of individual armed forces' experiences and the wide variety of factors influencing those experiences, but other military historians and political scientists have favored simpler explanations. Hence, Kier's argument is a welcome antidote to the prevailing but inadequate models, and it is hoped that the book will lead more scholars to acknowledge the complexity of doctrinal development.

The main strength of the book is the author's extensive research. Instead of relying excessively on secondary materials, she has conducted archival research in Paris and London and has reviewed many of the contemporary military journals and periodicals germane to her subject. Her impressive command of the literature also is evident in the lengthy bibliography. Though Kier could be criticized for seeking only evidence that supports her thesis, her extensive research not only adds credibility to her argument but also provides her the insights necessary to understand and convey the intricacies of a complicated subject.

Notwithstanding the considerable strengths of this book, it has several weaknesses or points with which an historian could disagree. First, though Kier carefully explains what she means by culture and organizational culture, she never clearly defines doctrine. Even worse, her usage of the term sometimes suggests she is considering strategy instead. The distinction is important because the French and British had different approaches to doctrine—both in role and content—during the interwar period and because recent authors have sometimes folded a wide variety of incongruous ideas under the broad umbrella of doctrine. The importance of a definition is most evident from the variety of approaches and definitions current among the U.S. armed services and Joint communities.

Kier also lapses into jargon that most historians (and perhaps some political scientists) would not understand. When she investigates, for example, whether "culture has causal power," she asserts: "Some of the recent work adopting a constructivist approach argues that certain epistemologies are inappropriate for examining the role of intersubjective factors" (p. 33). Such sentences not only lessen the chances that historians will find much value in her book but also weaken her argument by obscuring her points beneath a cloak of code words which have meaning only to her intellectual community. In particular, portions of the book dealing with theory contrast sharply with the clear, coherent, and persuasive writing in the four chapters focusing on historical events in France and Britain.

Of the several points with which one could disagree, a particularly important one pertains to the nature of offensive or defensive doctrine. Throughout the book, Kier argues as if the two were not interrelated. Her tendency to blur the distinction between strategy and doctrine adds to her willingness to place an intellectual firewall between offensive and defensive doctrines. In reality, neither Britain nor France consciously "chose" a doctrine that was solely offensive or defensive. Instead, they emphasized one or the other but retained essential elements of both. In France, doctrine envisaged first the weakening of an attack by a defender's fire and then destruction of the enemy by a massive and tightly controlled "methodical battle." To see this view as purely defensive is to distort the outcome of a very long and elaborate process.

One also could disagree with Kier's insistence that French politicians deferred to the military on questions of doctrine. Though the French obviously emphasized the offensive more in the 1920s than in the 1930s, the military did not have the freedom to adopt a solely offensive doctrine in the 1930s. The key restraint appeared in the late 1920s, when several laws on the organization and employment of the army assured the dominance of reservists and conscripts in any future war. Politicians, however, were not the only ones who supported these laws; General Eugène Debeney, the chief of staff during the late 1920s, later acknowledged proudly his significant role in creating "this solid infrastructure" for the army. In reality, France emphasized an offensive strategy in the 1930s because of Germany's weakness, but as that weakness dissipated, neither political nor military leaders in France favored an offensive strategy. Kier insists that "the French army could have adopted an offensive doctrine if it had wanted to" (p. 73), but the combination of the 1927–28 laws, vulnerability of crucial natural resources on the frontier, and experiences of World War I ruled out that possibility. Thus, the French army's organizational culture influenced its development of doctrine, but public laws, previous experience, and ongoing experimentation also shaped doctrine and strategy.

Despite these reservations, Kier's book is a welcome and important addition to the growing body of literature concerned with doctrine. The high quality of her work reflects not only a keen understanding of the doctrinal process but also a willingness to investigate the archival materials essential for such an understanding. Let us hope future works will rely on such materials and also will avoid the vocabulary that have meaning and utility for only a narrow group of scholars.

Britain and the Politics of Modernization in the Middle East 1945–1958. By Paul W. T. Kingston. Cambridge: Cambridge University Press, 1996. 191p. $54.95.

Robert Vitalis, *Clark University*

Paul Kingston's study of the operations of the first British bilateral aid mission in the Middle East, the Development Division of the British Middle East Office (BMEO), raises problems of value and method in the historical comparative study of development. He does this explicitly by framing a history of this little-known postwar British development agency as a critique of emerging development assumptions of the 1940s and 1950s. These beliefs, which mix political self-interest and selflessness, rely on the big fix and take relatively little notice of local characteristics in designing and implementing programs from the top down; unfortunately, Kingston argues, they still shape thinking about development today, across the divide between neoliberals and many of their critics. Kingston sees in the history of the Development Division an early proof of the validity of some of the alternative local capacity-enhancing "small is beautiful" tenets that would later emerge within the aid industry. He treads carefully yet is unable to avoid the anachronism trap in an argument that reads the past backward in terms of policy debates and conflicts that were not part of the era. Doing so lets him make a modest case for the relative "success" of this now extinct agency. The odd turn of argument also means, however, a missed opportunity to engage multiple historical comparative debates about the course of empire, decolonization, and state and market building for which the period does represent a critical juncture.

The emergence of the BMEO as a precocious institutional pioneer in small-scale, local capacity-building development strategy is an accident of history. According to Kingston, Labor Party foreign minister Ernest Bevin originally envisioned an ambitious postwar initiative in the empire's Middle East dependencies; it would draw on the allies' wartime experience with coordinating the regional economy and would be centered on infrastructure investment and land reform. This initiative was intended, not surprisingly, to shore up the political influence of a declining imperial power by promoting a degree of socioeconomic reform (a "peasants not pashas policy") and, not least, to compete with the American engineering and heavy industry exporters in these markets. As Kingston documents, this Labor Party "version of the 'white man's burden' " never stood a chance against the opposition of Treasury, which refused to fund the initiative, the inability of hard-strapped British firms to supply the goods, a shortage of technicians to staff it, and resistance from Egyptian and other nationalists. Without the ability either to supply the capital on which infrastructure development depended or to persuade the Americans to underwrite the British initiative, there was little choice but to scale back plans, concentrate on reliable clients (Iraq, Jordan), and turn to places like Cyprus, where rival foreign aid agencies were less interested.

Kingston uses case studies of BMEO's work in Iran, Iraq, and Jordan to show how a small committed cadre of British technicians was paradoxically liberated from both political "shackles" and the rapidly emerging orthodoxy of big is beautiful to the ultimate benefit of local institutions. This alternative model looks even better, Kingston insists, when compared to the record of U.S. Point IV aid or U.N. programs in the Middle East. The initiative in the late Truman era suffered the combined flaws of TVA-sized imaginations, Americans' antistatist bias, and a no less rigid understanding of how to implement inappropriately designed projects. The account ends in 1958, at a point when Britain's efforts to salvage its influence in the region's political and economic order, which had already been set back by the debacle of the 1956 Suez War, suffered the further loss via revolution of a client regime in Iraq. In a brief concluding note, Kingston tells us that the potential lessons of these years were lost as official aid efforts in the 1960s and 1970s turned in the more conventional direction of subsidizing sales of British goods and services, before budget cutbacks led the Thatcher government to disband the Middle East Development Division in 1981.

Among the many strengths of this book, Kingston mines the declassified records of the British and, to a lesser extent, U.S. governments for material on the political economy, an integral dimension of the historical process that diplomatic historians, who routinely use such archives, tend to overlook. He also extends his search to archives, notably of both the BMEO and of Point IV (formally, the Technical Cooperation Administration), that remain woefully unexplored. The result is an original contribution to the historiography of decolonization. The chapter on Jordan is particularly fine. The author's careful and sober judgment extends to recognizing that his work remains open to (serious) revision, given its heavy reliance on British documents, relatively scant source materials in Arabic or from within the region, and a more cursory treatment of the American side. More critical still, Kingston recognizes that the makeshift strategy of the British technocrats in the 1950s has no real historical connection to the radical strains of populism and romantic anticapitalism that actually fed the small-is-beautiful critiques of the 1970s, or to the latest 1990s wave of grass-roots-oriented development strategies.

Acknowledging these complications does not, however, solve the problems created by the author's "presentism" or the choice to write the history as if these debates were foreshadowed in, and somehow settled through, this study of contrasting British and U.S. aid programs. The author does not provide a test of the alternative top-down and bottom-up models, for example, by attempting to trace the institutional or local capacity-enhancing effects of each. (He concedes that the small-scale British projects had no effect on income growth or poverty reduction.) Kingston's evaluation is ultimately tautological. Small-scale, bottom-up projects are by definition better at enhancing institutional capacity; the BMEO's approach was small-scale; hence its efforts were a success.

Kingston has missed a chance to integrate his work with other studies of decolonization and development in the early postwar years and to contribute to the multiple, ongoing debates in historical comparative theory that center on this era. For example, his findings challenge those who argue that British imperial power continued to "control" and "dominate" the economies of Egypt, Iran, Iraq, and so on; as Kingston shows, the Bevin government was unable to launch much less consolidate the development initiative on which its postwar position in the region depended.

His work matters as well to our understanding of the real clash of ideas and interests in the early postwar development policy arena. Kingston shows much less familiarity with the evolution of discourse and action in the 1940s than in the 1990s. The author notes without comment that the BMEO showed little interest in developing manufacturing capacity in Iraq and Jordan in the 1940s and 1950s. If true, then this "progressive" postwar British initiative in local development was a reversal rather than extension of the wartime efforts, under joint Anglo-American direction, to create import-substitution industries in the Middle East. Kingston's inter-

pretation of American development ideas and the history of Point IV is even more flat and one-sided (not least because he relies so heavily on British sources). His positive view of BMEO depends in part on the contrast with the Americans' allegedly powerful and visceral antistatist bias. Yet, the Point IV archives document active and regular U.S. support for building state-owned infrastructure and import-substitution manufacturing in the region, consistent with the evolution of U.S.-backed ISI programs outside the Middle East in the same period.

Regional Orders: Building Security in a New World. Edited by David A. Lake and Patrick M. Morgan. University Park: Pennsylvania State University Press, 1997. 406p. $55.00 cloth, $19.95 paper.

Benjamin Miller, *Hebrew University of Jerusalem*

With the end of the Cold War there is a strong impression that processes of globalization have become very powerful. At the same time, some analysts have noticed, quite to the contrary, increasing regional variation, especially the regionalization of security, rather than global uniformity. The Lake and Morgan volume on regional orders is a major new contribution to our understanding of the great variety of regional orders emerging in the post–Cold War era. Together with some other recent volumes on regionalism, it serves as a useful antidote to purely systemic analyses and especially to the widely assumed increasing globalization of world affairs. The authors demonstrate forcefully the centrality of the regional level for understanding key developments in the security field in the post–Cold War era. At the same time, this first-rate volume goes much beyond an analysis of the uniqueness of different regional systems. In contrast to many books on regional security, which tend to be mainly descriptive or policy relevant, it makes an important theoretical contribution by linking useful general concepts and theories from the international relations field with crucial questions of regional security.

The volume develops the conceptualization of a region, presents a typology of regional security orders, and identifies key independent variables that explain important aspects of regional security. Yet, there is still a long way to go toward the development of a coherent theory of regional orders, especially one that makes sense of the linkages between the global system, on the one hand, and regional/domestic factors, on the other, and their combined influence on regional war and peace. Indeed, without a theory of regional war and peace, it is difficult to evaluate the utility of different conflict-management mechanisms and the conditions for their effectiveness.

The first theoretical contribution of the volume is the development of the concept of a regional security system. In this context, David Lake introduces the idea of externalities (or transborder "spillovers") as producing the strategic interactions central to a regional system. This important concept enhances our ability to identify the boundaries of a certain regional system. It is more useful than pure geographic criteria and less problematic and subjective than the constructivist criterion of regional identity and consciousness. At the same time, the externality notion by itself does not go beyond identifying a regional system and predicting the intensity of the interactions within it. It is unable to explain and predict what will be the nature of these interactions, namely, whether a regional system will be cooperative or conflictual, inclined to war or peaceful.

A second useful contribution of the volume is the development of the dependent variable—the classification of regional security orders. Patrick Morgan distinguishes between the classical management of conflict by balancing power and the more collaborative attempts at security management: a great-power concert, collective security, a pluralistic security community, and regional integration. Additional insightful contributions to this typology are made by Paul Papayoanou, who focuses on different types of great power regional intervention, and Brian Job, who discusses the role of multilateral security arrangements, including regional institutions. Rosecrance and Schott elaborate on the mechanism of concerts of great powers. These distinctions regarding security management are a necessary condition for an understanding of regional security. Yet, in order to evaluate their effectiveness and durability, it is necessary to identify the theoretical conditions under which different regional orders are more likely to emerge and endure.

In this regard, the volume identifies three major determinants of regional orders. The first is the structure of the regional system (unipolar/hegemonic, bipolar or multipolar). The second is the international system, including how the area was influenced by the Cold War and its termination (an excellent chapter by Stein and Lobell) as well as the current motivations of the great powers for regional intervention (a first-rate chapter by Papayoanou). The third is domestic political coalitions; most notable in this regard is Solingen's innovative work on liberalizing coalitions, which produce benign/cooperative regional orders, as opposed to statist-nationalist and radical-confessional domestic coalitions, which produce regional instability and conflict. (Israeli Premier Benjamin Netanyahu poses an interesting anomaly: He is both an economic liberalizer and a hard-line nationalist.)

The three key independent variables are significant but problematic. The argument that regional multipolarity is conducive to more cooperative security complexes, even if only in some regions (Africa, East Asia, the Middle East, and ASEAN), seems misplaced and overstates the influence of the regional structure. It makes much more sense to attribute the progress in regional cooperation in these areas either to external influence by great powers (notably the end of the Cold War and U.S. hegemony in the Middle East) or to nonstructural domestic-regional factors (ASEAN and Western Europe). Indeed, the extent of the legitimacy of the regional order, namely, acceptance of the existing territorial order, may provide greater explanatory power. The extent of legitimacy, in turn, depends on domestic-ideological attributes and orientations of the regional states (notably, the power of revisionist ideologies and the degree of state coherence or strength in the region). This only partly overlaps with Solingen's important concept of liberalizing coalitions. Questions for further research are whether such coalitions are a necessary or a sufficient condition for legitimate regional orders, and what are the precise relations between liberalizing coalitions and the democratic peace theory.

The volume also understates the central international factor—U.S. hegemonic leadership in the post–Cold War era. Although, as various chapters suggest, the end of the Cold War opened the way for the evolution of a number of systemic possibilities (such as concert, balance of power, hegemony, and collective security), the hegemonic option has been especially prominent. Rather than a great power concert, a partial unipolarity or U.S. hegemony has emerged since the collapse of the Soviet Union with regard to many regions, notably East Asia, the Middle East, Central America, and the Balkans. At the same time, the volume exaggerates the extent of globalization of regional security during the

Cold War; thus, there is altogether an overstatement of the extent of the change since then. Rather, both types of factors—international and regional—affect regional security in every period, and the challenge of theory is to specify which type of regional security outcomes are best explained by global factors and which type are best accounted for by regional/domestic causes. The theory should also assess the combined effects of the global and regional/domestic factors on regional orders.

To sum up, the general problem of this book, which is typical of many other good edited volumes, is the lack of a coherent theory or a well-integrated theoretical framework to address the general topic, in this case the origins of regional war and peace. Such a theory should not only present causal linkages between global versus regional/domestic factors and variations in regional orders but also explain the effects of global and domestic causes on the extent of regional war proneness. Without understanding the causes of regional wars, it is difficult to know the most effective ways to reach regional peace and to manage conflicts. Thus, such a theory could also be helpful in evaluating the effects of various global and regional mechanisms on the management of regional security.

Notwithstanding this shortcoming, this important volume is a major theoretical addition to the growing literature on regional security. It highlights the rising importance of regional questions for the international relations field in the post–Cold War era, and it deserves a wide audience of both international relations theorists and area specialists.

Anatomy of Mistrust: U.S.-Soviet Relations during the Cold War. By Deborah Welch Larson. Ithaca, NY: Cornell University Press, 1997. 329p. $35.00.

Alexander Dallin, *Stanford University*

The central thesis of this thoughtful and well-researched volume is "the claim that the United States and the Soviet Union, if not for mutual mistrust, could have reached several significant agreements on security issues [during the Cold War] and that, through sustained effort, they could have overcome such suspicions" (p. 3).

Larson examines five cases—at what she considers "branching points" in the superpowers' relationship—to make her case. They do not include the years commonly seen as the origins of the Cold War but, rather, begin with the immediate aftermath of Stalin's death. The Eisenhower years and the German question figure prominently. The Berlin Wall and Cuban Missile Crisis as well as the arms control negotiations under JFK are the subject of another chapter. A further case covers the Nixon years and the SALT negotiations. Finally, the Reagan-Gorbachev period is the subject of another careful study. One can disagree, now and then, with Larson's allocation of responsibility for failure to improve relations—improvements whose payoff, she implies, would have been above all an increase of "trust." But the story of each "case" is well told, with a fine knowledge of documentary and memoir literature. True, with every passing year new materials appear, and the White House tapes made during the Cuban Missile Crisis and the various recent Russian documents and memoirs would serve to distribute the accents a bit differently. No matter, the major thrust of her analysis is not affected thereby.

Positing that distrust is a key cause of failure, the author sets out to consider three alternative explanations for it: rational choice, domestic politics, and psychological factors. Finding the first two unsatisfactory, she disaggregates the latter into ideological differences between the partners, historical baggage, and intuitive mental biases. While the case studies are not greatly illuminated by "intuitive mental biases," they are present, clearly and importantly.

A more serious difficulty concerns the concept of trust and its role. Larson tends to equate missed opportunities with mistrust. On the one hand, to pinpoint missed opportunities requires identifying what was "objectively" possible—a very dubious, iffy, and subjective judgment. On the other hand, not all missed opportunities can be attributed to lack of trust. More fundamentally, even leaving aside the questions of defining trust and how much trust is needed in what situation, there were clearly a good many instances in which (for both sides) trust was not in order. The Cuban Missile Crisis of 1962 is a good example—and there are many others—of a situation in which there was not any basis for trust, given Khrushchev's prior assurances. Furthermore, a lack of trust need not be an obstacle to negotiations nor to agreements or treaties if sufficient transparency is built into the agreement or the risks are perceived to be minimal. Indeed, is trust not always conditional, partial, intangible in foreign (as in domestic and interpersonal) affairs? Foreign policy, it can be argued, should always be based on the assumed possibility of justified mistrust. The whole U.S. insistence on on-site inspection in arms control and other forms of verification was based precisely on that assumption; alternating unilateral moves require no significant trust (though confidence-building measures are obviously desirable); and Larson fairly cites the work of scholars such as Robert Axelrod and Charles Osgood to explain the argument.

Agreements between the superpowers became possible not because trust appeared between them but because one side (or both) perceived a change in the other's calculus or behavior. Why was progress on the German question impossible in the 1950s but possible forty years later? It was not because of the miraculous appearance of trust but because of a fundamental change in Soviet behavior, if we compare 1953 with 1989. Trust did not appear suddenly in 1963 when John F. Kennedy delivered his American University speech, yet that changed the atmosphere sufficiently to make possible the negotiation of the first test-ban treaty. Moreover, Khrushchev's eagerness to conclude an agreement with the British and Americans in mid-1963 was due not only to Soviet domestic politics but also (and especially) to his rift with China (which Larson ignores). The real changes in relations came under Gorbachev, thanks to the "new thinking" and the new behavior of the Soviet leadership, not just because of a change in attitudes toward mutual trust.

What does stand out in all the cases is the highly imperfect intelligence and failure to understand, let alone empathize with, the other side: the frequently contradictory and erratic behavior, the misreading of signals on both sides, the divided counsel between rigid ideologists and frequently naive pragmatists (again on both sides), the built-in tendency to resort to double standards, and the importance of individual personalities. Larson makes a good case that the Soviet Union made substantially more concessions than the United States on the road to agreements, and there are perfectly good reasons why this was so.

Of course, one might question the author's assumption that agreements between the superpowers were a worthy end in themselves. Perhaps often they were, but it is something that deserves a fuller argument.

There is no question that mistrust was virtually a constant across the years of the Cold War. Exploring its dimensions and manifestations with the sophistication and skill of this volume deserves applause, and if the answers are not all

satisfactory, the effort is sure to stimulate, inform, and at times challenge the reader.

Dealing with Britain: The Six and the First UK Application to the EEC. By N. Piers Ludlow. Cambridge: Cambridge University Press, 1997. 282p. $64.95 cloth, $24.95 paper.

Andrew P. Geddes, *University of Liverpool*

Only four years after the creation of the European Community, the British Conservative government was forced to reevaluate its attitude to European integration and apply for EC membership. The economic benefits of accession were apparent, while the political implications of exclusion also loomed large in the minds of pro-Europeans in Prime Minister Macmillan's cabinet. Britain's first tentative steps toward Europe were, though, rebuffed in January 1963 by General de Gaulle's *non*. "Great Britain," said de Gaulle in June 1962, "is moving toward Europe, but it has not yet arrived." *Plus ça change* . . .

Ludlow's analysis of events between 1961 and 1963 is explicitly "Community centred" and intended as a contribution to EC history. In effect, this means that the focus of the analysis is Brussels and, more particularly, the nitty-gritty of the accession talks between the six EC member states and Britain. The argument is that the negotiations were not a tedious subplot that was largely irrelevant to the final outcome; nor was the final result preordained as a consequence of ingrained French hostility to a potential challenge from Britain to its interests and leadership role. Rather, it was the progress of the negotiations, and attendant problems, which provided leeway for expression of French hostility to British accession.

The detailed analysis of the negotiations has three merits that this book clearly demonstrates. First, it provides insight into the attitude of member states toward European integration at a key stage in its development. A vivid picture of member state preferences and their accommodation in interstate bargains is painted. Another strength is that this is done by making extensive use of primary documentation from the member states and EC.

Second, examination of the negotiations displays the success enjoyed by the EC in its formative years but also highlights the fragility of the achievements and the fear among member states that enlargement might jeopardize the rather fragile deals upon which the European project had been constructed. This point about the community's fragility is illustrated by the position taken by the six in the talks with Britain. The six decided to negotiate qua community. This necessitated formulation of common positions on key issues and lengthy meetings at which these were determined before discussions with the British were enjoined. Attempts to forge a united front fell apart in late 1962 and early 1963, when the French acted unilaterally to express their opposition to U.K. accession. Given the centrality of France to the community and the emphasis placed by Adenaeur on Franco-German rapprochement as an overriding priority, there was little the other member states could do. Limits upon communitarization were thus displayed, which again came to the fore when the French adopted their "empty chair" policy in 1965.

Third, insight is provided into the enlargement process. In 1961 the member states were venturing into the unknown. Even though Article 237 of the EC treaty laid down the basic rules for enlargement, the organization of the talks was to be determined by the member states. There was no precedent for enlargement, and the talks with Britain were a case of learning by doing. Moreover, even though Britain was an applicant, it was not prepared to be a supplicant. At times the British seemed arrogant in their assumption that the deals struck at the creation of the community could be unravelled to suit British preferences.

It is, however, the focus on interstate talks and events in Brussels that lie at the theoretical and methodological core of Ludlow's analysis. The attention of the reader is directed toward the talks on agricultural price-support mechanisms, relations between the EC and EFTA, and the shape of postaccession trade links with Commonwealth countries. The slow progress of the talks, which was related in part to British preconditions, presented the French with the opportunity to argue that Britain was not sufficiently certain of its European vocation. Britain's retention of ties to the Commonwealth and United States only confirmed these suspicions and raised the dreaded specter of American influence.

Ludlow's argument is that if the British had been able to express their European vocation in less equivocal terms, then the outcome could have been different. This raises a rather important question: Why were the British unable to express a clearer European vocation? A reliance on analysis of interstate deals within the accession talks and associated summit meetings falls short in its answer to this question because domestic influences on state preference formation are neglected. Political factors in Britain are hinted at in the text but remain peripheral to the detailed assessment of the progress of the talks. Yet, these were febrile times in British politics: Macmillan's Conservative government was unpopular and limping toward its denouement. There were divisions within the cabinet and the Conservative Party over the European issue. Nor was there cross-party consensus in support of accession. In 1962, Labour's leader, Hugh Gaitskell, had expressed clear opposition to European integration and described British accession as a "betrayal of one thousand years of history." Meanwhile, the strains of colonial disengagement and the increased salience of the immigration issue affected reevaluation of Britain's role in world affairs. If the talks failed because Britain was not able to make a clear commitment to the EC, then we need to know why this clear commitment was not forthcoming. At the very least, this suggests interplay between national and EC politics, which in turn suggests that EC history needs to pay closer attention to relations between member state and community politics.

Dealing with Britain is unequivocally centered on the deals struck, or not struck, in Brussels between 1961 and 1963. As such it provides an excellent and authoritative analysis of the state of the EC in that period and makes an important contribution to the burgeoning school of EC history. It demonstrates the tensions between member states and the relative fragility of the European construction at this formative time in its history. The book also suggests a weakness in approaches to EC history that take Brussels as their frame of reference. By laying so much emphasis on the conduct of the talks, domestic influences on state preference formation are neglected. As Ludlow shows, Britain's inability to express a clear commitment to the EC was a major stumbling block for its first application. Clearly, this was associated with difficulties caused by trade and agricultural policy arrangements, which were the meat and drink of the accession talks. It also had, and still has for that matter, a lot to do with British unease about European integration, upon which domestic political factors have had highly consequential effects.

Rush to Union: Understanding the European Federal Bargain. By David McKay. New York: Oxford University Press, 1996. 192p. $55.00.

The European Union in the 1990s. By Paul Taylor. New York: Oxford University Press, 1996. 204p. $65.00 cloth, $22.50 paper.

Alison M. S. Watson, *University of St. Andrews*

When we look back on the history of Europe, more analysis will have been devoted to the question, "Whither integration?" than any other. From state, regional, and local dialogue to debate within and among political parties, the European Union has been scrutinized by economic analysts, political scientists, and social commentators alike. Central to this debate has been the theoretical dimension of European integration, with its emphasis on explaining the dynamics of the integration process and, in particular, where the power in Europe resides. In essence, the question most often addressed is whether increasing integration necessarily implies the diminution of the role of the nation-state. It is with just such an issue that both of the books under review, each in its own way, are concerned.

For McKay, European integration is a "puzzle" with two major dimensions. The first is the timing of European union, and the second is the uniqueness of the European response to the changing economic circumstances of the 1980s and 1990s. In particular, McKay considers why European union occurred in the late 1980s and early 1990s as opposed to the 1950s, a time when the potential for military threat would have suggested the strongest motivation for political federation. The instinctive reaction is to explain recent European union in terms of changing economic conditions, but this is a response McKay rejects. Instead, the unique program of economic integration upon which the European Union (EU) has embarked is contrasted with the experience of other regional economic groupings to form the second part of the puzzle—the distinctive European response of the last two decades. To understand this requires, for McKay, an examination of Rikerian federalism, an approach that blends politicians' motives with the issue of why states form political unions, which synthesizes "the realist perspective of international relations theory with the rational choice tradition in political science" (p. 9). Indeed, it is argued that no other theoretical perspective can provide "such potentially powerful analytic tools" (p. 9).

Thus, *Rush to Union* embarks upon its analysis, in Part I of the volume, of the origins of the EU, encompassing an overview of federal theory and European union in chapter 2, followed by analyses of European union in the periods 1945 to 1982, 1983 to 1992, and 1992 and 1993. It is the examination of federal theory and European union that forms the core of McKay's analysis. After defining the EU as an, albeit incomplete, federal state (an argument based on such factors as the EU's supranational institutional framework), McKay then goes on to provide justification for the use of the rational choice framework, with its emphasis upon the perceptions of politicians in the decision-making process. Such analyses are continued in Part II of the book, with McKay's attempt to define the viability of the EU within the context of "those theories, drawn from political science and economics, which attempt to explain the relationship between fiscal or economic centralization and political centralization" (p. 13). This leads to an interesting analysis, but one which does not always sit well within the confines of the preceding Rikerian framework. In particular, more could have been done to draw out the very real linkages between the domestic political and institutional environment of the individual member states and the wider EU project. For the latter, this translates into the question of how the structures and policies of domestic political and economic life in the member states interact with the supranational political and economic structures and policies of the EU, and how they in turn operate within the wider international system. Thus, each member state cannot divorce national and domestic interests from their context within the wider European framework. At the same time, however, any set of national interests formulated by a member state must take account of how its policies will fit with those agreed within the context of the wider EU. Such a bargaining process has been prevalent throughout the lifetime of the European integration process. For example, the history of the community consists, to a considerable extent, of the bargains and agreements negotiated among the original six members. In more recent times, the relationship of the original six with new members of the community has been a pivotal factor in the EU's development. Brining out such factors in a more distinct manner would have enabled a fuller justification to be made of McKay's assertion that "only a core of the existing EU states will eventually agree to the creation of a genuine federal state" (p. 178).

Moreover, a further criticism of McKay's work is that there is no mention, in his analysis of the political economy of currency crises in chapter 5, of the issue of credibility in government policymaking. This is something that surely affects the ability of politicians to pursue their domestic policy ideals. Thus, for example, if the electorate is accustomed to the government behaving in such a manner as will influence, say, voting patterns (such as increased government expenditure prior to an election), then the government's professed commitment to a policy (for example, low inflation) will command little credibility. The manipulation of the economy by the government for its own aims will be expected, and private firms and the electorate will thus readjust their expectations up to a point at which the government no longer has an incentive to manipulate the economy for electoral gain. In other words, a government must maintain a consistent policy over time and hence build up a reputation for adhering to a particular policy, rather than engage in an obvious policy cycle designed for electoral advantage. In addition, the nature of the international political economy itself may make unbelievable any promises made by national policymakers to their electorates regarding future domestic political and/or economic behavior. This is due to the fact that the use of economic policy instruments by national authorities is undoubtedly influenced by conditions in the international economy. Thus, national policymakers may simply not have the degree of policy control in the contemporary environment that would allow them to manipulate the economy to their own ends. The link between the domestic and international policymaking environment once again appears obvious.

In *The European Union in the 1990s*, Paul Taylor similarly makes a case for a new theoretical approach toward policymaking. As with McKay, the emphasis is upon the rational actor model, which once again is used to justify the issue of whether increasing integration necessarily implies the diminution of the role of the nation-state. For Taylor, this results in a vision of a Europe which does not threaten the nation-state but which instead "benefits the majority of individuals without threatening national sovereignty" (p. 2). The basis for such a statement runs directly from the way in which Taylor conducts his analysis, and again it is intimately bound up with the notion of the links between domestic and international policymaking. Hence, in chapter 2, where the theoretical foundation for Taylor's later analysis is laid, the

notion is put forward of consociationalism as a way of understanding the European Union. The states are held within a consociation, which is one element of the dual structure that comprises the horizontal linkage between the individual nation-states, and the vertical linkage between the EU nation-states and the EU "collectivity." From this follows the analysis of what kind of membership of the EU is most conducive to its making a positive contribution to the well-being of Europe and the globe. Such arguments are very much bound up with the question of the deepening of European union. In order to justify a full global role, the EU should be viewed in the international sphere as a unified actor. Such an image, however, brings into question the notion, once again, of national identity, the issue of the way in which European integration has affected the identity of people in the EU member states, and, indeed, the manner of their involvement in the agreements of that organization. This is a theme that Taylor elucidates in chapter 5. In particular, and where Taylor's book especially adds to the theoretical analysis of integration in Europe, his attempts to address both the issue of the extension of the range of international organization in the EU and the survival and entrenchment of the state is commendable. Indeed, it is important that we consider the nature of the state and its exact role in the interaction between domestic and international decision making. It must never be forgotten that the six founding members signed a treaty and not a constitution. It is thus the individual member states who control the degree to which Europe integrates. The fact that both McKay and Taylor attempt to deal with the role of the state within a theoretical foundation makes both of their analyses important contributions to the European integration literature.

Anchors against Change: American Opinion Leaders' Beliefs after the Cold War. By Shoon Kathleen Murray. Ann Arbor: University of Michigan Press, 1996. 201p. $44.50.

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Shoon Murray argues that while American opinion leaders' views of the Soviet Union (subsequently Russia) and containment policy underwent dramatic shifts at the end of the Cold War, their broader beliefs about the conduct of U.S. foreign affairs remained fairly stable. This conclusion is based on a panel survey design (surveying the same individuals at two different times) that compares the responses of American opinion leaders in 1988 with their responses in 1992. Murray finds that their broad beliefs about foreign policy along two dimensions (militant internationalism, or the utility of force and containment, and cooperative internationalism, or multilateral efforts to address common international problems) remained constant, even though the global environment changed dramatically during this period.

Using the 1988 Foreign Policy Leadership Project (FPLP) of Holsti and Rosenau as a starting point, Murray resurveyed a portion of these participants again in 1992 in her Leadership Opinion Project (LOP). While preserving the respondents' anonymity, she matched responses from the first and second survey in order to ascertain continuity or change in individual response patterns. Although the FPLP and the Chicago Council on Foreign Relations (CCFR) both undertake quadrennial assessments of views of American opinion leaders, these cross-sectional surveys do not attempt to resurvey the same individuals. These cross-sectional analyses can address the question of static constraint, which Philip Converse defined as "given initial knowledge that an individual holds a specified attitude, that he holds certain further ideas and attitudes" (p. 4), but they cannot address the question of "dynamic constraint," which Converse defined as "the probability that a change in the perceived status . . . of one idea-element would *psychologically* require, from the point of view of the actor, some compensating change(s) in the status of idea-elements elsewhere in the configuration" (p. 5). Murray's panel survey adds to our understanding of belief systems by evaluating the extent of dynamic constraint in the views of American opinion leaders.

Murray finds stability in individual beliefs on militant and cooperative internationalism, domestic policy preferences, and domestic ideological orientations but not in attitudes toward the Soviet Union/Russia and containment policy. Unlike some previous scholars, Murray concludes that views toward the Soviet Union and containment did not provide dynamic constraint on an individual's attitudes toward other issues or broader beliefs. Instead, Murray finds the evidence is consistent with the conclusion that "domestic ideological orientations" (an individual's conservative or liberal leanings) constrain both domestic policy views and "basic foreign policy postures" (pp. 6–7). As a result, even though international circumstances dramatically changed, Murray concludes that, "because leaders draw upon long-held values to evaluate new international circumstances, their beliefs about the proper way to conduct foreign policy were largely unchanged" (p. 8).

Murray develops her argument across several data-filled chapters. Chapter 2 recounts changing elite views toward the Soviet Union from the 1970s through 1992. Not unexpectedly, Murray finds in the FPLP, CCFR, and LOP surveys that, to varying degrees, elites shifted from a view largely skeptical of the Soviet Union's intentions and favoring containment policy in the period before the fall of the Berlin wall in 1989 to a much more benign attitude in 1992 toward Russia and with significantly less support of containment. None of this is surprising, but the survey data provide the foundation for the later analyses. More distracting and of less value is an extensive historical discussion, based on the author's interviews of officials and their public statements, which tracks the evolution of the policymaking community's struggles to make sense of Mikhail Gorbachev's policies in the 1980s. Given the book's extensive survey data, the detailed views of high-level officials who were not in the LOP survey seem mostly tangential. Since Murray's presentation and discussion of the survey data are sufficient and accomplish her purpose, a brief summary of historical events could have established the context for her analysis of the LOP data.

Chapters 3 and 4 contain the empirical core of the book and provide the main contribution to the literature on elite beliefs. Chapter 3 brings the LOP panel data to bear on the question of the stability of beliefs at the individual level across time on militant and cooperative internationalism and specific policy issues. As Murray points out, previous cross-sectional analyses can illuminate aggregate trends across time but cannot address the stability of individual beliefs. Although previous scholars treated militant and cooperative internationalism as orthogonal dimensions from which four types of belief orientations are created, Murray examines each dimension as an independent attitude construct, which allows her to evaluate the interconnection between the dimension and other factors. These analyses indicate the stability of the militant and cooperative internationalism beliefs despite radically shifting views of the Soviet Union/Russia and containment.

Chapter 4 provides data suggesting that the dimensions of foreign policy beliefs and domestic policy beliefs are substan-

tially connected. Based on this finding, Murray concludes that "both types of beliefs are derived from the same core values and, second, that sets of values coalesce along the liberal-to-conservative continuum." Although she clearly indicates these final conclusions are based upon conjecture that "is consistent with, but cannot be confirmed by, the data" (p. 85), her analysis soon runs into trouble. Although the author at several points indicates that she does not view leaders' beliefs as unidimensional based upon a liberalism/conservatism continuum and that other dimensions are necessary to explain the complicated nature of beliefs (e.g., p. 89), the analysis in the next chapter belies these assertions.

Chapter 5 examines the distribution of policy attitudes largely in reference only to ideological views along the liberalism/conservatism spectrum. Despite assertions to the contrary, the analysis significantly extrapolates from the one dimension in such a manner that it approaches a unidimensional presentation in practice. This tension also points to a lack of clarity in the book regarding the "core values" that Murray asserts are arrayed along a liberalism/conservatism dimension (p. 111). It is not clear from her presentation how these values differ significantly from the ideological orientations used in the analysis.

Even with these shortcomings, Murray makes an important contribution to the scholarly discourse on the stability and structure of elite beliefs. She is also to be commended for the extensive documentation of her research methods in four detailed appendices. Through her imaginative creation of the panel survey, Murray addresses an important question that the previous literature on this subject had failed to address adequately.

Disarming Strangers: Nuclear Diplomacy with North Korea. By Leon Sigal. Princeton, NJ: Princeton University Press, 1998. 321p. $29.95.

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For some, the confrontation between the United States and North Korea over the latter's nuclear weapons program typifies the post–Cold War security environment. A rogue state, hostile to democracy and free markets, acquires the capacity to build nuclear weapons. The United States, as the hegemonic power, must patch together a coalition of allies (South Korea, Japan) and other great powers (Russia, China) and negotiate with a regime that has consistently violated international norms. Approaching the brink of war in June 1994, a last-minute settlement is reached because of the "track II diplomacy" of former president Jimmy Carter. In return for giving up its nuclear ambitions, North Korea will receive proliferation-proof South Korean light-water nuclear reactors. In essence, North Korea acquiesced in return for a $5-billion carrot, equal to one-quarter of its gross national product.

If this episode is a harbinger of future challenges to U.S. security, then what can be gleaned from it as a guide to policymakers? In *Disarming Strangers*, the main thesis is that the United States and its allies wrongly adopted a "crime and punishment" strategy, relying on coercion to force North Korea to acquiesce. Sigal comes to the opposite conclusion: "Cooperation works. It can succeed where coercion fails. It is also cheap by comparison" (p. 4). Tracing the episode from its origins in early 1991, he tries to demonstrate that North Korea practiced a tit-for-tat strategy. It responded positively to any sign of cooperation but turned bellicose when threatened. This is why the Framework Agreement, which was decried by most policy analysts when it was signed in October 1994, has worked. North Korea has matched every American concession; three years later, the odds of success are much stronger than when it was signed.

If coercion was such an ill-conceived strategy, then why did the United States pursue it for three years? Sigal provides four reasons. First, there was a consensus among policy elites in the United States that any compromise would be an act of appeasement. Their "shared images" of nuclear diplomacy dictated that, when dealing with rogue states, the stick was better than the carrot. Compromise was a tough political sell. Second, the International Atomic Energy Agency (IAEA), burned by previous lapses in Iraq, was determined to make an example of North Korea. It wanted total compliance and rejected any hint of compromise. Third, South Korea was a substantial brake on any kind of rapprochement with the North. Surprisingly, this was not due to national security but to domestic politics. President Kim Young Sam needed the support of hardliners to stay in power in South Korea. Kim also felt he needed to distance himself from the leader of the opposition, Kim Dae Jung, who preferred a more accommodating strategy. Fourth, there was a lack of consensus among U.S. bureaucracies over the best course of action. Those that opposed a deal, such as the CIA, made life more difficult for the negotiators by leaking intelligence estimates that grossly inflated the North Korean threat.

Within this context, the United States had precious little wiggle room. A diplomatic solution lacked a powerful backer within the Clinton administration. When compromise was proposed, it rankled the IAEA, scared the South Korean regime, and ran into a buzzsaw of vitriolic op-eds and congressional belligerence. Faced with these constraints, it is not surprising that the United States spent three years threatening economic sanctions and refusing to compromise its demand of unfettered access to all North Korean nuclear installations.

Michael Mazarr's (1995) *North Korea and the Bomb* and Don Oberdorfer's (1997) *The Two Koreas* are two other recent books about the crisis with North Korea. *Disarming Strangers* is better than either of these books in describing the bureaucratic and political environments in Seoul and Washington that contributed to such a belligerent strategy. Another plus is Sigal's description of the nongovernmental organizations and academics that developed more contacts with the North Korean regime than the U.S. government possessed. In this context, it is not surprising that Carter was able to meet with Kim Il Sung and secure an agreement, while U.S. negotiators were hamstrung with political constraints and a lack of knowledge about North Korea.

In other ways, however, Sigal's book is more flawed than Mazarr's or Oberdorfer's. *Disarming Strangers* gets some facts wrong and omits others of crucial importance. Sigal argues that, in the past, inducements and reassurances were responsible for stopping nuclear weapons programs in several other states, including South Korea and Taiwan (p. 4). This is a gross exaggeration; a look at those two cases shows that it was economic coercion, not economic inducements, that worked. In the North Korean case, there is credible evidence that the threat of coercion contributed more to compliance than Sigal would care to admit. North Korea's initial friendliness on this issue in 1991 was as much a product of Chinese pressure as American inducements. History repeated itself in 1994. Oberdorfer notes that in June 1994, China told North Korea it could not veto U.N. sanctions and strongly recommended reaching an agreement with the United States. This threat helps to explain Pyongyang's willingness to cut a deal with Carter. Sigal also castigates the United States for demanding a complete account of North Korea's nuclear past and being

so inflexible in its demands. What he fails to mention is that the Nuclear Non-Proliferation Treaty (NPT) was up for renewal in 1995. Renewal was an important goal of the Clinton administration and affected negotiations with North Korea. To weaken the norm of total compliance with the NPT just before renewal would have made it very difficult for the United States to maintain the strong norm against nonproliferation.

In addition to these empirical weaknesses, *Disarming Strangers* suffers from some theoretical confusion. A tit-for-tat strategy has two crucial components: cooperation with "nice" players and the quick punishment of actors who defect. Sigal completely ignores the second aspect, which is a shame, since it also helps explain U.S. strategy. North Korea's first defection, reprocessing its spent fuel, triggered U.S. attention. The second North Korean defection, failure to comply with either the Safeguards Agreement or its agreements with South Korea, escalated the crisis and initiated the U.S. push for sanctions. A key aim of *Disarming Strangers* is to describe how the United States erred in not pursuing a tit-for-tat strategy. But the act of negotiating while preparing a credible threat in case of defection is the epitome of tit-for-tat.

With *Disarming Strangers*, Sigal provides an excellent account of how domestic politics and alliance dynamics can affect crisis bargaining. As a guide to scholars and policymakers of how to avoid such confrontations, it leaves much to be desired.

Fiscal Policy Outcomes and Electoral Accountability in American States

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Clear fiscal policy effects appear in American state gubernatorial and legislative elections between 1968 and 1992, independent of the effects of incumbency, coattails, term limits, and macroeconomic conditions. The results show that accountability is generally stronger following a period of unified party control than under divided government. Voter reactions to taxes and spending relative to the state economy are conditional on expectations, which differ for each party. Net of these expectations, Republican gubernatorial candidates lose votes if their party is responsible for unanticipated increases in the size of the state budget; Democrats do not and, indeed, may be rewarded for small increases. Independent of this, the incumbent governor's party is punished in legislative elections for failing to maintain fiscal balance. Taken together, these results show how electoral accountability for fiscal policy outcomes is strong but highly contingent on a complex configuration of party labels, partisan control, expectations, and institutions.

Despite the attention given to big government and budget deficits during recent political campaigns, the connection between fiscal policy outcomes and election results in the American states is not well understood. Some studies indicate that gubernatorial elections are influenced by changes in taxes and spending, and others suggest that voters rightly expect Democratic politicians to tax and spend more than Republicans, but there is almost no empirical work on whether voter reactions to fiscal policy are contingent on which party is in power. Moreover, no study has asked whether voter reactions to fiscal policy outcomes are contingent on partisan control of both the executive and legislative branches, or systematically examined voter reactions to budget surpluses and deficits per se, or investigated the effects of fiscal policy outcomes on state legislative elections.

We estimate models of retrospective voting in American state elections to determine whether voters respond to fiscal policy outcomes and how their responses depend on their expectations and ability to assign responsibility for outcomes to specific officeholders. Our data encompass gubernatorial and legislative races from 1968 to 1992 in 41 states with similar electoral cycles. In order to determine the independent effects of fiscal policy outcomes on election results, we control for many relevant political and economic factors identified in previous studies. Throughout, we also ask how two institutional factors affect accountability. First, are voter responses stronger under unified government than divided government? Does divided government dilute accountability? Second, does collective action dilute accountability, that is, are the results stronger in gubernatorial than legislative elections?

We find evidence that politicians are more likely to be held accountable following a period of unified party control. Electoral penalties for choices voters dislike are less evident after periods of divided government. With regard to collective action, the answer is less clear. All in all, accountability may be stronger in gubernatorial than legislative elections, but the effects of fiscal policy are evident in both. Altogether, our results demonstrate that the extent to which voters hold elected officials responsible for fiscal policy outcomes depends on party labels, patterns of partisan control, expectations, and institutions.

Our results also suggest that voters respond to two different aspects of fiscal policy: *scale*, the size of state revenues and spending relative to the state economy, and *balance*, the size of the current surplus or deficit in the state budget. Fiscal scale appears more important in gubernatorial elections, but voter reactions to fiscal imbalance appear in legislative contests. We find evidence that voters dislike both budget deficits and surpluses. Moreover, voters hold politicians accountable for changes in fiscal scale in a *partisan* way: They punish Republican incumbents and reward Democrats for unexpected increases in fiscal scale, but they reward Republicans and penalize Democrats for unexpected cuts.

In the next section, we briefly summarize the literature on fiscal performance and state elections. Then we describe our model of voter expectations and how voter reactions to fiscal performance depend on the incumbent partisan configuration. We lay out specific expectations to be tested with the data. Following that, we describe our data set and the measures used in our empirical analysis and present the estimation results. Finally, we relate our findings to theory, previous studies, and such recent trends as the growth of public spending and divided government.

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This research was supported by the National Science Foundation under grant SBR 92-23638. A previous version was presented at the annual meetings of the Public Choice Society, Houston, TX, April 12–14, 1996, and the Midwest Political Science Association, Chicago, IL, April 18–20, 1996. We thank Brian Collier, Gerhard Glomm, Philip Grossman, Mark Jones, Gary King, Emily Van Dunk, Ronald Weber, and seminar participants at Columbia University, California Institute of Technology, Massachusetts Institute of Technology, and Princeton University for their comments.

PARTIES, POLICY OUTCOMES, AND ELECTIONS IN THE AMERICAN STATES

Recent research supports several propositions important to our analysis. First, fiscal policy depends on which party controls the state government and whether the incumbent governor is eligible for reelection. For example, Alt and Lowry (1994) find that the implicit long-run targets for spending and revenues as shares of personal income are higher for (nonsouthern) unified governments led by Democrats than by Republicans, with divided governments falling in between. According to Besley and Case (1995a), state taxes and spending are higher under Democratic governors and higher still if an incumbent Democrat is ineligible for reelection because of term limits.

Second, in state elections, Chubb (1988), Kone and Winters (1993), Niemi, Stanley, and Vogel (1995), and Peltzman (1987) all find that the party of the president benefits from growth in *national* income; less consensus exists about *state* economic conditions. Chubb found state income growth to have marginal effects on gubernatorial races and none on state legislature elections. By contrast, Niemi, Stanley, and Vogel's exit poll data from 34 gubernatorial races in 1986 show that voters reward candidates of the incumbent governor's party for increases in state per capita disposable income. Besley and Case (1995b) find that governors of states with better economic performance than neighboring states are more likely to be reelected. Finally, Carsey and Wright (1998) find that in gubernatorial elections voters' subjective evaluations of the state economy have greater influence than their evaluations of the national economy.

Third, many studies connect elections and state taxes. Niemi, Stanley, and Vogel (1995) find that voters punish the incumbent party's candidate for past increases in state taxes; Kone and Winters (1993) reveal that a candidate of the incumbent governor's party is punished for increasing taxes but not rewarded for decreasing them. Besley and Case (1995b) find that governors who raise taxes less than in neighboring states are more likely to be reelected.

Fourth, Peltzman's (1992) study of presidential senatorial, and gubernatorial elections finds that voters are "fiscal conservatives." Consistent with previous research (Peltzman 1987), candidates from the incumbent party are punished for increases in spending, holding macroeconomic conditions constant. Equations that include changes in both spending and revenue show that voter reactions are concentrated on changes in spending. Peltzman interprets this to mean that deficit financing does not matter politically: Voters punish spending increases regardless of whether they are financed by taxes or borrowing. He admits to being puzzled as to why public sector spending continued to rise throughout the nearly four decades covered by his data despite voter opposition.[1]

With the exception of Peltzman (1992), researchers have not tested whether voter reactions to fiscal policy outcomes are contingent on which party controls the government. Most of Peltzman's results are not party specific, but at the state level he finds that only Democratic candidates for governor are punished for increases in state spending. If that is the case, then it is hard to understand how Democrats have remained electorally competitive in an era of fiscal expansion.

Finally, very little systematic research exists on fiscal policy outcomes and state legislative elections. Only Chubb (1988) studies legislative elections, and he does not include any measures of fiscal policy outcomes in his equations.

TESTABLE PROPOSITIONS

We introduce several innovations. First, we consider voter response to fiscal outcomes in both gubernatorial and legislative elections. Second, we distinguish between fiscal scale and fiscal balance, that is, aggregate public revenues and spending and the difference between them, and deal simultaneously with voter response to both. Third, we propose a model of fiscal scale that is more sophisticated than prior ones and, as a result, resolves Peltzman's empirical puzzle of why spending grows if voters are fiscal conservatives. Finally, we explore how institutional factors, such as divided government and diluted accountability in the legislature, affect voter response to scale and balance. In total, we argue that voter reactions to fiscal policy do not depend simply on whether spending increased or even which party was in control when it did. Rather, the critical factors include whether a single party had control of both branches of government and whether outcomes deviated from expectations. Looking at voter response to fiscal policy in this way allows us to go beyond the results and puzzles of previous studies.

Below, we describe four specific conjectures regarding fiscal scale, fiscal balance, divided government, and legislative accountability. In the remaining sections of the article, we test these conjectures with data from gubernatorial and legislative elections.

A Partisan Model of Voting on Fiscal Scale

Prior models of fiscal scale have been fairly simple: A dependent variable measuring vote or seat share is regressed on a handful of political and economic control variables (e.g., incumbency, state income growth), along with a variable measuring change in expenditures or revenues. Peltzman (1992) included a variable for the governor's political party. As noted earlier, this produced the incongruous result that Democratic governors are punished more than Republican governors for spending increases. We extend previous models in a number of ways.

First, we assume that voters have varying preferences regarding fiscal outcomes and, other things equal, choose to vote for a Democrat or Republican accord-

[1] Peltzman (1992, 359–60) offers two explanations: Voter reactions to increased spending are unlikely to overcome the incumbency advantage, and in the 1950s and 1960s politicians believed that spending programs would be rewarded by voters and only recently have learned otherwise.

ing to these preferences. The tacit assumption is that Democratic and Republican politicians pursue different fiscal policies, with the latter generally favoring fiscal restraint. This builds on an extensive comparative literature on economic voting (see Alesina, Roubini, and Cohen 1997; Keech 1995; and Nannestad and Paldam 1995) by extending the usual partisan theory assumption, so that parties differ with respect to instruments (taxing, spending) rather than outcomes (unemployment, inflation).

Second, voters form party-specific expectations about fiscal performance: They believe Democrats will raise more revenues and spend more money than Republicans (Baumer and Gold 1995). Among the 10,545 respondents in the NES file, 68% place Democrats higher than Republicans on the seven-point scale describing how the parties stand on "government services and spending," while 18% award both parties the same score, and 14% place Republicans higher than Democrats (Miller and the National Election Studies 1998).[2] We sought a method for modeling such expectations that would minimize demands on voter information and allow expectations about parties to differ across place and time. Therefore, we modeled them as a function of the performance of the last previous administration with a governor from the same party as the current one. Where possible, we checked the robustness of the results based on this model against those produced by others.

Third, when voters evaluate the performance of incumbent governments, they react most strongly to fiscal "surprises," or how far fiscal policy at the time of the election differs from what voters expected it to be, based on which party is incumbent. Thus, unlike Peltzman, we believe that high levels of spending may or may not invoke voter sanction, depending on which party is in office and its previous levels of spending.

Fourth, voters may punish deviations from expectations even to the point of choosing more of "the hair of the dog that bit them," that is, replacing a high-spending Republican with a Democrat expected to spend at least as much or more. On the surface such voting may appear puzzling, but Austen-Smith and Banks (1989), building on Ferejohn (1986), show that when there is incomplete information, incumbents have incentives to keep their promises only when there is some positive probability that voters will punish any undesired past performance. Thus, at least under some circumstances, rational Republican (Democratic) voters should be prepared to vote against incumbents of their own party because of fiscal increases (contractions).

Finally, in order to deal with an "instrument problem" of possible reverse causation (does fiscal policy affect elections, or do incumbents in danger of losing or not running for reelection spend more?), we follow Besley and Case (1995a) and control for gubernatorial elections that take place under binding term limits. While this is an imperfect instrument, it isolates many cases of behavior by incumbent governors who do not expect to be around after the next election.

Whereas Peltzman suggests that voters are all fiscal conservatives, we allow them to have varying preferences about fiscal outcomes. Furthermore, previous authors see voters as reacting uniformly to all increases in spending, but we contend that they respond most strongly to deviations from party-specific expectations following periods of unified party control. Finally, we attempt to deal with an important reverse causality problem ignored by all previous researchers except Besley and Case. While our model is considerably more complex than previous work, we believe our additions are warranted and may help resolve the puzzle of persistent growth in government despite apparent voter opposition.

To sum up, we test whether the data are consistent with the following proposition about partisan retrospective voting.

PROPOSITION 1. *Voters react to the difference between observed performance and party-specific expectations about fiscal scale, allowing for whether the incumbent governor is ineligible for reelection.*

Furthermore, we expect that this will hold independent of the effects of fiscal balance on voting.

Voting on Fiscal Balance

The effects of budget surpluses and deficits per se on election outcomes depend on both voter preferences and the extent to which voters can determine whether their preferences are being satisfied. Economic theory provides no unambiguous expectations about voter preferences regarding deficits. According to Ricardian equivalence theory, voters will be indifferent between taxes and government borrowing if they discount the future at the same rate as financial markets (see Barro 1974). Yet, myopic and self-interested voters may reward incumbents who run deficits and punish incumbents who run surpluses, since a deficit means that the costs of current benefits are being imposed on future generations (Alesina and Perotti 1995). Alternatively, voters may punish incumbents for running deficits but reward surpluses, at least as long as surpluses are used to retire past debt.[3] Finally, voters may punish incumbents for both deficits and surpluses if they view failure to maintain a balanced budget as a sign of fiscal mismanagement. While theory is agnostic, we hypothesize that voters expect elected officials to maintain fiscal balance on an annual basis as well as over time. This implies that voters punish incumbents for both deficits and surpluses.

Do voters react to deficits in party-specific ways, as we argued for fiscal scale? Some interpretations of party ideology attribute different attitudes about budget balance to Democrats and Republicans (Keech

[2] The latter percentage falls to 12% if we restrict the sample to voters. While either figure may seem high, each is less than half the 30% of ANES respondents who did not know which party controlled the House of Representatives either before or after the election.

[3] Our data on expenditures include interest payments on general debt but not retirement of debt.

1995), but research on the U.S. federal budget and comparative studies of OECD countries reveal no systematic link between leftist or rightist parties and the frequency or size of deficits (Alesina, Roubini, and Cohen 1997). Work on state public finances also finds no systematic differences (Poterba 1996). Although the rate at which deficits occur in our data is lower for unified Republican government than for other partisan configurations, the differences are not significant at conventional levels according to a χ^2 test.[4] Therefore, we do not expect to find systematic differences in voter reactions to fiscal balance based on party, although our empirical model would not prevent their discovery.

PROPOSITION 2. *Net of the expectation that incumbents of both parties will provide fiscal balance, voters react negatively to both surpluses and deficits, whichever party is in office.*

Again, we expect that this will hold independent of the effects of fiscal scale. We also considered whether voter reactions might be stronger in states with a balanced budget law, but, as reported below, we did not find significant effects.

Unified Party Government and Responsibility for Outcomes

Previous studies of fiscal policy outcomes and state elections examine only the race for governor, without reference to partisan control of the legislature. Yet, recent work on economic voting (Anderson 1995; Powell and Whitten 1993) shows that the greater the "clarity of responsibility"[5] in a government, the more likely it is to be held accountable (rewarded or punished) for economic performance. While these studies mainly consider parliamentary coalitions, their insights can be extended to state governments. Because it is possible for opposite parties to control the different branches of state government, we expect that clarity of responsibility will be highest under unified control. Therefore, we anticipate that voters are more likely to hold candidates from one party responsible for fiscal policy when that party controls both branches of government. For both fiscal scale and balance, we argue that divided government has real electoral consequences.

PROPOSITION 3. *Voter reactions to fiscal policy outcomes are stronger if the same party controls both the executive and legislative branches of state government prior to the election.*

Uncertainty Due to Collective Responsibility

In addition to the effects of divided government, voters in legislative elections face the problem of assigning credit or blame for collective actions (Austen-Smith and Banks 1989). Individual legislators may be able to avoid blame for bad outcomes, even when their party controls both branches of government. Conversely, they may try to claim credit for good outcomes in which they played no role (Fiorina 1989). Chubb (1988) found that macroeconomic outcomes have less effect on legislative elections than on gubernatorial elections, and Peltzman (1992, 343) found that the effects of federal spending growth were smaller for U.S. Senate candidates than for presidential candidates. We expect that the same result holds for state fiscal policy.

PROPOSITION 4. *Voters do not react to state fiscal policy outcomes as strongly in state legislative elections as in gubernatorial elections.*

MODEL SPECIFICATION AND DATA

We study elections for governor and the lower house of the legislature from 1968 through 1992 in 41 states that have four-year gubernatorial and two-year legislative cycles.[6] Appendix A provides a listing of the states and years in our data set. In our generic specification, *Democratic Party Support* is related to state-specific fixed effects. Other explanatory variables vary over space and time and include the effects of incumbency, term limits, political coattails, macroeconomic performance (interacted with incumbent partisan control), and fiscal policy outcomes (also interacted with incumbent partisan control).

Dependent Variables

For gubernatorial elections, the dependent variable is *Dem. Pct. Votes for Governor*, that is, votes received by the Democratic candidate as a percentage of all votes cast for major party candidates (Scammon and McGillivray 1995). For legislative equations, we use *Dem. Pct. Legislative Seats*, or the number of seats won by Democrats in the lower chamber of the state legislature as a percentage of all seats won by Democrats and Republicans (U.S. Department of Commerce, various years). To compare coefficients in gubernatorial and legislative elections, we estimate alternative legislative models using the statewide votes received by Democratic candidates as a percentage of major party votes for 1970–89, since the data are available only for this period.[7] In Appendix B we report any differences in the

[4] Our data set includes 84 (28) cases of unified Democratic (Republican) state government and 56 (65) cases of divided government, that is, a Democratic (Republican) governor faces a legislature controlled by the opposite party. There are 35 (9) cases of deficits under unified Democrats (Republicans), and 25 (32) cases of deficits under a Democratic (Republican) governor heading a divided government.

[5] Powell and Whitten (1993, 398) define "clarity of responsibility" as "the degree of perceived unified control of policymaking by the incumbent government."

[6] Alabama, Mississippi, and Maryland are omitted because they have four-year legislative terms. New Hampshire, Rhode Island, and Vermont are omitted because they have two-year gubernatorial terms. We also omit Nebraska, which has a unicameral legislature with nonpartisan elections; Louisiana, which has a unique open primary/runoff system; and Alaska, where state revenues depend heavily on severance taxes and exhibit very large annual swings.

[7] The percentage of seats won may not be a linear measure of voter preferences, due to the "votes-seats" curve in each state (Gelman and King 1994). Data on votes were obtained from the Inter-

results that depend on whether we used the percentages of seats or statewide votes. In each case, the dependent variable is the untransformed percentage of votes or seats won by Democrats.[8]

Political Control Variables

To determine the independent effects of fiscal policy outcomes on elections, we control for certain political variables.

Fixed Effects. We use a different intercept for each state. This captures differences in vote outcomes due to state-specific factors, including differences in average (long-term) partisan support among the state electorates.

Incumbency Advantage. We include a dummy variable for *Incumbent Candidate* for governor (the individual, not the party). Incumbency is coded 1 for Democrat and −1 for Republican to capture the expected direction of effects on the dependent variable, Democratic vote share. The legislative equation includes the *lagged percentage of seats* captured by Democrats in the state legislature.[9]

Term Limit Effects. We follow Besley and Case (1995a) by including separate dummy variables for *Term Limited Democrat* and *Term Limited Republican* incumbents in our gubernatorial equation. See Council of State Governments (various years) for states having gubernatorial term limits.

Coattail Effects. Results in lower level elections may be affected by increased turnout and party voting due to concurrent elections for higher office (Chubb 1988). We measure coattail effects by the Democrats' percentage of major party votes for president and U.S. Senate, respectively, *Dem. Pct. Votes for President* and *Dem. Pct. Votes for U.S. Senate*, plus dummy variables for presidential and senatorial election years to capture intercept shifts. We also include gubernatorial coattail effects in equations for concurrent legislative elections.[10]

Macroeconomic Performance

We measure the effects of relative economic performance by *Relative Income Growth*, the percentage change in real per-capita state personal income over two years ending with the election year, minus the equivalent change in national personal income.[11] We do not believe voters know the exact growth rates of national and state real per-capita income, but they may have a feeling about how their state is doing relative to what they hear about the nation as a whole. Credit for income growth is assumed to go to the governor's party, and separate growth variables are included for each party.

State Fiscal Policy Outcomes, Voter Expectations, and Party Configurations

The state fiscal policy outcomes we examine are deviations from expected state revenues and spending (scale) and surpluses and deficits (balance). Revenues are measured by general revenues minus federal transfers, while spending is measured by general expenditures. The difference between "total" and "general" revenues or spending includes amounts for state-owned liquor stores, utility companies, and insurance trusts. We subtract federal transfers from general revenues in the belief that voters react to changes in revenues generated by state taxes and fees.[12] For consistency across variables, we measure fiscal scale by taking the ratios of revenues and spending to state personal income averaged over the election year and the year before.

Voter Expectations

We model voter expectations of fiscal policy in a newly elected administration as the observed performance of the last previous administration with a governor from the same party as the current one.[13] We calculate expectations for both fiscal scale variables (*Expected*

University Consortium for Political and Social Research (1992). This data set covers state legislative elections from 1968 through 1989. Data for 1968 and 1969 were used to create an independent variable for lagged vote share.

[8] An alternative would be to use a log-odds transformation, but the untransformed share makes the coefficients much easier to interpret, and the vast majority of our observations lie between 30% and 70%, so distortions due to our linear specifications are minimal. Chubb (1988) and Peltzman (1992) both eliminate one-sided elections from their data. Such elections are likely due to the absence of quality challengers, which is itself a sign of satisfaction with past performance.

[9] Chubb (1988) and Peltzman (1987, 1992) use lagged dependent variables (or their instruments) in the gubernatorial equations. In most variations of our gubernatorial equations, including a lagged vote-share variable produced an insignificant coefficient and did not change our qualitative interpretations. Therefore, we omitted it from our reported models.

[10] Chubb (1988) includes gubernatorial coattails in his legislative equation only for years in which there is no presidential election. We tested the assumption that gubernatorial coattails depend on whether there is a presidential election. While the point estimate of the coattail variable is greater in nonpresidential years, the t-ratio for the difference is less than 1.

[11] This gave clearer results than either including the state and national components separately or ignoring the national component. When we include separate variables for national and state income growth interacted with party control, coefficients on the national income growth variable are insignificant or perversely negative. Multicolinearity between the average performance of neighboring states and the national growth rate precludes finding any effect of differences in performance between a state and its neighbors. We also experimented with including changes in the state unemployment rate, but our results regarding the effects of fiscal policy were unaffected.

[12] Removing federal transfers from revenues does not mean that we treat voters as being highly sophisticated. Rather, we treat voters as reacting specifically to the amounts taken from their incomes in state taxes. In a way, federal transfers are the opposite of state revenues: An increase in federal transfers feels more like a windfall than an added tax cost, since the money can be interpreted as coming largely from taxpayers in other states.

[13] On the whole, last incumbency produced more accurate expectations of fiscal scale than did an alternative model using data from all years since 1949 with a governor from the same party. Across revenues and spending for unified party government, the standard deviations of the unexpected components of fiscal scale were be-

Revenues and *Expected Spending*), as well as that part of spending and revenue that is not accounted for by past level (*Unexpected Revenues* and *Unexpected Spending*). Neither theory nor data give us strong reasons for believing that expectations based on past performance matter for fiscal balance.[14] Instead, we assume that voters expect the fiscal year budget to be in balance.

We use separate variables, *Surplus* and *Deficit*, to measure fiscal balance. We create these by subtracting real per-capita general spending from real per-capita general revenues for the year of and the year prior to the election. We then calculate the ratio of this difference to state personal income in each year as well as the average of the two ratios. Finally, we create separate variables for surpluses, coded as zero or positive, and deficits, coded as zero or negative.

Except where noted, each of the four partisan configuration dummy variables (*Unified Democrats*, *Unified Republicans*, *Divided Government/Democratic Governor*, *Divided Government/Republican Governor*) is interacted with both of the balance variables and a variety of scale variables, described in the next section. Divided government is coded according to the party of the governor to facilitate analyzing the effects of coattails in the legislative equations.

Table 1 reports summary statistics for gubernatorial election years in our data set. Eleven elections are omitted during 1968–90 because there were no previous administrations with the same gubernatorial party since 1949 to provide data for expectations. (See Appendix A.) Using the last incumbency model, both expected revenues and spending are about 20% larger under unified, nonsouthern Democrats than unified Republicans (the *t*-statistics for the difference exceed 2.2), with southern Democrats and divided government in between. Both parties have produced unanticipated increases in scale on average.

TABLE 1. Data Summary

Variable	Cases	Mean	Std. Dev.
Dem Pct. Votes for Governor	233	52.5	9.6
Dem Pct. Legislative Seats	233	57.6	16.2
Dem Pct. Votes for President	63	43.6	8.1
Dem Pct. Votes for U.S. Senate	148	52.6	14.1
Relative Income Growth	233	0.47	3.85
Expected Revenues	233	7.81	2.19
Nonsouthern, Unified Democrats	43	8.59	2.88
Unified Republicans	28	6.88	1.65
Unexpected Revenues	233	0.93	1.22
Expected Spending	233	10.51	2.90
Nonsouthern, Unified Democrats	43	11.46	3.65
Unified Republicans	28	9.65	3.05
Unexpected Spending	233	0.95	1.70
Fiscal Balance	233	0.33	0.80

Note: Data are for gubernatorial election years in states shown in Appendix A. The Democratic percentage of votes refers to the share of votes cast for major party candidates.

Relative income growth is the difference between the percentage change in state and national real per-capita personal income for the election year and the preceding year. Revenues are general revenues minus federal transfers as a percentage of state personal income, averaged over the election year and the preceding year. Spending and fiscal balance are based on general revenues and general expenditures as a percentage of state personal income, averaged over the election year and the preceding year.

Differences (standard errors) between nonsouthern, unified Democrats and unified Republicans are 1.71 (0.54) for mean expected revenues and 1.81 (0.80) for mean expected spending. Southern states in our data set include AR, FL, GA, KY, NC, OK, SC, TN, TX, VA, WV.

Estimation

Equations for gubernatorial and legislative elections occurring at the same time ("on-year" elections) were estimated using three-stage least squares (3SLS), with gubernatorial coattails in the legislative equation as the endogenous explanatory variable.[15] Since omitted variables would induce correlations between errors of both equations (in practice, these correlations had magnitudes up to 0.1), 3SLS was preferable to two-stage least squares. Midterm legislative election results are from a separate ordinary-least-squares regression.

EMPIRICAL TESTS

Due to the complexity of our hypotheses, we started with a comprehensive model and used specification tests to eliminate variables, rather than introduce a simple model and progressively add to it. This method helped us identify interactions that otherwise might not have been revealed. In particular, we find that electoral accountability for fiscal policy outcomes is evident but highly contingent on an elaborate pattern of partisan configurations and expectations.

As a first step, we modeled Democratic vote or seat share as a linear function of fixed effects, political control variables, macroeconomic outcomes, and a total of 24 fiscal policy coefficients: six fiscal policy variables (expected and unexpected revenues, expected and unexpected spending, surpluses, and deficits) interacted with four partisan configuration variables. Joint hypotheses tests comparing this model with reduced ones disclosed several results for gubernatorial election years. First, all balance terms (eight total) can be eliminated from the gubernatorial equation. Second, all scale terms (sixteen total) can be eliminated from the on-year legislative equation. Third, all scale terms involving divided governments or expected spending or revenues can be pooled into just three:

tween 5% and 10% smaller for last incumbency as compared to all past years since 1949.

[14] When we estimated models including expected fiscal balance (calculated analogously to expected scale) and distinguished between unexpected deficits and unexpected surpluses, the additional terms were not jointly significant in the regression, and the pattern of our other results did not change.

[15] We assume that the systematic component of elections for president and U.S. Senate depends on factors that are independent of the variables included in our equations, so those coattail effects need not be measured using instrumental variables.

Divided government (coded 1 for a Democratic governor and −1 for a Republican governor) times the sum of unexpected spending and revenues; unified government times the sum of expected spending and revenues; and divided government times the sum of expected spending and revenues. The first and second results suggest that voters punish legislatures for imbalance, but they punish governors for unexpected changes in scale. The third result suggests that voters treat Democratic and Republican governors differently when government is unified but the same when it is divided. These purely empirical results provide interesting twists to our expectations about the role of fiscal scale and balance in state elections.

In the interest of space, we do not include a results table for our initial model of on-year elections.[16] Instead, in the next sections we present and describe the main findings of the reduced model and review the specification tests we used to arrive at it. We also discuss the magnitude of the effects relative to electoral margins and midterm election results. We then use simulations to explore a key nonlinearity, that Democratic governors appear to be rewarded for small unanticipated fiscal scale increases while being punished for cuts and large increases. Finally, we review our conjectures about the effects of divided government and collective action, and we evaluate the robustness of the results.

Linear Model

Table 2 presents the results of the reduced linear model. The coefficients measure the marginal effect of each explanatory variable on the Democratic share of the major party vote for gubernatorial elections, or the corresponding share of seats for legislative elections. Fixed state effects and intercept shifts for presidential and Senate election years were estimated but are not shown. A high degree of multicolinearity between expenditures and revenues makes it impossible to disentangle satisfactorily all the individual effects of spending and taxes (see our discussion of robustness in Appendix B). Although we estimate separate coefficients for unexpected revenues and spending in our gubernatorial model and for all scale terms in our midterm legislative model, we show the effects of a unit increase in *both* revenues and spending in Table 2 to emphasize results that proved qualitatively stable across respecifications.

Political Controls. Incumbency effects (incumbent candidate, term limits, lagged seats) generally are significant. Candidates of each party running to succeed an incumbent from their own party do worse if the incumbent was term limited rather than retired for other reasons. An incumbent candidate of either party benefits by about twice as much (7.67%) as the incumbent party candidate who follows a term-limited governor loses (2.81–3.76%). Lagged seat share in the legislative equations has a large, significant effect. Presidential coattails are significant in the legislative equations, with Democrats gaining about one-third of a percent more seats for each Democratic percentage point of the presidential vote. We find no effects for U.S. Senate coattails in either on-year equation, although it has significant effects in the midterm elections for the state house, when the U.S. Senate race may be the only "statewide" contest. Governors have long coattails in on-year elections, with a magnitude of effect similar to that of the president. Apart from our estimate of presidential coattails in gubernatorial equations (still positive, but smaller and statistically insignificant), these results reinforce and extend Chubb's (1988) findings based on 1940–82.[17] The pattern of these results appears whether we include fiscal scale, balance, or both, regardless of how expectations are specified, and across several different estimation techniques.

Economic Variables. The coefficients of the macroeconomic variables (state income growth relative to national, separately by party of the governor) have the expected signs. In the gubernatorial equation, Democratic candidates get a vote share boost of .31 for each percentage point of relative state growth under Democrats, and they do worse (−.44 per point) when there has been growth under a Republican governor. The coefficients are each more than twice their standard errors. Almost regardless of specification, across many variations, the significant income effect for Democratic gubernatorial candidates and a more variable but always beneficial effect for Republicans hold. By contrast, the income effects in state legislative contests are inconsistent.

Scale. The estimates in Table 2 reveal a number of important insights. First, voters indeed appear to hold the party of the governor responsible for unexpected changes in fiscal scale; second, they do so in a partisan fashion unanticipated by theory; third, the extent to which they do so depends on whether government is divided or unified. If both revenues and spending are 1% of income below (above) expectations, then a Republican candidate for governor gains (loses) more than 4.5 percentage points of the vote following a period of unified Republican control. The opposite is true for Democrats, who gain just over one percentage point from surprise increases (although this is not statistically significant).[18] In contrast, the coefficient on

[16] Data and documentation verifying all estimation results, redefinitions, or respecifications mentioned but not presented here are available from the authors upon request.

[17] If we omit expectations and add the cases for which we lack data on a prior administration by the same party, the effect of presidential coattails in the gubernatorial equation increases fourfold.

[18] Individual spending and revenue coefficients (not shown in Table 2) follow a similar pattern but are less stable across the redefinitions and respecifications described in the section on robustness, below. For the model presented in Table 2, the coefficient (standard error) for unified Democrats × unexpected revenues is 2.50 (1.09); for unified Democrats × unexpected spending it is −1.47 (0.72). The sum of the two coefficients, 1.03, and joint standard error (0.84) are presented in Table 2. For unified Republicans the unexpected revenue and spending effects are, respectively, 7.05 (2.58) and −2.50 (1.86).

TABLE 2. Effects of Fiscal Policy Outcomes on the Democratic Share of Votes or Seats, 1968–92

Variable	Governor	State H.R. Seats, On Years	State H.R. Seats, Midterm
Political and economic controls			
Incumbent Candidate/lagged Dem. Pct. Legislative Seats	7.67*** (0.69)	.474*** (.046)	.413*** (.065)
Term-Limited Dem. Governor	−3.76* (1.51)	—	—
Term-Limited Rep. Governor	2.81 (1.72)	—	—
Dem. Pct., Votes for President	.141 (.140)	.461*** (.123)	.264** (.093)
Dem. Pct., Votes for U.S. Senate	.017 (.044)	.007 (.038)	.168** (.053)
Predicted Dem. Pct., Votes for Governor	—	.369*** (.075)	—
Relative Income Growth × Dem. Governor	.311* (.136)	−.129 (.121)	.217* (.086)
Relative Income Growth × Rep. Governor	−.446* (.220)	−.149 (.200)	.139 (.210)
Fiscal scale variables			
Unified Democrats × joint unexpected effects	1.03 (0.84)	—	−0.29 (1.24)
Unified Republicans × joint unexpected effects	4.56*** (1.07)	—	2.52 (1.47)
Divided Government × unexpected revenues + spending	0.46 (0.27)	—	—
Divided, Democratic Governor × joint unexpected effects	—	—	2.73** (1.02)
Divided, Republican Governor × joint unexpected effects	—	—	−0.84 (1.27)
Unified Government × expected revenues + spending	0.27 (0.24)	—	—
Unified Democrats × joint expected effects	—	—	0.27 (0.73)
Unified Republicans × joint expected effects	—	—	2.34* (0.98)
Divided Government × expected revenues + spending	0.42 (0.23)	—	—
Divided, Democratic Governor × joint expected effects	—	—	1.52* (0.69)
Divided, Republican Governor × joint expected effects	—	—	−0.49 (0.76)
Fiscal balance variables			
Unified Democrats × deficit	—	5.28* (2.07)	1.54 (3.01)
Unified Republicans × deficit	—	−5.79 (5.10)	6.17 (4.55)
Divided, Democratic Governor × deficit	—	7.88* (3.46)	−5.48 (7.22)
Divided, Republican Governor × deficit	—	3.38 (3.29)	8.40 (4.62)
Unified Democrats × surplus	—	−1.88 (1.19)	2.83 (3.14)

TABLE 2. (continued)

Variable	Governor	State H.R. Seats, On Years	State H.R. Seats, Midterm
Unified Republicans × surplus	—	22.30***	10.23
		(3.77)	(6.84)
Divided, Democratic Governor × surplus	—	1.40	−4.83
		(1.19)	(2.51)
Divided, Republican Governor × surplus	—	−1.90	2.84
		(1.78)	(3.48)
N of Cases	233	233	214
Adjusted R^2	.481	.861	.866
Standard error of the regression	5.99	5.24	5.91

Note: The dependent variable is the Democratic share of the major party votes for governor or seats in the lower house of the state legislature. Fixed state effects and intercept shifts are not shown. Deficits are coded as negative numbers. For interaction terms that do not specify party, fiscal policy outcomes are multiplied by 1 for a Democratic governor, −1 for a Republican governor. "Joint" effects of fiscal scale refer to the sum of separate coefficients for revenues and spending. The standard error of the sum is the square root of the sum of both coefficient variances and twice the covariance between the two coefficients.

Results for gubernatorial and on-year state house elections were estimated using three-stage least squares with SST v. 2.0. Results for the midterm state house elections were estimated using OLS with SST v. 2.0.

We anticipate that the signs of the coefficients will be negative for term-limited Democratic governor and relative income growth × Republican governor, positive for all other political and economic controls. We anticipate that the joint effect of unexpected increases in revenues and spending will be negative for unified Democrats, positive for unified Republicans, and zero (or at least weaker) for divided government. In a fully rational model, because expected scale (based on previous incumbents' performance) has been fully discounted into past voting, it would have no further effect on the current election. We simply report the empirical results we find. Anticipated signs for deficits (surpluses) are positive (negative) for unified Democrats, negative (positive) for unified Republicans, and zero (or at least weaker) for divided government.

Standard errors are in parentheses. *$p < .05$, **$p < .01$, ***$p < .001$; two-tailed test.

divided government is much smaller (and also not statistically significant).

The partisan flavor of these results is unexpected. In our model, partisan differences are encapsulated within the expectations term. We therefore anticipated a similar effect for both unified Democrats and unified Republicans. Instead, the results show strong partisan differences. Empirically, this could reflect many things, including voter uncertainty and learning, changes in the electorate, or even a "partisan" way of looking at fiscal policy, that is, Republicans not only are expected to provide fiscal restraint but also are rewarded when they do so, even after allowing for voters' partisan expectations. Of course, the partisan differences could reflect some deficiency in our measure of expectations. Yet, the basic pattern of the results is robust in virtually all variations in specification we tried.

More important, these results are inconsistent with Peltzman's view of uniform fiscal conservatism among voters. Because voters expect Democrats to provide greater scale, Republicans are punished for surprise increases, whereas Democrats are not. Each extra element in our specification—controlling for term limits, modeling expectations, including both taxes and spending, and considering differences between unified and divided control—facilitates the emergence of this pattern.

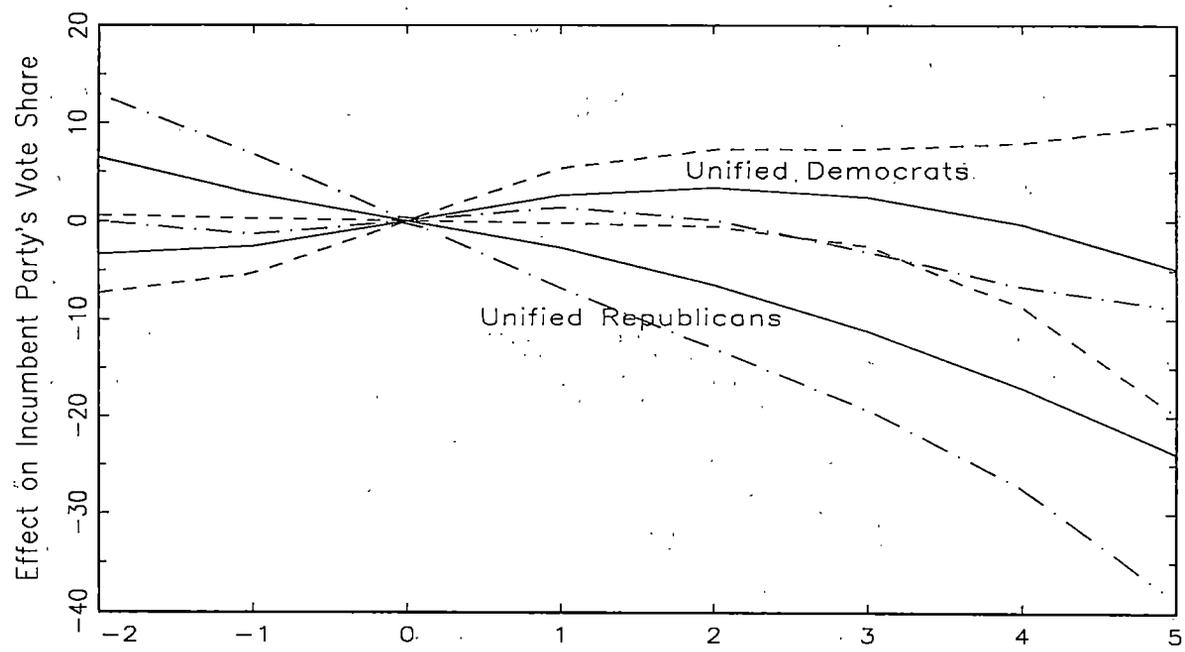
Magnitude of Effects. The following scenario illustrates the magnitude of these estimated results. Suppose a unified party government produces unexpected spending and unexpected revenues of at least 2.5% each. (Table 1 shows that this is about one standard deviation greater than the mean for our data set.) All else equal, the coefficients in Table 2 predict that after such an unexpected increase, the gubernatorial candidate of an incumbent Democratic Party will actually gain about 2.6% (2.5 × 1.03) of the vote, while an incumbent Republican Party will lose 11.4%. If the incumbent governor is ineligible for reelection, Democrats lose about 1.2%, while Republicans lose about 14.2%. The loss of legislative seats (due to the coattail effect) is predicted to be only 0.4% for Democrats and 5.2% for Republicans. Since the average margin of victory following a period of unified party government in our data set is 7.1% for the gubernatorial election but 17.4% in the legislature,[19] the predicted effect exceeds the average margin only for unified Republicans in the governor's race. Voter reactions to fiscal performance could thus easily contribute to the trend toward divided government documented by Fiorina (1996) and others.

Balance. Problems with multicolinearity are far less severe for fiscal balance (see Appendix B). Therefore, we consider the effects of deficits and surpluses separately. As mentioned above, we obtain the empirical result that fiscal balance does not affect the governor's race directly. Under unified government, however, both Democrats and Republicans lose seats in the state legislature when they run deficits.[20] The size of the effect is similar in both cases (a deficit that is 1% of state income costs either party around 5% of seats), but it is statistically significant only for Democrats. Surpluses, in contrast, strike unified Republicans much harder than unified Democrats. Results are generally much weaker under divided government, although Democratic governors with divided control appear to

[19] Average margin of victory is measured as the mean difference between the percentage of votes or seats received by the winning party and 50%.

[20] Recall that a negative deficit and a negative coefficient imply a positive effect (Democrats do better) on vote share.

FIGURE 1. Effects of Fiscal Surprises on Gubernatorial Elections



Note: A fiscal scale surprise of size *X* means that unexpected revenues and unexpected spending are both *X* percent of state personal income. Solid lines show the predicted effects of fiscal scale surprises on unified Democrats or unified Republicans based on the model in Table 2, but with party-specific scale coefficients for divided government times unexpected revenues or spending, and quadratic terms for all unexpected scale effects. Dashed lines indicate ± 1.96 standard errors. Thus, for a fiscal scale surprise of +2 under a unified Democratic administration, the predicted effect on the Democratic candidate for governor is 3.3, with a 95% confidence interval of −0.7 to 7.3. The figure was created using Gauss v. 3.1.

lose support in the state legislature when there is a deficit.

Specification Tests. As noted above, various specification tests support the results in Table 2. For the gubernatorial equation, no significant improvement is obtained by separating positive and negative surprises for unified governments. It is not significantly improved by further differentiation among the remaining pooled fiscal scale coefficients or by including any group of fiscal balance coefficients. (All remaining coefficients are at least as large as their standard errors.) In contrast, no further subset of fiscal scale coefficients significantly improves the fit of the legislative on-year model in Table 2, and no subset of fiscal balance variables can be excluded without significantly worsening the fit.[21] Finally, none of the three main groups of variables (unexpected and expected fiscal scale and fiscal balance) can be excluded from the midterm equation.

Midterm Elections. For the midterm equation in Table 2, the last incumbency expectations are the same as for the gubernatorial equation two years following, so the data are limited to 1990 or earlier. Empirical tests show that the parsimonious specification used in the on-year model results in significant loss of information when applied to midterm elections, so we estimate a full set of 16 scale coefficients and eight balance coefficients.[22] For simplicity, we again show the joint effects of revenues and spending. We find that unified Republicans are punished for unexpected increases (but not as severely as in gubernatorial elections), while Democrats are rewarded in the case of divided government. We find similar results for expected fiscal scale. The fiscal balance results are less consistent than in the on-year legislative elections: Neither deficits nor surpluses have a significant effect on unified Democrats, while unified Republicans are punished for surpluses but not deficits. On the whole, current policy does not appear to matter as much in midterm elections, perhaps because these are partly a rerun of the election two years before, and the incumbent administration has not yet been completely evaluated. The divided government results are mixed.

Nonlinear Results and Refinements from Simulations

Are the Effects of Fiscal Scale Increases Uniform? It seems unlikely that governors and parties would be rewarded for surprise cuts and increases of any size. Are there limits? Do voters reward small surprises but

[21] If the seven fiscal scale variables are excluded from Table 2, column 1, then the resulting *F*-statistic has $p < .006$. Once they are included, the *F*-statistic for excluding the eight balance coefficients has $p < .73$. Similar magnitudes (with opposite inferences) obtain in the legislative equation.

[22] The model including 24 fiscal coefficients fits significantly better ($p < .01$) than any model omitting one or more groups of our fiscal scale or fiscal balance coefficients.

TABLE 3. Simulation Results for Fiscal Scale

Partisan Configuration	Unexpected Spending and Revenues as a Percentage of Income						
	−2	−1	+1	+2	+3	+4	+5
Mean change in support for incumbent governor's party in the gubernatorial race (std. error):							
Unified Democrats	−3.3	−2.4	2.4	3.3	2.6	0.3	−3.5
	(2.1)	(1.4)	(1.4)	(2.1)	(2.6)	(4.2)	(7.4)
Unified Republicans	6.8	3.0	−3.0	−6.8	−11.5	−17.1	−23.5
	(3.3)	(2.1)	(2.1)	(3.3)	(4.1)	(5.2)	(7.5)
Divided, Democratic Governor	−2.4	−1.2	1.2	2.4	3.6	4.7	5.7
	(2.0)	(1.2)	(1.2)	(2.0)	(2.5)	(2.9)	(3.5)
Divided, Republican Governor	2.0	0.8	−0.8	−2.0	−3.5	−5.5	−7.8
	(2.3)	(1.6)	(1.6)	(2.3)	(2.7)	(4.3)	(7.5)
Probability incumbent governor's party loses votes:							
Unified Democrats	0.94	0.95	0.05	0.06	0.16	0.47	0.68
Unified Republicans	0.02	0.08	0.92	0.98	0.99	0.99	0.99
Divided, Democratic Governor	0.89	0.85	0.15	0.11	0.07	0.05	0.05
Divided, Republican Governor	0.20	0.31	0.69	0.80	0.90	0.90	0.85
Probability Republicans suffer a greater vote share loss in the governor's race:							
Unified Republicans	0.01	0.02	0.98	0.99	0.99	0.99	0.97
Divided, Republican Governor	0.11	0.19	0.81	0.89	0.95	0.97	0.95

Note: The on-year model from Table 2 is used but with party-specific coefficients for divided government times unexpected revenues or spending, and quadratic coefficients for all unexpected scale effects. Results are from 10,000 Monte Carlo simulations performed using Gauss v. 3.1. Simulated shocks assume unexpected revenues and unexpected spending each equal the given percentage of income. The relative frequencies of (unexpected revenues + unexpected spending)/2 in our data are as follows:

shock < −1.5	.013	1.5 ≤ shock < 2.5	.116
−1.5 ≤ shock < −0.5	.060	2.5 ≤ shock < 3.5	.056
−0.5 ≤ shock < 0.5	.369	3.5 ≤ shock < 4.5	.052
0.5 ≤ shock < 1.5	.313	4.5 ≤ shock	.021

punish larger changes? If so, then a nonlinear specification is needed, and the restrictiveness of the linear specification above could explain the insignificant joint results for unified Democrats. We modified the on-year model in Table 2 by adding a full set of quadratic terms for unexpected scale effects and (for divided government) by interacting unexpected spending and revenues with the governor's party. Figure 1 presents the results for fiscal scale shocks on unified incumbent parties in gubernatorial elections. (Note that the vertical axis measures the effect on the incumbent party vote share, not the Democrats' vote share.) It shows that Republican governors who also control the legislature benefit from cuts and lose from increases in a way that remains nearly linear. Conversely, Democratic governors are significantly rewarded for increases up to about 2% of income, beyond which the standard errors increase, and eventually it appears that voters dislike big surprise increases enough to penalize them. Unified Republican governors are punished more than Democrats for surprise increases ranging from about +2% to +4% of income, with symmetric effects (electoral gains) for simulated cuts beyond the range of observed data.

We can gain further insight into the precision of these estimates from simulations based on random draws from a multivariate normal distribution whose first moments are the full set of estimated coefficients and whose second moments are their variance-covariance matrix. The first two lines of Table 3 give the mean effects for unified parties and their standard errors from 10,000 random draws. For example, a 1% fiscal scale surprise (unanticipated increase of 1% in both revenues and spending) under unified Democrats increases Democratic gubernatorial vote share at the next election by 2.4% on average, with a standard error of 1.4. These results, naturally, strongly resemble the analytic calculations plotted in Figure 1.

We can also use the simulations to answer questions that would be more difficult to answer analytically. For example, how often would a fiscal scale surprise of a given size result in the incumbent party losing votes? As Table 3 shows, the probability that unified Democrats gain (lose) support in the governor's race following unexpected increases (cuts) in fiscal scale of 1% or 2% is .94 or greater, but the probability of their gaining support falls below .5 at surprise increases of +5 and larger. For Republicans the situation is reversed: They are very likely to lose from surprise increases but to gain from unanticipated cuts. Moreover, the difference in how voters treat the parties is even more clearly established: Unified Republicans are nearly certain ($p > .96$) to do worse (better) than Democrats for any increase (cut) of magnitude greater than +1. Moreover, in every specification variation that allows for a "partisan pattern," these qualitative features appear.

TABLE 4. Simulation Results for Fiscal Balance

Partisan Configuration	Unexpected Spending and Revenues as a Percentage of Income					
	−1	−.5	+.5	+1	+2	+3
Mean percent change in legislative seats of governor's party (std. error):						
Unified Democrats	−5.3	−2.6	−0.9	−1.9	−3.8	−5.6
	(2.0)	(1.0)	(0.6)	(1.2)	(2.4)	(3.5)
Unified Republicans	−5.7	−2.8	−11.1	−22.4	n.a.	n.a.
	(5.1)	(2.6)	(1.9)	(3.8)		
Divided, Democratic Governor	−7.7	−3.8	0.7	1.4	n.a.	n.a.
	(3.4)	(1.7)	(0.6)	(1.2)		
Divided, Republican Governor	3.7	1.8	0.9	1.8	n.a.	n.a.
	(3.3)	(1.7)	(0.9)	(1.8)		
Probability incumbent governor's party loses votes in the legislative election:						
	Any deficit			Any surplus		
Unified Democrats	0.99			0.94		
Unified Republicans	0.87			0.99		
Divided, Democratic Governor	0.99			0.12		
Divided, Republican Governor	0.14			0.16		

Note: n.a. indicates that our data do not include surpluses in this range for the party configuration indicated. Results are from 10,000 Monte Carlo simulations performed using Gauss v. 3.1. Relative frequencies of shocks in our data are as follows:

balance < −0.5	.082	1.5 ≤ balance < 2.5	.030
−0.5 ≤ balance < 0.5	.618	2.5 ≤ balance < 3.5	.026
0.5 ≤ balance < 1.5	.245		

Unified and Divided Government. Is this partisan pattern of accountability more clear when control is unified? Does divided government get in the way? We have seen several times that the estimates under divided government are less well determined than for unified government. We can clarify this with simulations using the same quadratic model. Table 3 indicates that the mean changes in governors' vote share under divided government preserve the sign pattern described above: Democratic governors are more likely to lose support from surprise cuts, while Republicans are more likely to lose from increases. The pattern of sizes of effects is stronger for unified than divided government. Moreover, the difference between parties (the extent to which Republican governors are likely to gain or lose support more than Democrats from surprise cuts or increases, respectively) is consistently smaller under divided government.

Governor and Legislature. Our results (scale effects in the gubernatorial equation and balance effects in the legislature, plus scale effects if the governor's coattails are considered) call into question our conjecture that collective action inhibits accountability. The top four rows of Table 4 analyze simulations from the quadratic specification for varying fiscal balance shocks. The main effects of balance all appear: Unified Democrats are punished for both deficits and surpluses, although the effects of surpluses are small. Unified Republicans are punished severely for surpluses and may also be punished for deficits, although significance for deficits is weak. Finally, Democrats lose seats in the legislature following deficits if there is divided government and a Democratic governor.

As the last panel of Table 4 shows, in three of four cases with unified government (but only one with divided government), the presence of a surplus or deficit leads to vote loss with probability greater than .93. This supports the argument that voters expect fiscal balance, but the support is less clear when the lines of accountability are blurred by divided government, not as a consequence of legislative contests. Analysis with seat share rather than vote share (this restricts data to 1970–89) changes the patterns and strengths of effects but not the pattern of signs. (See Appendix B.)

How Robust Are the Results?

The results for fiscal scale hold up when balance variables are included in the gubernatorial equation, and the effects of balance remain when we include scale terms in the legislative equation. When we exclude balance variables from the model completely, some scale effects appear in the legislative equation (independent of coattails). If we measure scale with only expenditures in the gubernatorial equation, then partisan differences become smaller but do not disappear, and the penalty for unified Republicans running surpluses becomes significant. (See Appendix B.) With few exceptions (noted below), these qualitative results are robust: Net of expectations, Republicans are penalized for surprise increases in scale and for surpluses; Democrats are penalized for deficits and rewarded (to some extent) for increases in fiscal scale.

Moreover, the overall pattern of results (and especially the partisan component) does not depend excessively on our choice of expectations mechanism or our measurement of fiscal policy. In general, because "all past years" expectations are less accurate, standard

errors in models using that formulation are larger.[23] When we omit expectations entirely and just investigate the effects of revenues and spending as income shares, the residual sum of squares for the gubernatorial equation rises by about 7%, but the overall contours of the result remain.[24] Similarly, if we investigate two-year percentage changes in revenues and spending without normalizing by income, the partisan pattern in particular shines through. Only when we include expectations *and* measure policy in terms of changes do we get no results. That model, which has voters judge performance in terms of the degree of deviation from the amount of change that might have been expected, is most likely too demanding on voter information. We have also done some replications varying spatial and temporal coverage, using a measure of balance that normalizes the difference between revenues and spending by general spending less federal transfers, and we have explored the extent to which fiscal scale reactions depend on whether economic conditions are good and whether the budget is in surplus or deficit. There is a hint that governors of both parties are more likely to be punished for increased spending when there are deficits, but even so, the overall pattern of results in Table 2 survives.[25]

One caveat is necessary. In Table 2, where such comparisons are appropriate, the unexpected scale components are more important and better determined than the corresponding expected components. This is consistent with a rational retrospective model in which information is limited. This difference does not hold up in some robustness checks. Indeed, the expected components are more important in midterm elections. Estimated effects of "expected" changes could be consistent with longer voter memories or misspecification of the expectations model.

Finally, to look for effects of fiscal institutions, we interacted the eight balance variables in the legislative equation in Table 2 with a dummy variable indicating the presence of a no-carryover law.[26] The legislative seat penalty received by the Democratic Party when Democratic governors run deficits (regardless of which party controls the legislature) is apparently confined to states with no-carryover laws, but the eight extra coefficients needed to capture this interaction do not jointly improve the fit of the equation significantly ($p < .34$), and estimates based on separate subsamples divided by no-carryover laws fared no better. We also added changes in the rating assigned to a state's 20-year general obligation bonds to our model of scale and balance, since under uncertainty voters may take cues from expert observers, such as bond rating agencies (Goldstein and Woglom 1992), and anecdotal evidence indicates that elected politicians worry about the message sent by a change in bond ratings (Briffault 1996; U.S. General Accounting Office 1993). We did not find any significant results, probably because there are very few cases of rating changes (19 negative and 20 positive ones) in our data.

DISCUSSION

Electoral accountability for fiscal policy outcomes is contingent on patterns of partisan control in several ways. There is evidence that voters react differently to increases in spending and revenues, depending on which party controls the government. We find that Democrats are expected to provide a larger public sector, a factor omitted in the literature, and may even be rewarded for unexpected, moderate increases in fiscal scale under unified Democratic government, with opposite effects for fiscal cuts; the results are symmetrically opposite for Republicans. We find support for the conjecture that accountability for state-level policy is stronger when one party controls the executive and legislative branches. There is also evidence that voters prefer a balanced budget to either deficits or surpluses and that both parties are punished for fiscal imbalance.

Less expected was the finding that fiscal scale variables are more important in gubernatorial elections and fiscal balance in concurrent legislative equations. The bottom line appears to be that voters expect different things from governor and legislature. They expect governors to carry out Democratic and Republican budgets as appropriate, and they penalize the governor's party in the legislature if the budget is mismanaged. After all, the legislature does pass the budget, and under unified control the governor's party bears clear responsibility when the budget does not balance. It is puzzling that the governor seems able to avoid a direct penalty for fiscal imbalance, and our new finding regarding legislative contests deserves further research.

Our results both support and contradict earlier work. We replicate, extend, and qualify the findings of Chubb (1988) and Besley and Case (1995a, 1995b) on coattails, incumbency, income effects (at least state-specific ones), and term limits. We have new, party-specific results for fiscal scale that reflect our distinction between parties and a model that includes expectations, controls for term limits, and does not try to separate revenue and spending changes from each other. We also show that while the Powell-Whitten results about diluting accountability resonate with ours, countries such as the United States cannot be coded one way or

[23] For instance, in a close replication using all past years expectations, the fiscal scale effect for unified Republicans is 5.2, with a standard error of 1.4. Qualitatively, inferences are unaffected.

[24] Since we cannot find an effect for expectations on fiscal balance, which counts more in the legislative equation, that equation is unaffected. All the main results below are also robust to limited experiments with expectations based on average performance by parties across all states, a means of allowing for voter mobility.

[25] Eliminate the South, and the effect of term limits disappears, but the party-based scale and balance coefficients are similar to those in Table 2. Half the cases of unified Republican government are in 1968–74, so eliminating the early years matters to those coefficients and standard errors, but eliminating the last four years, when there was a significant recession, does not alter the results in any important way.

[26] Laws limiting the ability to carry deficits into the next fiscal year are stronger than those that merely require politicians to propose or adopt a budget projected to be in balance (Poterba 1996), and states with such laws pay lower interest rates on general obligation bonds (Bayoumi, Goldstein, and Woglom 1994; Eichengreen and Bayoumi 1995; Lowry and Alt 1997).

the other (as they do). Powell and Whitten (1993, 406) classify the United States as a country in which responsibility is relatively clear, but we show that accountability varies over time and is less clear when different branches are controlled by different parties. Our findings imply that any attempt to generalize about accountability from studies based on parliamentary systems is likely to be flawed, given the popularity of separately elected executives in non-OECD democracies (see Jones 1995).

Our results are quite different from those of Peltzman (1992). We find that voters do not uniformly punish all spending increases. They are far more discerning: Their reaction to fiscal outcomes depends on the party in office and their ability to assign responsibility for outcomes. Our results are consistent with the growth in state budgets over the past half century, with the electoral success of Democrats who are rewarded for providing such growth, and with the increase in divided government in the states. Our findings also suggest that partisan differences are more pronounced than earlier studies indicated.

With regard to balance, the main positive theory explanations for deficits are either Ricardian equivalence or collective responsibility problems, with the executive being the one who has an incentive to maintain some kind of balance. Our results seem to say that Ricardian equivalence does not apply, since voters are not indifferent. They like balance, neither net public saving nor deficits. They punish fiscal irresponsibility in a collective way by weakening the governor's party in the legislature, as though desiring to divide government, which in turn could lead to larger deficits (Alt and Lowry 1994). While the results are not as consistent as we would like, they cut against explanations for changes in government debt that rely on selfish or myopic voters (see Alesina and Perotti 1995).

Taken as a whole, this study suggests the outlines of an explanation for the paradox of continued government growth in the face of rational electorates who punish fiscal scale increases. This explanation consists of three parts. First, voters do not punish fiscal excess per se but deviation from expected performance. Furthermore, when performance expectations are formed retrospectively, and different parties have tended to pursue different policies in the past, voters are likely to hold varying expectations about future performance. It therefore seems likely that voters will evaluate the same fiscal performance differently according to which party produced it. Democrats are expected to spend more than Republicans and are punished less when they do so. Republicans are expected to work toward smaller government and to return to voters in the form of tax cuts the excess funds that result from cutbacks in government programs. They are punished when spending increases or when surpluses are not returned. One might speculate that in both cases the voters with stronger expectations of party policy account for the bulk of missing votes or defections, but that issue remains to be investigated. Second, voters punish outcomes only when they are able to establish clear responsibility for them. Consequently, they are more likely to punish unified governments than divided ones. Third, within the subset of unified governments, the executive branch bears the brunt of voter discipline for unexpected changes in fiscal scale.

The above factors can interact over time in ways that result in ever-growing levels of expenditure, despite rational, sophisticated voters who might prefer restraint. Our results suggest that voters most consistently punish one type of political configuration for high levels of spending: unified Republicans. When these administrations increase expenditures in ways that violate voter expectations, voters are placed in a difficult position, for they can only punish such outcomes by not supporting Republicans. Because our dependent variable is the Democratic *share* of major party votes or seats, even abstention by former Republican voters helps Democratic candidates. Thus, despite voter interest in fiscal restraint, expenditure increases.

Of course, one way to mitigate this "hair of the dog" problem is to make an incomplete switch—change the governor but leave the legislature the same—thus creating divided government. Voters may not do this deliberately. Burden and Kimball (1998) argue from evidence on ticket splitting in national elections that divided government is a by-product of lopsided congressional campaigns. We suggest that it may result from differences in the extent to which voters hold different officeholders accountable for particular outcomes. Divided government, in turn, has weaker accountability. Moreover, our results show that in many circumstances candidates for *either* branch are less affected by further fiscal changes if the incumbent government is divided. Then changes in the size of government will not be rewarded or punished by voters until some other factor brings about another period of unified party control. These other factors are outside our model but may be identifiable through further research.

While voters hold politicians responsible for fiscal outcomes, it appears they are more restrained in their tendency to punish than suggested by some earlier studies. Not all increases in expenditure are punished, only those that deviate from expectations and only when clear responsibility can be established. This is likely to be a small subset of cases. Thus, precisely because voters are sophisticated and selective in the way in which they react to fiscal outcomes, many spending increases go unpunished. In the long run, this may result in rising levels of expenditure.

APPENDIX A. STATES AND YEARS IN THE DATA SET

State	Years	State	Years
AR*	1986–90[a]	NC*	1968–72, 1978–92[d]
AZ*	1970–88[b,c]	ND	1968–92
CA	1968–90	NJ*	1969–89
CO*	1968–90	NM*	1968–90
CT	1968–90	NV	1968–90
DE	1968–92	NY	1968–90
FL*	1972–90[d]	OH*	1968–90
GA*	1972–90[d]	OK*	1968–90
HI*	1968–90	OR	1968–90
IA*	1974–90[e]	PA*	1968–90
ID*	1968–70, 1976–90[d]	SC*	1968–74, 1980–90[d]
IN*	1968–92	SD*	1974–90[e]
IL	1968–90	TN*	1968–70, 1976–90[d]
KS*	1974–90[e]	TX	1974–78, 1984–90[e,d]
KY	1973–91[d]	UT*	1970–92[b]
MA	1968–92	VA	1969–91
ME*	1968–74, 1980–90[d,f]	WA	1968–90
MI*	1968–90	WI	1968–92
MN	1976–90[g]	WV*	1968–92
MO*	1968–72, 1978–92[d]	WY	1968–90
MT	1968–92		

* A balanced budget law limits the ability to carry deficits forward (Advisory Commission on Intergovernmental Relations 1987).
[a] Four-year gubernatorial terms began 1986.
[b] Four-year gubernatorial terms began 1970.
[c] The 1990 election is omitted because the governor elected in 1986 was impeached.
[d] Expectations are not available for one gubernatorial cycle because there was no earlier administration of the same party since 1949.
[e] Four-year gubernatorial terms began 1974.
[f] Independent incumbent governor, 1975–78.
[g] Nonpartisan legislature, 1968–74.

APPENDIX B. FURTHER EVIDENCE ON ROBUSTNESS

Multicolinearity

Multicolinearity could pose problems for our results in two places: between surpluses and unexpected tax increases and between revenues and spending. In reality, the latter is a much larger issue than the former. On average, correlations between deficits or surpluses, on the one hand, and unexpected revenues or spending, on the other, are about .35. In contrast, colinearity of revenues and spending is much greater, which makes it impossible to disentangle satisfactorily individual effects of spending and taxes. For example, when each of the unanticipated and expected revenue and spending components underlying Table 2 is regressed on all other explanatory variables in the gubernatorial election equation, the resulting R^2 values vary from .72 to .98, so the individual coefficient standard errors can become large. For the cases in the equation in Table 2, the simple correlation between general revenues and spending is .97. The correlations between unanticipated revenues and spending vary from .73 to .94 across partisan configurations and are even higher for expected components.

We prefer strategies other than artificially trying to reduce the effect of multicolinearity on the results, and so we did not try to choose between revenues and spending, which could introduce bias by omitting a relevant variable or including an unwarranted restriction. Hence, we choose to estimate separate effects but describe the effect of a unit increase in both unanticipated revenues and spending. If revenue variables are excluded, then the partisan pattern weakens but does not disappear. For example, the simulation shown in Table 3, including both revenues and spending in the model, yields probabilities that unified Republicans suffer a greater vote share loss in the governor's race after a positive scale shock of 0.92–0.99. Repeated with expenditure variables only, the probabilities fall to 0.68–0.73. This strengthens our belief that omitting revenues is a misspecification of the model and that the distinction we make and employ between scale and balance is valuable.

Legislative Seats and Votes

We estimate the model shown in Table 2 for gubernatorial votes and legislative seats primarily because the seat data are available for a longer period. Whether seats or votes are used as the dependent variable does affect the results for the on-year legislative election, but only partly because of differing numbers of cases. Generally, the results are smaller with votes: The mean change in legislative seats of the governor's party reported in Table 4 is 20% smaller for unified Democrats and two-thirds smaller for unified Republicans, but all coefficients more than twice their standard errors remain so, whichever variable is used. Unlike Table 4, with vote data there are no significant results under divided government.

In contrast to Table 2, neither income nor governors' coattails affect the on-year legislative vote. With seats as the dependent variable, the same 181 cases (that are available with vote data) do produce a significant coattails coefficient. The coattails effect on seats but not votes could mean that a strong gubernatorial candidate willing to stump for legislative candidates is enough to push a few close races over the top but does not have a large overall effect on voting. Similarly, none of the scale or balance results are significant for the midterm legislative elections when we use vote share. The on-year legislative results hold up when we use vote share, so this is a difference between on-year and midterm elections.

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Organized Interests and the Decision of Whom to Lobby in Congress

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In a departure from previous research, we focus on the dyadic relationship between lobbyists and committee members in the House of Representatives in order to test hypotheses about what factors shape the decisions of individual groups to lobby individual committee members. Our primary assumption is that organized interests seek to expand their supportive coalitions and affect the content and fate of bills referred to committees. In order to accomplish these goals, they give highest priority to lobbying their legislative allies in committee; allies may lobby other members of Congress on a group's behalf and shape legislation to conform with a group's preferences. But organizations with access to a strong resource base can move beyond their allies and work directly to expand support among undecided committee members and legislative opponents. Our empirical analysis provides evidence to support our expectations.

Organized interests' decisions about whom to lobby in Congress go to the heart of their efforts to influence the legislative process. If opposing or undecided legislators are targeted, then lobbyists can affect overall support for or against a policy proposal (Austen-Smith and Wright 1994; Schattschneider 1975). If mainly allies are targeted, then the influence of lobbying may be more subtle as organizations reinforce the preferences of friendly legislators (Kingdon 1989) and mobilize participation in the legislative process on their behalf (Hall and Wayman 1990). But despite the attention devoted to whom groups lobby, tests of competing hypotheses about why and when specific legislators are targeted have been incomplete. Specifically, previous work has not examined simultaneously the full set of forces purported to shape target choices, a limitation that results from the analytic approaches of these studies.

We use data from a survey of organized interests to examine the dyadic interactions between lobbyists and committee members in the House of Representatives on four issues considered during the 104th Congress (1994–96). Our focus on dyadic relationships allows us to test hypotheses about how the decisions of individual groups to target individual committee members are shaped by a combination of legislator, issue, and organization-specific characteristics. In another departure from previous research, the hypotheses are rooted in our assumption that groups have two primary legislative goals when a bill is referred to committee: (1) expand the size of their supportive coalition and (2) affect the content and fate of proposed legislation. Our empirical analysis reveals that, other things being equal, organized interests are more likely to lobby their allies in committee than their opponents or undecided members. This tendency is consistent with a group's desire to shape the content of legislation and mobilize legislators to act on its behalf, but not with efforts to counteract lobbying by its organizational opponents. A group moves beyond its allies and lobbies legislators who are not among its usual supporters only when it has strong ties to their legislative district. It then can work directly to build coalitions in Congress in addition to relying on legislative friends.

Our purpose is to integrate three different perspectives on lobbying that have been pursued largely in isolation of one another. One approach uses individual legislators as the unit of analysis and examines characteristics (such as their initial policy preferences and their power in Congress) that determine whether they will be the targets of interest group lobbying. Researchers taking this approach have investigated why organized interests expend effort to lobby legislators who are already inclined to support their interests (e.g., Ainsworth and Sened 1995; Baumgartner and Leech 1996; Denzau and Munger 1986; Hall and Wayman 1990), as well as whether the tendency to lobby allies is a dominant strategy for groups, as suggested by Bauer, Pool, and Dexter (1963) (e.g., Austen-Smith and Wright 1994, 1996; Rothenberg 1992; see also Kollman 1997).

A second approach uses the interest group as the unit of analysis and studies how its resources shape the extent to which it engages in different forms of advocacy. Several scholars have examined how organizational resources (including staff, outside financial support, and an active membership) enhance a group's ability to pursue a successful Washington-based lobbying strategy (Bacheller 1977; Gais and Walker 1991; Schlozman and Tierney 1986; R. Smith 1984). In addition, economic models of interest group behavior often focus on the financial and human resources that

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We appreciate thoughtful comments from Laura Arnold, Frank Baumgartner, Scott Bennett, Michael Berkman, Bill Bianco, Jan Box-Steffensmeier, Greg Caldeira, Beth Leech, Lettie McSpadden, Herb Weisberg, and Jack Wright. We also are grateful to Sarah Poggione, Killian Seper, and especially Nancy Wiefek for research assistance during various stages of this project. Our appreciation also extends to the many organization representatives who provided us with information for this project. The names of the authors are listed alphabetically. A version of this article was presented at the 1996 American Political Science Association meetings in San Francisco. This research was supported by a faculty research award from the Research and Graduate Studies Office, College of the Liberal Arts, Pennsylvania State University.

groups devote to influencing government policy (see, e.g., Mitchell and Munger 1991; Olson 1982).

A third perspective examines how issue context shapes the lobbying strategies and effectiveness of interest groups. For example, Bacheller (1977) investigates how lobbying activities vary depending on the public salience of issues and the level of controversy they engender, and Richard Smith (1984) argues that the ability of lobbyists to influence congressional decisions is affected by the amount of time Congress deliberates on an issue.

Each of these vantage points has informed the study of lobbying, but no work has integrated all three perspectives. In a departure from previous studies, our use of the organization-legislator pair as the unit of analysis and our focus on multiple issues allows us to investigate how the choice of lobbying target is affected by interest group resources, legislator characteristics, the interaction of these two sets of forces, and issue context.

Our study also continues a recent effort to examine interest group activity in congressional committees (C. L. Evans 1989; D. Evans 1996; Hall and Wayman 1990; Hansen 1991; Wright 1990). Until recently, most studies of interest group lobbying focused on the floor stage of the legislative process, when the agenda is often fixed or narrow. A bill's content often has been determined by the time a proposal reaches the full chamber, since most of the work of crafting legislation takes place in committees (Smith and Deering 1990), and committee bills frequently go unchallenged on the floor (C.L. Evans 1989; S. Smith 1989). Because of an organized interest's potential to shape the legislative agenda by lobbying in committee, many argue that interest group influence is strongest during this stage of the legislative process (Austen-Smith 1993; Hall and Wayman 1990; Ripley and Franklin 1980; Schlozman and Tierney 1986; Smith and Deering 1990).

In the section that follows, we review research about how legislator characteristics and group resources affect lobbying efforts. We then state our assumptions about the goals groups pursue when they lobby and present a conceptual model that illustrates how organizations' lobbying decisions during the committee stage are affected by legislator characteristics, organizational forces, and issue context. Next, we describe the data and methods used in our research and specify the statistical model of groups' lobbying decisions at the committee level. The results of our investigation and a discussion of some of the implications follow.

PREVAILING PERSPECTIVES ON LOBBYING STRATEGIES

Most scholars agree that organized interests have incentives to lobby fence-sitters or "swing" legislators, that is, those who are undecided or neutral on an issue (Denzau and Munger 1986; Langbein 1991; Wright 1990).[1] Groups can anticipate a high payoff from lobbying fence-sitters because these legislators are most susceptible to persuasion (Rothenberg 1992; R. Smith 1984). Many researchers also suggest that groups lobby influential legislators (e.g., members of key committees, committee and subcommittee chairs, and party leaders) because they can set the political agenda and serve as cue givers for their colleagues (Austen-Smith and Wright 1994; Kingdon 1989; Matthews and Stimson 1975; Rothenberg 1992; Wright 1990).

But the literature also contains several conflicting ideas about the congressional lobbying strategies used by organized interests (e.g., Austen-Smith and Wright 1996; Baumgartner and Leech 1996). Although most scholars agree that groups lobby their friends in the legislature, there is disagreement about why groups should expend resources and effort on their regular supporters. One common thread in this body of work is that legislators have the upper hand in relationships with interest groups (Bauer, Pool, and Dexter 1963; Dexter 1969; Hayes 1981; Matthews 1960; Milbrath 1963; Ziegler 1964), granting access primarily to people who share their predispositions and to groups with whom they have established relationships (Ainsworth 1993, 1997; Hansen 1991; Milbrath 1963). Another explanation is that allies in Congress can be enlisted to act as agents on a group's behalf by lobbying colleagues, sponsoring legislation or amendments, keeping an issue off the agenda, and modifying the language of a bill (Denzau and Munger 1986; Hall and Wayman 1990).[2]

Others argue that groups lobby allies in Congress to counteract the efforts of organizational opponents. The formal model developed by Austen-Smith and Wright (1992, 1994) predicts that, other things being equal, groups are expected to lobby members of Congress who are inclined to oppose their interests in order to persuade them to take the group's position. In essence, a group has no incentive to devote scarce lobbying resources to legislators who are inclined to support it unless those legislators are contacted by the group's organizational opponents.[3]

[1] Researchers have defined fence-sitters in a variety of ways. Rothenberg (1992), for example, defines them as legislators whose prior probability of voting for a bill is near the midpoint (i.e., $p = .50$). At least implicitly, Austen-Smith and Wright (1994, 1996) appear to view them as legislators whose prior probabilities of voting are indeterminate (i.e., not more or less than .50). Langbein (1991) equates uncertainty with ideological moderation. In general, the various labels—fence-sitter, swing, and undecided—are used to refer to members of Congress who, for whatever reason, are perceived as having ambiguous positions on issues.

[2] Hall and Wayman's (1990) analysis shows that PAC contributions given to supporters have a substantial effect on supporters' actions in committee. In contrast, contributions play almost no role in demobilizing the opposition.

[3] In addition, Kollman (1997) argues that organizations have little choice but to lobby committees ideologically similar to them because groups active in an issue area tend to have the same ideological biases as the committees with jurisdiction over the legislative proposals in that domain. Although Kollman finds a similarity between the aggregate ideology of groups lobbying the same committees, his data also show that more diverse committees attract the lobbying attention of a more diverse array of groups. Because there is a fair amount of ideological diversity on any congressional committee, the question of whom groups lobby in committee remains important. Kollman's data do not indicate which committee members groups choose to lobby.

To some extent, researchers' expectations about groups' incentives for targeting allies, fence-sitters, and foes are rooted in different interpretations of what it means to lobby and what groups are trying to accomplish at different points in the legislative process. For instance, many studies examine lobbying on roll-call votes with a fixed agenda, at which stage groups can only seek to influence passage or defeat of a bill or motion (Austen-Smith and Wright 1994; Langbein 1991; Rothenberg 1992). If all that is at stake is the outcome of an up or down vote, with no opportunity for legislators to manipulate the agenda or alter proposal content, then the operative goal for organizations will be to expand their coalition to winning size. That is, groups attempt to convert opponents and swing legislators while minimizing defections by likely supporters. In this context, it may be reasonable to define lobbying solely in terms of efforts to reinforce or change legislators' policy positions (Austen-Smith and Wright 1994, 1996).

At points in the legislative process preceding final floor action, persuading legislators to vote a particular way is only part of what groups attempt to accomplish by lobbying. At the agenda-setting and committee mark-up stages, a multitude of decisions about the content of legislation often are made without a vote (Light 1985; Schneier and Gross 1993; Smith and Deering 1990). By enlisting a set of agents to act on their behalf, lobbyists may be able to achieve what they need without much notice and without having to expand their supportive coalition. It is much more difficult for groups to insert their preferences into legislation at later stages (Hall and Wayman 1990). During points in the legislative process when the agenda is fluid, a broader conceptualization of lobbying goals is appropriate; a group will encourage legislators to sponsor favorable legislation, craft legislation that conforms to its interests, change the wording of a bill, and give prominent place on a committee's agenda to favorable bills and amendments.[4]

Aside from studies that focus on legislators' characteristics, research on how groups' advocacy efforts are shaped by their resource capabilities has a long tradition. An investigation of lobbying on trade policy in the 1950s (Bauer, Pool, and Dexter 1963) found that a lack of resources limited organizations' effectiveness as advocates. More recently there is evidence that a group is likely to target legislators from states or districts where it has a strong organized presence (Austen-Smith and Wright 1994; Rothenberg 1989; Wright 1990). In addition, Wright (1996) argues that legislators grant access to groups that contribute PAC funds because, through their PACs, these groups are more likely than noncontributors to acquire information that will be useful to the legislator.

The amount of personnel an organization devotes to advocacy also may affect its capacity to lobby (Gais and Walker 1991). A single lobbyist will not be able to contact as many legislators as will a staff of ten. Yet, some scholars believe that even a relatively small budget and staff may be used effectively if a group works with other organizations as part of a coalition (Berry 1989; Schlozman and Tierney 1986).

In addition to what is known about direct linkages between resources and lobbying effort, Richard Smith (1984) argues that the quantity and quality of a group's resources affect its ability to formulate arguments that will be compelling to members of Congress. Organizations with a qualitative and quantitative resource advantage over opposing groups are expected to have a greater chance of building and maintaining a supportive coalition over the course of the legislative process. Thus, Smith argues that these organizations will be better able to manipulate the policy positions and interpretations of legislators.

Empirical studies, however, have not assessed how organizational resources affect a group's decision to lobby legislators with different issue positions. Research that focuses on whom groups lobby relies on data about individual legislators to make statements about groups' target choices. For example, Rothenberg (1992) examined which legislators were lobbied by Common Cause, so that group-related determinants of the lobbying decision could not be assessed. Austen-Smith and Wright (1994, 1996) investigated the lobbying activities of multiple groups but only those acting in coalition. This focus on coalitional activity does not allow an assessment of how individual group characteristics affect the lobbying choice or how group and legislator characteristics interact to shape the decision.

By the same token, research on the relationship between organizational resources and lobbying does not explore how resources affect whom groups lobby but rather how resources affect the techniques organizations use to achieve their objectives (Bacheller 1977; Gais and Walker 1991). Only Richard Smith (1984) suggests that resources have implications for a group's ability to influence different legislators and build coalitions in Congress.

HOW ORGANIZATIONS DECIDE WHO TO LOBBY IN COMMITTEE

The definition of lobbying used here is rooted in our assumption that an organized interest tries to accomplish two main legislative tasks when a bill with implications for its interests is referred to a congressional committee.[5] First, the group seeks to maintain and expand the size of its supportive coalition. If it wants a

[4] Many scholars in addition to Hall and Wayman (1990) and Denzau and Munger (1986) define lobbying in relatively broad terms (Berry 1989; Gais and Walker 1991; Jewell and Patterson 1986; Schlozman and Tierney 1986; but see Austen-Smith and Wright 1994). This broader view of lobbying also is supported by numerous anecdotes of lobbyists drafting legislation for members of Congress (Abramson 1995; Birnbaum and Murray 1988, 121–2; Cushman 1995; Drew 1996, 114–7; Egan 1995; Engelberg 1995; C. L. Evans 1989; Pear 1996) and even briefing legislators and their staff on proposed legislation (Judis 1996).

[5] Of course, groups will not try to accomplish these two tasks on every bill referred to committee. A proposal is most likely to merit their attention when it is likely to receive attention in committee and when it has a reasonable chance of moving out of committee.

bill passed (or defeated), then it will need a sizeable number of legislators to accomplish this feat. In order to form a winning legislative coalition, it needs to cast its lobbying net as widely as possible and target traditional opponents as well as undecided members in an effort to persuade them to support the group's interests.

Second, organizations seek to affect the content and fate of proposed legislation (Hall and Wayman 1990). Interest groups are not allowed to introduce legislation, offer amendments, or otherwise participate formally in drafting legislation, so they need to find legislators who will carry out those tasks. Organizations also have incentives to encourage legislators to negotiate and bargain with other committee members in order to enhance support for proposals that reflect their interests, shield proposals from opposing interests, and ensure that proposals move swiftly out of committee.

A group that enlists informed and vigorous advocates for its interests is likely to have a greater chance of achieving its policy objectives than a group that seeks only to persuade legislators to support its interests and, therefore, presumes that the emerging legislation will be consistent with its objectives. Active legislators can have a substantial effect on proposals that are considered in committee. As Hall (1989) discovered in his study of three House committees, between 34% and 45% of the committee members either voted by proxy or failed to vote at mark-up hearings. Thus, "proxy voting tends to augment the power of the active, not preserve the authority of the absent," because a proxy cannot object, compromise, or offer an amendment (Hall 1989, 204).

In contrast to the way lobbying is viewed in many studies, our conceptualization allows for the fact that groups may contact legislators to persuade them to support a particular position *and* to shape the content of proposals that affect an organization's interests. To influence legislators' voting decisions, groups provide information about the pros and cons of a piece of legislation and its electoral consequences, but they also submit draft legislation and amendments as well as other facts, statistics, and arguments to help legislators participate both in bill formulation and coalition building on behalf of the group. Many of the groups in our study acknowledged engaging in activities such as drafting legislation (46%) and enlisting committee members to lobby other legislators on the group's behalf (77%), which implies that the goals of shaping legislation and mobilizing legislators are important components of the lobbying enterprise.

We recognize as well that these two goals (coalition building and affecting legislation) are related. For example, some groups may choose not to work directly to expand the size of their legislative coalition if they can successfully alter the content of a bill. Alternatively, a group may need to accept a less than optimal bill in order to gain the support of some legislators and thus increase the size of its coalition. Groups may use less important provisions of a bill as bargaining chips to attract support, and they may obtain supporters for a bill by bargaining over the provisions of future legislation.

Thus, we define lobbying as providing information directly to members of Congress (or their staff) to further the two goals described above.[6] We believe that this definition characterizes what groups do and what they hope to achieve by applying their resources to advocacy.

FORCES THAT INFLUENCE WHO GROUPS CHOOSE TO LOBBY

We believe that an organization's choice of whom to lobby will vary depending on the characteristics of legislators (including their prior issue positions), the resources available to the interest group (i.e., its capacity for lobbying), and the context of the issue. Three points underlie our conceptual argument. First, all else the same, a group will target committee members based on the likelihood of those legislators acting on the group's behalf in committee as well as later in the legislative process. Second, because organized interests differ in their capacity to lobby, the approaches they take to coalition building in Congress will differ. Third, lobbying strategies are expected to differ across policy issues depending on the importance of the issue to an organization and the time available for advocacy.

Legislative allies are expected to be the most important targets of a group's direct lobbying efforts in committee, regardless of whether allies are the targets of opposing organizations' advocacy efforts. A group lobbies its likely committee allies in order to provide them with arguments, issue interpretations, and information that make the strongest case for the group's position.[7] With these data, legislators with policy preferences similar to those of the group may lobby on the group's behalf, may push favorable amendments and repel unfavorable amendments in committee to strengthen the content of a bill, and may negotiate with their colleagues so that the proposal which emerges from committee has the best chance of survival on the floor (Hall and Wayman 1990).[8] If legislators are to help control the agenda and tailor legislation for an

[6] In this way, our definition includes lobbyists meeting with legislators to confirm their support for the organization's interests, persuade them to adopt the organization's position, and ask them to work on the organization's behalf in committee (e.g., contact other legislators, introduce amendments, and change the wording of a key provision). Because the incentives for groups to contact legislators through the grassroots differ from the incentives groups have to lobby through their offices in Washington, lobbying through organization members is not subsumed under our definition (Hojnacki and Kimball 1998).

[7] Indeed, how an issue is interpreted, or the "spin," affects "the dimensions of judgment on which the issues and policies are evaluated" (R. Smith 1993, 184) and, potentially, the ultimate outcome of the issue debate (McKissick 1995; Riker 1986; Schattschneider 1975; R. Smith 1984, 1993).

[8] Legislative allies also may ask questions and make arguments during hearings, submit statements for the hearing record or committee report, and request information from executive branch agencies, the Government Accounting Office (GAO), or the Congressional Research Service (CRS) on a particular topic or agency. In general, legislators can act in a variety of ways as groups' agents in committee.

interest group, they need information from the group to parry the arguments of legislative foes.

A group also may lobby legislative opponents and undecided committee members. The latter are second only to legislative allies in their potential to be susceptible to the information and arguments a group provides. The former present a more difficult challenge. During the initial stages of the legislative process, efforts to alter the position of opponents and undecided legislators may not be an efficient strategy. This is especially true for committee members, who are likely to have expertise on an issue. For these reasons, we believe that a group's probability of lobbying an opposing or an undecided committee member is conditional; only those organizations with a strong resource base (such as diverse district ties) will have the capacity to move beyond their allies and lobby opponents and undecided legislators.

Finally, we believe that groups will assign high priority to committee leaders, party leaders, and members of subcommittees with jurisdiction over the relevant proposal. Aside from their potential role as information providers and cue givers for other legislators, these key figures have unique opportunities to shape the content and affect the fate of legislative proposals (see Schneier and Gross 1993).

When groups make decisions about how to build support for their policy objectives in congressional committees, they also must consider what they are able to do, given the issue at hand. Consistent with the literature, we expect that an organized interest will be more likely to lobby any committee member if it is part of an advocacy coalition, has an active PAC, and has a large lobbying staff. In addition, we anticipate that legislators are especially likely to be lobbied by groups with a base of support in their district.

More important, we expect that groups with a comparative advantage in resources will be more likely to move beyond their allies and lobby opposing and undecided committee members. Organizations with fewer resources will be more likely to concentrate solely on their allies in committee. Allies are a better investment for these groups because friends in committee may lobby other legislators on a group's behalf, and they are more likely to be vigorous supporters as proposals move through the legislative process.

The character and context of an issue as well as the nature of congressional action also will influence lobbying decisions. First, other things being equal, we expect that a group will try to lobby more legislators on important issues, when the stakes are high. Second, the amount of time Congress spends on an issue, either in committee or on the floor, influences the ability of organized interests to lobby in Congress (R. Smith 1984). Issues that are well defined or assigned priority by congressional leaders before the session begins are often acted upon quickly, so groups have less time to lobby and may not be able to contact as many legislators as they would like. For example, on issues contained in the Contract with America, such as crime and product liability, the House passed legislation within the first 100 days of the 104th Congress, which afforded scant time for groups to lobby. Finally, any examination of lobbying strategies must control for the efforts of organizational opponents in order to distinguish between counteractive lobbying of allies and noncounteractive efforts (Austen-Smith and Wright 1994, 1996).

ISSUES, GROUPS, AND DATA

To examine organized interests' decisions about whom to lobby, we focus on four issues considered during the 104th Congress: product liability, financial services deregulation, crime control and criminal justice reform, and grazing use and management. Various proposals were introduced to cap punitive or noneconomic damage awards in product liability lawsuits. Financial services deregulation included measures to allow cross-market access for banks and securities firms (i.e., overhaul of the Glass-Steagall Act); modification or repeal of certain bank regulations and consumer laws; and permission for banks to sell insurance. Various bills were introduced to repeal the ban on certain assault-style firearms and to amend sections of the 1994 anticrime law: allow the "good faith" exception to the exclusionary rule; combine grant programs for police hiring and crime prevention into a single block-grant program; and limit federal appeals by state prisoners, including death row inmates. Finally, a set of proposals sought to overhaul regulations instituted by Interior Secretary Bruce Babbitt in 1995 that affected grazing fees and grazing practices on public lands.

We selected these issues because they vary in terms of public visibility and scope of conflict. Crime and, to a lesser extent, tort reform attracted considerable attention from the media and a broad array of organizations representing diverse interests. The grazing and financial services measures received minimal media attention and affect the interests of a more narrow array of groups.

We compiled a list of 648 groups to whom these four issues conceivably could be of interest by drawing on subject categories in *Washington Representatives* (1995) and *Congressional Quarterly's Washington Information Directory* (1996–97), issue-related stories in the *Congressional Quarterly Weekly Report* and *National Journal*, and congressional committee reports of organizations testifying at issue-related hearings (Appendix A provides details about our sampling method and mail survey). We selected 241 groups for the product liability issue, 119 for financial services deregulation, 160 for the anticrime proposals, and 128 for grazing use and management.

Mail questionnaires were sent to the 648 organizations in May 1996. Follow-up questionnaires were mailed a month later to those not responding. Overall, 211 organizations provided at least some information, yielding a response rate of 33%. For the present analysis our sample consists of the 69 groups who indicated they were taking action on one of the four issues. Even though our study examines only a subset of respondents, there is evidence that these 69 organized interests are representative of the overall universe of

TABLE 1. Variables Affecting Organizations' Lobbying Decisions in Committee

Variable	Mean	Value Range Min	Value Range Max	Std. Deviation	Valid Observations[a]	Expected Effect on Decision to Lobby
Decision to Lobby	.47	0	1	.50	2,354	
Legislator's Prior Position						
Uncertain	.27	0	1	.44	3,011	−
Opponent	.30	0	1	.46	3,011	−
Legislator Characteristics						
Party leader	.11	0	1	.31	3,173	+
Committee leader	.13	0	1	.33	3,173	+
Subcommittee member	.43	0	1	.50	3,173	+
Group Resource Capabilities						
District support	.67	0	2	.78	2,214	+
PAC	.37	0	1	.48	2,991	+
Lobbying staff	3.94	0	27	4.28	3,048	+
Coalition membership	.76	0	1	.43	3,133	+
Issue Context						
Importance	.80	0	1	.40	3,173	+
Organizational opposition	2.21	−.46	11	1.59	2,876	+
Financial services	.25	0	1	.43	3,173	+
Anticrime	.28	0	1	.45	3,173	ns[b]
Grazing	.15	0	1	.35	3,173	+

[a]Observations are group-legislator dyads. The number of observations equals the number of groups active on an issue multiplied by the number of legislators on the relevant House committee and the leaders of both parties in the House summed across the four issues.
[b]We anticipate no significant difference in the incidence of lobbying on the anticrime and product liability issues.

groups that were active on these issues (see Appendix A).

ANALYTIC APPROACH

Because each active organization can potentially lobby every relevant committee member, we examine lobbying at the committee stage using the group-legislator dyad as the unit of analysis.[9] This approach is consistent with our belief that the decision to lobby a legislator may be affected by variables at three levels of analysis: the dyad, the organization, and the legislator. Studies that focus on the group ignore legislator characteristics relevant to the decision, and studies using the legislator as the unit of analysis ignore individual group qualities as predictors of lobbying activity. The use of either groups or legislators as the unit of analysis also renders impossible any assessment of how their characteristics interact to affect lobbying choices.

For each group-legislator pair, we observe whether the group directly lobbies that legislator. Thus, the dependent variable is whether interest group i lobbies legislator j on issue k, as indicated by responses to our survey. The number of observations equals the number of groups active on an issue multiplied by the number of legislators on the relevant House committee and the leaders of both parties in the House summed across the four issues. Given that our sample includes 69 active interest groups and each issue features roughly 46 relevant House committee members and leaders, the total number of dyads in our analysis is 3,173.[10]

Variables

For our statistical analysis, *Decision to Lobby* takes on two values: 1 = the group lobbied the legislator, 0 = the group did not lobby the legislator (see Appendix B for questionnaire items and other details of variable measurement). Because the dependent variable is dichotomous, we use probit analysis to estimate our model of groups' lobbying choices. Table 1 provides summary statistics for each variable and includes the expected effect of each on the lobbying decision.

A legislator's prior position on the issue relative to the interest group is measured using a set of dichotomous variables. The first, *Uncertain*, is coded 1 if the legislator's prior position on the issue is unclear. The second, *Opponent*, is coded 1 if the legislator's prior issue position is at odds with the interest group's position on the legislative proposal. The omitted cate-

[9] Diana Evans (1996) and John Kingdon (1989) use similar research designs to examine lobbying success and legislators' voting decisions, respectively.

[10] The relevant committees are those that wrote the original legislation: (1) product liability, House Judiciary Committee and members of the House Commerce Committee who served on the Conference Committee (Bliley R-VA, Oxley R-OH, Cox R-CA, Dingell D-MI, and Wyden D-OR); (2) financial services, House Banking Committee; (3) crime, House Judiciary Committee; and (4) grazing use and management, House Resources Committee. Groups were asked whether they lobbied each member of the relevant committee and whether they lobbied any member of the party leadership in the House. So as not to add to the length of the survey, groups interested in product liability were asked about only the Commerce Committee members who sat on the Conference Committee.

gory represents the situation in which the legislator's prior position is consistent with the group's position on the issue. We expect that undecided members will be targeted less often than allies but more frequently than opponents. Other relevant characteristics of legislators are measured with dummy variables for *Committee Leader* (i.e., committee and subcommittee chairs and ranking members), *Subcommittee Member*, and *Party Leader*.

A series of variables measuring different dimensions of groups' resource capabilities also are included in our model. Our measure of *District Support* is coded 2 if the organization's base of support in the legislator's district is strong, 1 if it is moderate, and 0 if it is weak or nonexistent. *PAC* distinguishes between organizations with and without a PAC affiliation using a single dummy variable. *Lobbying Staff* is the number of full-time personnel assigned to lobbying. *Coalition Membership* equals 1 if the organization reported that it joined a coalition to take action on the issue, 0 otherwise.[11]

Three variables are included to test our expectations about how the context of each issue may influence the ability or inclination of interest groups to lobby at the committee level. First, *Importance* is equal to 1 if the issue is more important than other issues on a group's agenda, about as important as other issues, or the only one of interest to the group; a value of 0 indicates less importance than other issues. Second, we include a proxy measure for the presence and numerical strength of a group's *Organized Opposition* (see Appendix B). This allows us to test whether groups lobby counteractively in committee, as Austen-Smith and Wright (1994, 1996) contend about lobbying at the floor stage. Third, three dichotomous variables distinguish the *Banking*, *Crime*, and *Grazing* dyads from the legislator-group pairs relevant to product liability (our excluded category). We expect a higher incidence of committee lobbying on the banking and grazing issues. The House passed crime and product liability legislation so quickly (within the first 100 days of the 104th Congress) that there was little time for groups to lobby committee members.

A set of multiplicative terms are used to assess whether groups' tendencies to lobby legislators with different prior positions vary according to resource capabilities and number of organizational opponents. Each of the two prior position variables will be multiplied by the measure reflecting an organization's district strength (district support × uncertain, district support × opponent) and the number of lobbying staff in Washington (staff × uncertain, staff × opponent). We expect these interactive terms to have positive coefficients in the probit analysis, indicating that abundant lobbying resources enable organizations to move beyond their core supporters in House committees.

The variable tapping the number of opposition groups lobbying a legislator also will be multiplied by the two prior position variables (org. opposition × uncertain, org. opposition × opponent). We include these interactive terms to control for the possibility that a group's probability of lobbying a friendly legislator will increase when organizational opponents lobby that legislator. If groups are more likely to lobby their allies in response to the efforts of opposing groups, then the coefficient for org. opposition × opponent should be negative and roughly equivalent in size to the coefficient associated with the main effect, which should be positive and significant. Recall that the coefficient for the main effect reflects the influence of organizational opponents who are lobbying a given legislator on a group's probability of lobbying regardless of a legislator's position, and that our base (excluded) position measure indicates a legislative ally. Expectations are less precise for undecided legislators. The coefficient for org. opposition × uncertain may be either zero, positive, or negative, but if negative, it will not be as large as the coefficient measuring the main effect of organized opposition. In other words, we are not sure whether groups would be equally, more, or less likely to lobby undecided legislators relative to their allies in response to organized opposition.

Model Diagnostics

The use of a standard probit analysis presumes that our observations are independent. Given the nature of our data, this assumption is questionable. Because each element of a dyad appears in numerous other dyads, it is quite possible that all dyads sharing a particular legislator or group are related. For this reason, we base our inferences on robust standard errors that are calculated by applying Huber's (1967) formula to each cluster of observations identified with a single organization (see Beck 1996 and Beck and Tucker 1996 on the use of robust standard errors).[12] Appendix C presents information about an alternative estimation strategy we employed to remedy the cross-unit correlation.

RESULTS

The results of a probit analysis of organized interests' decisions to lobby members of selected House committees appear in Table 2. The first column shows the maximum likelihood coefficient estimates for each of the exogenous variables associated with the decision to

[11] Each resource indicator taps a distinct aspect of a group's lobbying capacity. As an alternative to separate treatment, we attempted to combine the indicators into a single index that would reflect an organization's overall resource capacity. We also tried to create an index of a group's Washington-based resource capacity which differed from the overall index by excluding the measure of district ties. Each index was formed by summing the relevant indicators after each was rescaled to range between zero and one. The scale reliability coefficients we obtained were quite low (.09 and .21 for the overall and Washington-based indices, respectively), suggesting that our individual measures should not be combined.

[12] Specifically, in Stata 5.0 we use the *probit* command with the *cluster* option to estimate our model. The robust standard errors are substantially larger than the usual probit standard errors, indicating that interdependence is present. We also estimated the robust standard errors that apply to each cluster of observations identified with a single legislator. These results provide broader support for the hypotheses we test than do the results presented in Table 2.

TABLE 2. Probit Analysis of Organizations' Decisions to Lobby in Committee

Variable	MLE[a]	Standard Error[b]	*p*-value[c]
Constant	−2.570	.618	—
Legislator's Prior Position			
Uncertain	−0.693	.234	.00
Opponent	−1.134	.277	.00
Legislator Characteristics			
Party leader	−0.118	.154	.45
Committee leader	0.347	.129	.01
Subcommittee member	0.070	.075	.35
Group Resource Capabilities			
District support	0.993	.135	.00
PAC	0.343	.266	.20
Lobbying staff	−0.035	.021	.10
Coalition membership	0.957	.354	.01
Issue Context			
Importance	0.977	.370	.01
Organizational opposition	0.081	.098	.41
Financial services	0.330	.226	.15
Anticrime	−0.180	.406	.66
Grazing	0.846	.385	.03
Interactive Effects			
District support × uncertain	0.306	.150	.04
District support × opponent	0.273	.178	.13
Staff × uncertain	0.000	.022	.99
Staff × opponent	−0.039	.039	.33
Org. opposition × uncertain	0.048	.083	.56
Org. opposition × opponent	0.080	.081	.32
Number of observations	2,024	Log-likelihood	−858.73
Dhrymes pseudo *R*-square	.38	Correctly predicted (%)	80.24
Mean value decision to lobby	.44	Reduction of error[d]	55.57

[a]Figures are unstandardized probit coefficients. Decision to lobby is coded 1 if the committee member was lobbied by the group, 0 if the committee member was not lobbied.
[b]Robust standard errors are shown. These standard errors are calculated by applying Huber's (1967) formula to each cluster of observations that are identified with a single organization.
[c]Observed probability level based on a two-tailed test.
[d]The reduction of error (ROE) statistic indicates the extent to which the model improves on predictions that each observation will fall into the modal category of the dependent variable (Hagle and Mitchell 1992, 781, n. 13): %ROE = 100 × (% correctly classified − % in modal category)/(100% − % in modal category).

lobby; the robust standard errors and *p*-values are shown in the second and third columns, respectively. Overall, the fit of the model is quite good, and the results provide considerable support for our conceptual model of organizations' lobbying decisions in committee.[13]

Table 2 reveals that, compared to legislative allies, legislators with opposing and uncertain issue positions are less likely to be lobbied. As expected, the coefficient estimate for opposing legislators is larger in absolute value than the estimate for uncertain legislators. In substantive terms this means that organizations are likely to assign the highest priority to lobbying allies on committees, a lower priority to those who are undecided or whose positions are unclear, and lowest priority to those whose prior issue positions conflict with their own. We examine the effect of legislators' prior positions on the lobbying decision in more detail below.

One other legislator characteristic included in our model also plays a significant role in the lobbying decision. As shown in Table 2, committee leaders are more likely to be the target of direct lobbying efforts than are other committee members. This is not surprising, since leaders shape the content of legislation and often are instrumental in determining whether, and how swiftly, proposals move beyond committee.

Like committee leaders, subcommittee members also have more opportunities than other committee members to modify the contents of a bill under consideration. Yet, our findings provide no support for the

[13] We estimated other model specifications with measures that controlled for freshman status and for the size and type of an organization's membership. In addition, we tested whether groups' lobbying choices vary across different types of groups. The results shown in Table 2 remain essentially the same under these alternative specifications. We also tested whether separate indicators were needed to capture the effects of moderate and strong district ties on groups' lobbying choices. The use of two measures of district ties and four interaction terms reduced the efficiency of our estimates and complicated the interpretation of the effects of the position and district tie variables, but the conclusions we draw from the results shown in Table 2 were left unchanged. These results are available from the authors.

TABLE 1. (continued)

Variable	Parameter Estimate (Standard Error)	p value	Total Effect of 1 S.D. Change[a]	Direct Effect of 1 S.D. Change[a]
Race (African-American)	−0.229 (0.046)	0.0001	3.4	3.4
Sex (female)	−0.031 (0.027)	0.2588	1.7	0.0
Number of cases = 1,280, Adj. R^2 = .44.				
Equation 3: Congressional Thermometer				
Intercept	0.211 (0.099)	0.0322		
Political Trust	0.280 (0.072)	0.0001	9.8	6.7
Partisanship	−0.110 (0.032)	0.0006	1.5	1.9
Approval of Congress	0.270 (0.039)	0.0001	6.1	4.6
Political Engagement	0.164 (0.042)	0.0001	2.7	2.0
Mean Issue Position	−0.030 (0.070)	0.6672	0.4	0.0
Overall Economic Evaluation	−0.070 (0.073)	0.3384	1.1	0.0
Education	−0.106 (0.040)	0.0080	1.3	1.4
Income	−0.055 (0.041)	0.1820	0.0	0.0
Age	0.048 (0.046)	0.3038	0.1	0.0
Race (African-American)	0.060 (0.032)	0.0596	0.9	0.9
Sex (female)	0.060 (0.019)	0.0019	2.6	1.5
Number of cases = 1,280, Adj. R^2 = .16.				

Source: American National Election Study, 1988.
Note: Entries for all three equations are unstandardized regression coefficients. p values are for two-tailed tests.
[a] Effects are given as a percentage of the dependent variable's range.

port for parliaments in central and eastern Europe, so I use the same overall economic evaluation variable as in the previous two equations. In addition, I include respondents' assessments of how well Congress is doing its job. Particularly strong short-term evaluations should significantly affect respondents' evaluations of the institution in general (Hibbing and Theiss-Morse 1995).[8] Finally, I control for the same demographic factors as in the trust and presidential equations, several of which are correlated with support for another institution, the Supreme Court (Gibson and Caldeira 1992),[9] so they may also affect support for Congress.

Figure 3 depicts the simultaneous equation model. I do not expect the presidential and congressional thermometers to affect one another. Because specific support implies support for leaders themselves, whereas diffuse support refers to evaluations of what leaders represent, measures of specific and diffuse support should be independent in the short run (Easton 1975, 444–5). Empirically, Patterson and Caldeira (1990) find that presidential popularity has no effect on public confidence in congressional leaders. In addition, Gibson and Caldeira (1992) find no relationship between specific and diffuse support for the Supreme Court.

DATA

Although trust has tended to decrease since the mid-1960s, making time-series analysis attractive, a number of factors militate against a reliable analysis of change over time. For example, using NES data, the congressional thermometer and questions about perceptions of

[8] Some may suggest that the congressional thermometer and the congressional approval rating tap the same concept. While the two are correlated (r = .36), their relationship is far weaker than that between the Reagan feeling thermometer and the rating approval for Reagan (r = .82).

[9] Some might suggest I should include the media consumption variables in the presidential and congressional thermometer equations. The bivariate correlations, however, are all quite small, with the majority under .06.

government effectiveness are only available for presidential years. In addition, the congressional thermometer and items on traditional views date only from 1988. Moreover, the NES uses a varying number of issue items from year to year, which undermines the comparability of policy satisfaction and mean issue position scores. Therefore, I rely on two cross-sectional data sets, the 1988 and 1996 NES.

The 1988 NES is particularly well suited for studying political trust. The late 1980s were "a relatively quiet period in American social, political, and economic life, in many ways parallel to what has been characterized as the quiescent fifties" (Neuman, Just, and Crigler 1992, 40). By 1988, the Iran-Contra affair had faded, so this scandal was not a central issue during that year's interviews. The 1988 NES allows for more confident generalizations than surveys during periods of political upheaval. It also offers a particularly useful combination of variables. It asks respondents to place themselves and the federal government on six separate issues, compared to just three in 1992, and so provides a more reliable estimate of respondents' policy satisfaction.[10]

Although the presence of a strong third-party alternative may alter the relationship between trust and other variables (A. Miller and Listhaug 1990), the fact that trust increased between 1994 and 1996 requires examination. Unfortunately, the NES stopped asking respondents to place the federal government on the seven-point issue scales after 1992 and asked only one seven-point foreign policy item after 1988. Rather than the absolute distance between respondents' placement of self and the federal government, I take absolute distance between their placement of self and each of the parties. Such a formulation is appealing, given that control of the federal government was divided during this period. All other variables in the model are available in 1996.

RESULTS

Appendix B presents descriptive statistics for each of the variables in the multivariate analyses. I convert all three endogenous variables to a scale of −1 to 1 in order to facilitate comparison of their relative effects. I convert each of the explanatory variables to a scale of 0 to 1 for the same purpose.[11]

Effects of the Endogenous Variables

Table 1 presents the two-stage least-squares estimates from the hypothesized simultaneous equation model using the 1988 data. I will discuss the 1996 data separately. To provide a sense of relative effect sizes, I present in Table 1 the direct and total effects of a change of one standard deviation in the explanatory variables. Direct effects are simply reflected in the parameter estimate, while the total effects of the endogenous variables are calculated by $\boldsymbol{\beta}^{-1}-\mathbf{I}$, where $\boldsymbol{\beta}^{-1}$ is the inverse of the matrix of endogenous direct effects, and $\mathbf{I}$ is the identity matrix. Since the endogenous variables are all two-point scales, the effect that one endogenous variable has as a percentage of another is the effect divided by two.

First, note that the endogenous variables seem to affect one another as hypothesized. All have statistically and substantively significant effects in the appropriate equations. Second, note that trust's effect on the Reagan thermometer score is substantially larger than the reverse. In terms of direct effects, a change of one standard deviation in political trust causes a roughly 10.5% change in the Reagan score. Conversely, a similar change in the Reagan thermometer causes only a change of about 6% in trust. In moving from most to least trustful, the Reagan thermometer declines by nearly 45% of its range, while moving from warmest to coolest on the Reagan thermometer decreases trust by only 19% of its range.

In addition, the direct effect of trust on the Reagan thermometer relative to other variables in the model is quite large. Viewed in terms of a change of one standard deviation, trust's effect is far larger than any variable except partisanship. Indeed, the effect of the next most influential variable, mean issue position, is half the size of political trust. If I take into account the range of each of the explanatory variables, trust's effect is larger than that of all factors, including partisanship.

Focusing entirely on direct effects, however, understates the full effect of political trust. For instance, the total effect of a change of one standard deviation in trust is more than 15% of the Reagan thermometer's range, and its influence is larger than that of any variable, no matter the change criterion. Using 1964 as a baseline dramatically illustrates trust's importance. The difference between the median in 1988 and 1964 is .60 points.[12] If, in 1988, political trust were to increase by this amount, the Reagan thermometer would increase by .388 points, or more than 19% of its range. While the presidential thermometer should not be confused with presidential approval ratings, this decrease of roughly 19% in the thermometer score can be used to approximate how much approval declined as a result of decline in political trust. To do so, I estimate the following model, cast in logit form:

$$\Pr(\mathrm{APP}_i) = (1 + exp[-(\beta_0 + \beta_1(\mathrm{PFT}_i) + \beta_2(\mathrm{PID}_i) + \beta_3(\mathrm{ECON}_i) + \beta_4(\mathrm{ISS}_i) + \beta_5(\mathrm{AA}_i)])^{-1} \quad (1)$$

where *exp* is the exponentiation to the base *e*, the subscript *i* refers to the *i*th individual, Pr(APP) is the probability that the respondent approves of the president,

[10] When I estimated the model with the 1988 data but used only the three issues available in the 1992 data set, policy satisfaction did not approach statistical significance.

[11] While space does not allow full presentation of the first-stage equations, a discussion of their goodness of fit is in order, since that affects the efficiency of the second-stage estimates. In 1988, the R^2 for the political trust, presidential thermometer, and congressional thermometer equations is .22, .47, and .16, respectively. In 1996, it is .14, .60, and .22. These fit measures for the first-stage equations are satisfactory for attitudinal data and compare favorably with other models that employ two-stage least-squares estimation for purposes similar to mine (see Huckfeldt et al. 1995; Kinder and Mendelberg 1995).

[12] Since the 1988 distribution is skewed, it is more appropriate to compare the medians. The precise median is +.054 in 1964, compared with −.546 in 1988.

PFT is the presidential feeling thermometer, PID is the respondent's location on the seven-point partisanship scale, ECON is the respondent's overall economic evaluation, ISS is the respondent's mean self-placement on the issues, and AA is a dummy variable indicating whether the respondent is African-American.

I use the parameter estimates generated from this model to simulate a predicted approval score based on actual responses to these questions. If the respondent's predicted approval score is greater than .50, then the respondent is scored as approving. If it is less than or equal to .50, the respondent is scored as disapproving. This simulation predicts an approval rating of 58.9%, only .9 percentage points greater than the observed approval rating in the sample.

Using the same logistic regression estimates, I calculate a second predicted score, again placing all variables at their observed scores except for the presidential thermometer. To each respondent's observed score, I add the .388 points that the thermometer would increase if trust were at the 1964 level. This simulation predicts an adjusted approval rating of 75.3%, which is 16.4 percentage points greater than the first predicted score. In other words, the model suggests that presidential approval would have been about 16 percentage points higher in the late 1980s had trust not declined since the mid-1960s, ceteris paribus. Interestingly, this difference in approval is only slightly larger than the gap in the outgoing approval ratings of Ronald Reagan and Dwight Eisenhower, who was another popular two-term Republican incumbent (Gallup 1972, 1651). In comparing approval ratings in the contemporary period with the beginning of the survey era, moreover, Brody (1991) observes a difference of approximately this amount. This suggests that political trust may be at least partially responsible for the lower presidential approval ratings in recent years compared to those in the late fifties and early sixties. Although largely ignored in the literature on presidential popularity, decreasing political trust can powerfully damage the president's standing.

Turning to the other endogenous variable, the congressional thermometer's direct and total effects on trust are larger than the reverse, but trust's effect on institutional assessments of Congress is substantial. For example, the total effect of a change of one standard deviation in the congressional thermometer on political trust is about 21% of trust's range. This is substantially larger than the nearly 10% change in the congressional thermometer caused by a corresponding change in trust. By far the most influential variable in the congressional equation is trust. The effect of a change of one standard deviation in the next most influential variable, approval of Congress, is 38% smaller than that of trust.[13]

While the implications of cooler feelings about the president are fairly straightforward, the implications of declining support for Congress as an institution are more speculative. When diffuse support wanes, the public may be more inclined to favor alternatives that bypass institutions, such as direct democracy. The Founding Fathers, however, created representative institutions because they feared this impulse toward direct citizen control. Furthermore, experiments in direct democracy have not proved altogether satisfying in terms of either policy outcomes or increasing political support (Citrin 1996). In addition, declining institutional support increases the possibility of fundamental change to the political system in the face of a serious crisis, even to an institutional structure as stable as that of the United States (Lipset and Schneider 1983). Any significant downward movement in the congressional thermometer must be viewed with concern.

As far as construct validity is concerned, the model suggests that trust is more a function of institutional than incumbent support. The effect of the congressional thermometer on trust is more than four times larger than that of the presidential thermometer, other things being equal. It is interesting to note, however, that trust seems to have a larger causal influence on support for incumbents than for institutions. While trust affects both substantially, its effect on the former is about one-half larger.

In sum, taking into account the simultaneity between political trust and other variables demonstrates trust's system-level relevance. Rather than just reflecting dissatisfaction with political leaders, political distrust is a powerful cause of this dissatisfaction. It affects both specific and diffuse support, as well as being affected by them.

Effects of Exogenous Variables on Political Trust

The total effects of the exogenous variables are calculated by $-\boldsymbol{\beta}^{-1}*\boldsymbol{\Gamma}$, where $-\boldsymbol{\beta}^{-1}$ is the negative of the inverse of the matrix of endogenous direct effects, and $\boldsymbol{\Gamma}$ is the matrix of exogenous direct effects.[14] An exogenous variable's effect across its range can be easily calculated by multiplying a variable's total effect by 1, since the range of all exogenous variables is one unit. Dividing this number by two yields the total effect of an exogenous variable across its range, as a percentage of the endogenous variable.

This model suggests that political trust does seem to have policy success and policy satisfaction components. Both overall economic evaluations and perceived gov-

[13] In terms of construct validity, the congressional thermometer seems, as hypothesized, to reflect more diffuse than specific support. In dramatic contrast to the Reagan thermometer, it depends only marginally on partisanship, insignificantly on mean issue position, and positively on political engagement.

[14] Based on a one-tailed test, if I am less than 95% confident that a variable's effect differs from 0, I treat the effect as 0 in calculating the total effects. In 1988, these variables are newspaper consumption, foreign policy satisfaction, income, age, and race in the trust equation; income and sex in the Reagan thermometer equation; and mean issue position, overall economic evaluation, income, and age in the congressional thermometer equation. In 1996, these variables are television news consumption, newspaper consumption, policy satisfaction with the Republicans, economic evaluations, and sex in the trust equation, and partisanship, political engagement, economic evaluations, income, and sex in the congressional thermometer equation.

ernment effectiveness significantly affect political trust. A change of one standard deviation in each of these variables can cause a change in trust of 4% and 4.4%, respectively. Across their ranges, they can increase trust by up to 24% and 14%. These results confirm previous findings about the economy's importance in understanding fluctuations in political trust (Citrin and Green 1986; A. Miller 1983). In fact, by allowing economic evaluations to enter the model through a direct effect on trust and indirectly through an effect on the other endogenous variables, the 1988 data suggest that the economy can be even more influential than previous research allows. Not surprisingly, the presidents who presided over the bulk of decline in political trust, Nixon, Carter, and Bush, all faced struggling economies at some point during their presidency.

The respondent's perceived distance from the federal government on domestic policy issues is statistically significant but not on foreign policy issues. This suggests that leaders may be somewhat more effective in raising trust levels by pursuing a domestic policy agenda, provided people are satisfied with its direction. In light of this finding, it is also noteworthy that Nixon, Carter, and Bush were relatively more successful with foreign than domestic policy.

The finding for the traditional world view variable, that is, statistical and substantive significance, represents an advance for research on political trust. Its parameter estimate is larger than all exogenous variables except economic evaluations. This indicates that public dissatisfaction with changes in general societal norms, and implicitly in government's role in promoting or condoning them, affects people's view of government in general. Since "family values" were not part of the political dialogue in the 1960s, a period many consider as a watershed for changing morals, concern about these changes may explain the decrease in political trust in the mid-1960s.

The model also produces some interesting results relating to political communication. As expected, television news consumption lessens political trust. The total effect of maximum television news consumption is −136 points, which represents a drop of nearly 7% of trust's range. While the television news consumption coefficient is negatively signed, that for newspaper consumption is *positively* signed, although neither of these estimates reaches conventional levels of statistical significance. A. Miller, Goldenberg, and Erbring (1979) obtained the same opposing sign and speculated that it was due to demographic differences between television and newspaper consumers. The present analysis, however, controls for education, age, race, income, and sex, which suggests that the nature of the media may explain the difference (see Hart 1994).

THE 1996 DATA

While the trend in political trust since the mid-1960s has been mostly decreasing, the trust index median increased by 3.6 percentage points between 1994 and 1996. More recent data suggest that the trend has continued (Pew Foundation 1998). By using 1996 data to replicate the simultaneous equation model, I can explore the causes and effects of this increase. The 1996 results appear in Table 2.

Again, the endogenous variables appear to affect one another as hypothesized. As was the case in 1988, the parameter estimate for trust on feelings about the president is more than two and one-quarter times larger than vice versa, although the magnitude of the effect in both directions is somewhat smaller in 1996. Trust's effect on the congressional thermometer in 1996 is slightly smaller than in 1988 as well, but the reciprocal link is slightly larger. Taken together, these results suggest that trust has become somewhat more closely linked to institutions than to incumbents.

By multiplying trust's total effect on feelings about Clinton by the amount that trust increased between 1994 and 1996, I can estimate its benefit to the president. I find that the increase of 3.6 percentage points in trust yields about a 1.6% increase in the Clinton thermometer. Given that Clinton's thermometer score increased by approximately 5% between 1994 and 1996 (Rosenstone, Miller and Kinder 1997), my model suggests that increasing trust accounts for about one-third of Clinton's jump, other things being equal. There is little doubt that President Clinton had more success in the second half of his first term, and perhaps increasing political trust played a role. By performing much the same calculation for the congressional equation, an increase of 3.6% in trust would increase the congressional thermometer by 1.2%.[15]

Turning to the exogenous variables, each has a direct effect on trust in the same direction in both years, with one exception. In 1988, being African-American carried a positive sign, although its effect was not statistically significant. In 1996, blacks were significantly less trustful. Although bivariate analysis shows that they were slightly more trustful than other races in 1996 (see Abramson, Aldrich, and Rhode 1998, 84), the reverse is true when proper controls are applied. A secondary analysis demonstrates that controlling for feelings about President Clinton, specifically, causes the change. This suggests that, once the expected tendency for blacks to trust a Democratic administration is taken into account, African-Americans are somewhat less trustful of the government in Washington. Perhaps they found the election and reelection of a Republican Congress in 1994 and 1996 particularly troubling.

It is also noteworthy that economic evaluations do not have a direct effect on trust in 1996. Although the economy's estimated effect on the presidential thermometer is 50% larger in 1996 than 1988, thus increasing its indirect effect on trust, its direct effect cannot be confidently discerned from 0. Hence, the economy's total effect in 1996 is less than one-fifth that of 1988.

What has caused the increase in trust? Given the set

[15] Because the 1994 NES did not include the congressional thermometer, I cannot determine the actual percentage of improvement.

TABLE 2. Structural Parameter Estimates of Simultaneous Equation System Involving Political Trust (2SLS), 1996

Variable	Parameter Estimate (Standard Error)	*p* value	Total Effect of 1 S.D. Change[a]	Direct Effect of 1 S.D. Change[a]
Equation 1: Political Trust				
Intercept	−0.564 (0.089)	0.0001		
Presidential Thermometer	0.126 (0.072)	0.0795	5.3	3.5
Congressional Thermometer	0.849 (0.115)	0.0001	21.9	14.5
TV News Consumption	−0.025 (0.044)	0.5589	0.0	0.0
Newspaper Consumption	0.011 (0.043)	0.7935	0.0	0.0
Policy (Dis)Satisfaction with Democrats	−0.271 (.035)	0.0215	3.1	2.3
Policy (Dis)Satisfaction with Republicans	−0.035 (0.094)	0.7091	0.0	0.0
Perceived Government Effectiveness	0.140 (0.057)	0.0145	2.3	1.7
Traditional World View	−0.236 (0.074)	0.0013	3.2	2.4
Overall Economic Evaluation	0.108 (0.111)	0.3277	0.7	0.0
Education	0.172 (0.056)	0.0023	0.9	2.3
Income	0.125 (0.051)	0.0145	2.0	1.8
Age	−0.140 (0.070)	0.0448	1.2	1.6
Race (African-American)	−0.156 (0.052)	0.0029	0.3	2.3
Sex (female)	−0.033 (0.026)	0.2046	0.2	0.0
Number of cases = 1,249, Adj. R^2 = .13.				
Equation 2: Presidential Thermometer				
Intercept	0.796 (0.092)	0.0001		
Political Trust	0.328 (0.073)	0.0001	11.2	7.4
Partisanship	−0.697 (0.038)	0.0001	13.0	12.3
Mean Issue Position	−0.769 (0.078)	0.0001	5.8	6.2
Overall Economic Evaluation	0.574 (0.087)	0.0001	4.4	4.2
Political Engagement	−0.105 (0.050)	0.0340	1.3	1.2
Education	−0.143 (0.046)	0.0020	1.6	1.9
Income	−0.144 (0.042)	0.0007	1.4	2.1
Age	−0.171 (0.051)	0.0008	2.3	1.9

TABLE 2. (continued)

Variable	Parameter Estimate (Standard Error)	p value	Total Effect of 1 S.D. Change[a]	Direct Effect of 1 S.D. Change[a]
Race (African-American)	−0.203 (0.036)	0.0001	3.1	3.0
Sex (female)	−0.055 (0.022)	0.0117	1.4	1.4
Number of cases = 1,249, Adj. R^2 = .57.				
Equation 3: Congressional Thermometer				
Intercept	−0.111 (0.092)	0.2264		
Political Trust	0.244 (0.085)	0.0043	8.3	5.5
Partisanship	0.028 (0.032)	0.3834	0.5	0.0
Approval of Congress	0.255 (0.035)	0.0001	5.9	4.6
Political Engagement	0.030 (0.040)	0.4502	0.0	0.0
Mean Issue Position	0.232 (0.066)	0.0005	2.1	1.9
Overall Economic Evaluation	0.107 (0.075)	0.1514	0.2	0.0
Education	−0.125 (0.037)	0.0007	1.5	1.7
Income	−0.053 (0.034)	0.1226	0.5	0.0
Age	0.232 (0.042)	0.0001	2.9	2.6
Race (African-American)	0.170 (0.029)	0.0001	2.6	2.6
Sex (female)	0.025 (0.017)	0.1518	0.0	0.0
Number of cases = 1,249, Adj. R^2 = .22				

Source: American National Election Study, 1996.
Note: Entries for all three equations are unstandardized regression coefficients. *p* values are for two-tailed tests.
[a] Effects are given as a percentage of the dependent variable's range.

of reciprocal relationships, all three endogenous variables (trust, feelings about the president, and feelings about Congress) will tend to remain at relatively low levels, barring significant changes in the exogenous variables. That is, the endogenous variables are unlikely to change without reason. For an exogenous variable to be a cause of increased trust, it must meet two conditions. First, it must have a substantial direct or indirect effect on trust in 1996. This eliminates the media consumption variables and policy dissatisfaction with the Republicans, which are not statistically significant, and political engagement, which does not have a substantively large effect. Second, the variables that affect trust must have changed substantially between 1994 and 1996, which eliminates the glacially changing social characteristics. Table 3 presents the differences in means of the substantive exogenous variables that either directly or indirectly affect trust.

To calculate a variable's contribution to increasing trust, I multiply the amount that it changed during the period by its total effect on trust. While previous scholarship indicates that increased trust may be the result of an improved economy (A. Miller 1983; Citrin and Green 1986), these results suggest otherwise. The 1.5 percentage point change in overall economic evaluations is not statistically significant and fails to cause a substantive change in trust. In addition, the differences in means for traditional world view, policy satisfaction, mean issue position, and partisanship are trivial.

People's perceptions of government effectiveness and their evaluations of congressional job performance improved markedly, however. Although the former item was not asked in 1994, the mean score in 1992 was .206, compared with .394 in 1996. By multiplying this difference by the variable's total effect on trust in 1996, I find that improved perceptions of government effectiveness increased political trust by 1.7%.

TABLE 3. Differences in Means for Exogenous Variables, 1994 and 1996

Issue	1994 Mean (s.d.) *n*	1996 Mean (s.d.) *n*	Difference in Means (*t*-statistic)	Resultant Percentage Change in Trust[a]
Perceived Government Effectiveness[b]	0.206 (0.267) 2223	0.394 (0.245) 1490	+0.188*** (10.92)	+1.7
Approval of Congress	0.346 (0.342) 1795	0.459 (0.358) 1714	+0.113*** (4.25)	+1.6
Overall Economic Evaluation	0.545 (0.163) 1781	0.560 (0.148) 1714	+0.015 (1.70)	—
Mean Issue Position	0.486 (0.166) 1382	0.495 (0.163) 1213	+0.009 (1.03)	—
Partisanship	0.486 (0.351) 1772	0.446 (0.350) 1695	−0.040 (−1.23)	—
Traditional World View	0.630 (0.215) 1765	0.622 (0.202) 1529	−0.008 (0.50)	—
Policy (Dis)Satisfaction with Democrats[c]	0.306 (0.262) 1408	0.323 (0.281) 1427	+0.017 (0.93)	—

Source: American National Election Study, 1992, 1994, 1996.
***$p < .001$, two-tailed tests.
[a]Calculated as (Difference in Means*Total Effect on Trust)/2.
[b]Variable not available in 1994. Statistics presented are from 1992 NES.
[c]Difference in means reflects only common elements asked in 1994 and 1996.

Improved congressional job approval also seems to have contributed indirectly to an increase in trust through its effect on feelings about Congress as an institution. The contribution can be calculated by multiplying the difference in means by the total effect of approval on trust. I find that the .113-point increase in the mean job approval rating of Congress accounts for an increase of 1.6% in trust, ceteris paribus.

These data also allow me to replicate the presidential approval simulation, this time comparing Bill Clinton and Lyndon Johnson. The results from equation 1 for the 1996 data, which I use to generate the predicted and simulated approval ratings, appear in the right-hand column of Appendix C. Between 1964 and 1996, the trust median dropped from +.054 to −.623, or by .677 points. To simulate 1996 feelings about Clinton given 1964 trust, I multiply .677 by the total effect of a one-unit change in trust on the Clinton thermometer. This calculation suggests that, on average, people's feelings about the president would be .295 points higher with trust at 1964 levels. I add this to each person's observed feeling thermometer score. If this adjustment produces a score greater than 1, I assign a score of 1.

In 1996, 67.5% of the sample approved of Bill Clinton. My predicted score, with trust and the other variables at their observed levels, is a somewhat higher 70.4%. When respondents' observed presidential thermometer scores are increased by .295 points, the adjusted approval rating is 78.3%. This is substantially higher than the 71% recorded for Johnson in late 1964, but it is noteworthy that Clinton's NES approval rating is almost 10 points higher than his Gallup rating during the same period. In fact, the gap of 8 percentage points between the predicted and adjusted scores is only one point smaller than the difference in the Gallup second-term preinaugural job approval ratings for Johnson and Clinton (Gallup Organization 1998, 15). Consistent with the 1988 data, these results further suggest that trust may be at least partially responsible for the lower contemporary presidential approval ratings.

CONCLUSION

These findings about political trust have important implications. First, higher levels of trust are of great benefit to both elected officials and political institutions. More trust translates into warmer feelings for both, which in turn provides leaders more leeway to govern effectively and institutions a larger store of support regardless of the performance of those running the government.

This study also adds to our understanding of what can increase political trust. In addition to the economy, improved perceptions of government effectiveness and higher levels of congressional approval can effect sub-

stantial changes. With regard to the former, perhaps Clinton's efforts in the second half of his first administration are instructive. While the public was not willing to embrace sweeping new policies such as health care reform, it seemed satisfied by the "bite-sized" proposals in areas of broad interest. Initiatives such as a minimum wage increase, a large-scale literacy program, strong stands on the environment, and family and medical leave can make a difference.

With regard to congressional approval, the election of the first Republican Congress in 40 years likely provided the public with an increased sense that the political system can respond to citizen discontent. Once in office, the minor but well-publicized institutional changes adopted by Congress in early 1995, such as cutting the number of committees and their staffs and requiring that laws apply equally to members of Congress and ordinary Americans, may have contributed to the improvement in approval as well. That these changes were institutional in nature is noteworthy, given the continued unpopularity of congressional leaders such as Newt Gingrich and certain congressional decisions, such as the government shutdown (see Hibbing and Theiss-Morse 1995).

In sum, these results suggest that political leaders can take steps to increase trust. But will the increases be as fleeting as those of the Reagan years? The country continues to face fundamental problems on issues such as race, Social Security, Medicare, and health care that will likely require large-scale solutions. As the health care reform fiasco of 1993–94 suggests, however, a public no longer possessed of a core trust in its political system is easily frightened by negative campaigns against broad new initiatives. On the supply side, leaders concerned about public reprisals will be less likely to support such initiatives. Without public support for solutions, problems will linger, will become more acute, and if not resolved will provide the foundation for renewed discontent.

APPENDIX A: NES QUESTION WORDING

Political Trust

People have different ideas about the government in Washington. These ideas don't refer to Democrats or Republicans in particular, but just to the government in general. We want to see how you feel about these ideas. For example:

1. How much of the time do you think you can trust the government in Washington to do what is right—just about always, most of the time, or only some of the time? (TRUST)
   1. Just about always
   0. Most of the time
   −1. Some of the time
2. Do you think that people in government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it? (WASTE)
   1. Not very much
   0. Some
   −1. A lot
3. Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people? (INTEREST)
   1. For the benefit of all
   −1. Few big interests
4. Do you think that quite a few of the people running the government are crooked, not very many are, or do you think hardly any of them are crooked? (CROOKED)
   1. Hardly any
   0. Not many
   −1. Quite a few

Missing Data: To conserve cases, I require that respondents answer only one question. In 1988, 9.1% skipped one of the four questions, 1.7% skipped two, and .6% skipped three. Only .6% were excluded from the analysis. In 1996, the corresponding percentages were 4.2, .5, .1, and .3. If I require that respondents answer all four questions, the results are consistent.

Mean Issue Position (1988 Data)

1. Some people think the government should provide fewer services, even in areas such as health and education in order to reduce spending (7). Other people feel it is important for the government to provide many more services even if it means an increase in spending (1). Where would you place yourself on this scale, or haven't you thought much about this?
2. Some people feel the government in Washington should see to it that every person has a job and a good standard of living (1). Others think the government should just let each person get ahead on their own (7).
3. Some people feel that the government in Washington should make every effort to improve the social and economic position of blacks (1). Others feel that the government should not make any special effort to help blacks because they should help themselves (7).
4. Some people feel that the government in Washington should make every effort to improve the social and economic position of blacks and other minorities (1). Others feel that the government should not make any special effort to help minorities because they should help themselves (7).
5. Some people feel it is important for us to try to cooperate more with Russia (1), while others believe we should be much tougher in our dealings with Russia (7).
6. Some people believe that we should spend much less money for defense (1). Others feel that defense spending should be greatly increased (7).
7. There is much concern about the rapid rise in medical and hospital costs. Some people feel there should be a government insurance plan which would cover all medical and hospital expenses for everyone (1). Others feel that all medical expenses should be paid by individuals and through private insurance plans like Blue Cross or some other company-paid plans (7).
8. Recently there was been a lot of talk about women's rights. Some people feel that women should have an equal role with men in running business, industry, and government (1). Others feel that a woman's place is in the home (7).

Policy Satisfaction (1988 Data)

Of the eight issue position items, respondents are asked to place themselves and the federal government on six, which I list below. I take the absolute value of the distance between the self-placement and the placement of the federal government.

Domestic

1. Government services
2. Government job
3. Aid to blacks
4. Aid to minorities

Foreign

1. Cooperation with Russia
2. Defense spending

Missing Data: To conserve cases, I require that respondents provide only one valid response for issue position and policy satisfaction indexes. It also bears noting that the aid to minorities and aid to blacks questions were asked to separate half-samples in the 1988 NES. For mean issue position, 19.8% of the sample skipped one item, 10% skipped two, 5.3% skipped three, 3.4% skipped four, 2% skipped five, 1.4% skipped six, and 1.2% failed to answer any. For domestic policy satisfaction, 18.9% of the sample missed one question, 10.5% missed two, and 11.5% failed to answer any. For foreign policy satisfaction, 15.2% missed one, and 14.8% answered neither.

Mean Issue Position (1996 Data)

1. Some people think the government should provide fewer services even in areas such as health and education in order to reduce spending (7). Other people feel it is important for the government to provide many more services even if it means an increase in spending (1).
2. Some people believe that we should spend much less money for defense (1). Others feel that defense spending should be greatly increased (7).
3. There is much concern about the rapid rise in medical and hospital costs. Some people feel there should be a government insurance plan which would cover all medical and hospital expenses for everyone (1). Others feel that all medical expenses should be paid by individuals and through private insurance plans like Blue Cross or some other company-paid plans (7).
4. Some people feel the government in Washington should see to it that every person has a job and a good standard of living (1). Others think the government should just let each person get ahead on their own (7).
5. Some people feel that the government in Washington should make every effort to improve the social and economic position of blacks (1). Others feel that the government should not make any special effort to help blacks because they should help themselves (7).
6. Some people say that the best way to reduce crime is to address the social problems that cause crime, like bad schools, poverty, and joblessness (1). Other people say the best way to reduce crime is to make sure that criminals are caught, convicted, and punished (7).
7. Some people think it is important to protect the environment even if it costs some jobs or otherwise reduces our standard of living (1). Other people think that protecting the environment is not as important as maintaining jobs and our standard of living (7).
8. Some people think we need much tougher government regulations on business in order to protect the environment (1). Others think that current regulation to protect the environment are already too much of a burden on business (7).
9. Recently there was been a lot of talk about women's rights. Some people feel that women should have an equal role with men in running business, industry, and government (1). Others feel that a woman's place is in the home (7).

Policy Satisfaction (1996 Data)

Of the nine items above, respondents are asked to place themselves and both major parties on the four below. I take the mean absolute difference between the placements of self and the respective parties.

1. Government services
2. Defense spending
3. Environment and jobs
4. Environment regulation

Missing Data: To conserve cases, I require that respondents provide only one valid response for issue position and policy satisfaction indexes. For mean issue position, 16.7% of the sample skipped one item, 9.2% skipped two, 5.2% skipped three, 3.2% skipped four, 1.5% skipped five, 1.2% skipped six, 1.1% skipped seven, .4% skipped eight, and .8% failed to answer any. For satisfaction with the Democrats, 16.6% of the sample missed one question, 8.7% missed two, 6.5% missed three, and 6.8% failed to answer any. For satisfaction with the Republicans, 16.4% missed one, 9.6% missed two, 6.3% missed three, and 6.8% failed to answer any.

Traditional World View

Responses to these items range from agree strongly to disagree strongly. I code each item to run from most tolerant to most traditional.

1. The newer lifestyles are contributing to the breakdown of our society.
2. The world is always changing and we should adjust our view of moral behavior to those changes.
3. This country would have many fewer problems if there were more emphasis on traditional family ties.
4. We should be more tolerant of people who choose to live according to their own moral standards, even if they are very different from our own.

Missing Data: To conserve cases, I require that respondents provide only one valid response. In 1988, 1.9% skipped one of the four questions, .3% skipped two, and .5% skipped three. Only .6% were excluded from the analysis. In 1996, the corresponding percentages were .8, .2, .1, and .3. If I require that respondents answer all four questions, the results are consistent.

Perception of Government Effectiveness

The item used to tap this concept is the second part of a two-part question. First, respondents are asked what is the single most important problem facing the government. Second, they are asked "how good a job is the government in Washington doing in dealing with this problem: a good job, only fair, or a poor job."

Missing Data: To conserve cases, I place those who do not identify a most important problem at the scale's midpoint. In 1988, 1.4% failed to identify a most important problem. Since the most important problem questions were asked to only a half sample in 1996, 51.2% were placed at the midpoint.

Congressional Approval

I use the four-point scale that asks respondents how strongly they approve or disapprove.

Missing Data: To conserve cases, I place those who do not provide a response to the approval question at the scale's midpoint. In 1996, 8.8% of cases fall into this category; in 1988, 6.27%.

APPENDIX B. Summary Statistics for Variables Included in Structural Equation Models, 1988 and 1996

Variable	1988 Mean (S.D.)	1996 Mean (S.D.)
Political Trust	−0.453 (0.476)	−0.512 (0.451)
Presidential Thermometer	0.238 (0.615)	0.194 (0.561)
Congressional Thermometer	0.198 (0.356)	0.118 (0.342)
Television News Consumption	0.638 (0.302)	0.503 (0.325)
Newspaper Consumption	0.458 (0.328)	0.384 (0.326)
Partisanship	0.491 (0.353)	0.453 (0.354)
Approval of Congress	0.517 (0.338)	0.455 (0.362)
Mean Issue Position	0.479 (0.139)	0.481 (0.161)
Political Engagement	0.561 (0.255)	0.615 (0.231)
Domestic Policy (Dis)Satisfaction	0.325 (0.203)	—
Foreign Policy (Dis)Satisfaction	0.282 (0.215)	—
Policy (Dis)Satisfaction with Democrats	—	0.255 (0.171)
Policy (Dis)Satisfaction with Republicans	—	0.280 (0.189)
Perceived Government Effectiveness	0.255 (0.314)	0.392 (0.245)
Traditional World View	0.611 (0.203)	0.624 (0.204)
Overall Economic Evaluation	0.535 (0.167)	0.565 (0.146)
Education	0.490 (0.270)	0.543 (0.269)
Income	0.277 (0.252)	0.320 (0.288)
Age	0.354 (0.216)	0.387 (0.224)
Race (African-American)	0.101 (0.301)	0.100 (0.300)
Sex (female)	0.518 (0.500)	0.542 (0.498)
Number of cases	1,280	1,249

Source: American National Election Study, 1988, 1996.

APPENDIX C. Logistic Regression Estimates Predicting Presidential Approval

Variable	1988 Parameter Estimate (Standard Error)	1996 Parameter Estimate (Standard Error)
Intercept	−1.595*** (0.339)	0.268 (0.468)
Presidential Thermometer	3.882*** (0.207)	3.922*** (0.273)
Partisanship	1.193*** (0.234)	−1.574*** (0.319)
Economic Evaluation	1.451** (0.116)	2.956*** (0.619)
Mean Issue Position	−0.144 (0.484)	−1.192 (0.658)
Race (African-American)	−0.826*** (0.234)	0.539 (0.338)
Number of cases	1,942	1,502
Beginning −2 X log likelihood	2644.40	1892.58
Ending −2 X log likelihood	1297.13	871.21
Pseudo-R^2	.50	.49

Source: American National Election Study, 1988, 1996.
Note: Entries are unstandardized maximum likelihood estimates. $^{**}p < .01$, $^{***}p < .001$, two-tailed tests.

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An Information Rationale for the Power of Special Interests

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Political decisions are often biased in favor of special interests at the expense of the general public, and they are frequently inefficient in the sense that the losses incurred by the majority exceed the gains enjoyed by the minority. This article explains the bias in terms of information asymmetries and the free-rider problem. First, incumbents increase their reelection prospects by biasing policy toward groups that are better able to monitor their activities. Second, because smaller groups are better able to overcome the free-rider problem of costly monitoring, policy will be biased in their favor. Third, the effect of asymmetric monitoring on voter welfare is ambiguous. The inefficiencies created by the policy bias *are offset by a positively valued* selection bias*: Incumbents of above-average quality are more likely to survive voter scrutiny than are low-quality types.*

In many areas of public policy, government favors special interests at the expense of the general public. That favor may take the form of subsidies, tax breaks, or market intervention through regulatory or trade policies. The "rural bias" in industrialized democracies illustrates the inequities and inefficiencies that often result. At the expense of taxpayers and consumers, agricultural price supports improve farm income. In the United States, one-third of the earnings from farming are due to protective agricultural policies; the corresponding proportions are one-half and two-thirds for the European Union (EU) and Japan, respectively.

The implied levels of taxation for agricultural support are quite extraordinary. In 1990, the average cost per nonfarm household amounted to $1,400 a year, and this cost is expected to increase to $1,800 (in 1990 dollars) by 2000. According to some estimates, a phased reduction of 50% in agricultural protection would increase economic welfare in the United States, the EU, and Japan by $32 billion (in 1985 dollars); this number captures the increase in consumer surplus net of the loss in producer surplus. Agricultural protection persists in industrialized economies even though farm output contributes only 2–3% to GDP and the percentage of voters who are members of farm households has dwindled into single digits. (Statistics are drawn from Anderson and Tyers 1992 and *Economist* 1992.)

In many developing countries, policy exhibits an urban bias even though the agricultural sector contributes critically to the national economy and farmers are in the majority (Bates 1981; Lipton 1977). Governments intervene in markets to lower the relative price of farm products, in effect taxing the countryside and subsidizing urban consumers. The reverse power-in-numbers effect manifested in rural and urban biases is one of the most intriguing puzzles in the political economy of development.

This article is structured as follows. I first review the explanations for policy biases and inefficiencies provided by the literature and argue that these explanations are incomplete. I then present my informational theory as an improvement on existing theories. Next, I develop a formal model, deriving the policy and selection biases that follow from information asymmetries across groups of voters and endogenizing these information asymmetries as resulting from differences in group size. I then outline the argument informally. I subsequently compare the implications of my theory to existing empirical results and discuss how they might be tested empirically. Finally, I present some extensions of the model and discuss the prospects for political reform.

LITERATURE REVIEW

Economists, who denounce the social deadweight losses arising from inefficient redistribution, are quick to identify the potential for Pareto-improvements: Each member of society could be made better off if special interest handouts were eliminated and the people who had gained from them were to compensate the losers. Theoretically, transaction costs can be invoked to explain why Pareto-improvements fail to be realized: It may be prohibitively costly or infeasible for the winners to pay the losers (Coase 1960). To the extent that transaction costs are impossible to measure, this hypothesis is irrefutable and thus vacuous. As a practical matter, the potential gains from eliminating the policy bias toward special interests appear so high as to render the transaction cost hypothesis implausible as an explanation. Instead, social scientists identify various deficiencies in political processes or institutions as the causal factor driving inefficient redistribution.

The Free-Rider Problem of Collective Action

In *Federalist 10*, James Madison raised the specter of democracy turning into a tyranny of the majority, that is, a political majority acquiring benefits at the expense of a political minority. With this admonition in mind, it

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This research was financially supported by National Science Foundation Grant No. SBR-9308405. I am indebted to Douglas Arnold, Russell Burgos, Avinash Dixit, David Epstein, Jack Hirshleifer, Kenneth Kollman, Nolan McCarty, Richard McKelvey, Roger Myerson, Klaus Nehring, Mancur Olson, Steven Postrel, Peter Rosendorff, Howard Rosenthal, John Zaller, and William Zame for insightful comments.

is all the more puzzling that politicians cater to narrow special interests at the expense of the electorate at large.

Olson (1965, 127–8) identifies the free-rider problem of collective action as the source of the policy bias toward special interests:

> Since relatively small groups will frequently be able voluntarily to organize and act in support of their common interests, and since large groups normally will not be able to do so, the outcome of the political struggle among the various groups in society will not be symmetrical.... The small oligopolistic industry seeking a tariff or a tax loophole will sometimes attain its objective even if the vast majority of the population loses as a result. The smaller groups—the privileged and intermediate groups—can often defeat the large groups—the latent groups—which are normally supposed to prevail in a democracy. The privileged and intermediate groups often triumph over the numerically superior forces in the latent or large groups because the former are generally organized and active while the latter are normally unorganized and inactive.

In a democracy characterized by regular free elections, the free-rider problem of collective action is not sufficient to explain why elected politicians cater to special interests. We would expect rational voters to see through political attempts to curry favor with special interests at their expense. A disaffected majority aware of this policy bias could then punish its political representatives by voting them out of office. Therefore, some degree of voter ignorance appears to be a necessary assumption of theories explaining the policy bias. Such theories rely implicitly on voter illusion: Political incumbents can and do systematically fool a majority of voters by favoring special interests while keeping the costs imposed on the majority below some awareness threshold (Becker 1983, 391–4; Wilson 1980, 369).

Alternatively, under the assumption of voter rationality, the electorate can be thought of as ill informed (Downs 1957). If information gathering is costly, then a large portion of voters may not seek to learn about political alternatives that have the characteristic of being a collective good (possibly with differentiated benefits). The problem is exacerbated because the probability that a single vote—informed or not—makes a difference for the election outcome is usually very small.

The argument that the policy bias derives from rational voter ignorance has some surface plausibility but becomes unsustainable under further scrutiny. As a by-product of engaging in various social and economic activities in daily life, voters accumulate costless information about their personal wealth and well-being. Even if they are ill informed about policy details, they nonetheless know how well off they are, and they can base their vote on this knowledge. Indeed, one robust empirical regularity that holds across industrialized democracies is retrospective voting: Incumbents who preside over good economic performance tend to get reelected (Fiorina 1981). The literature on rational retrospective voting interprets such behavior as the result of rational inferences about the quality of political representatives, formed on the basis of observed economic performance (Rogoff 1990; Rogoff and Sibert 1988).

In the face of retrospective voting, an incumbent must trade off gains and losses in political support that follow from favoring one group of voters rather than another. It is simply not obvious that an incumbent can generate a net gain in political support by catering to a well-informed minority at the expense of an ill-informed majority, especially if the total losses incurred by the general public exceed the total gains enjoyed by special interests. Such a policy bias reduces the welfare of a majority of voters and decreases the likelihood that they will vote for the incumbent. If economists are correct in identifying the huge deadweight losses associated with political handouts to special interests (*Economist* 1992; Hufbauer and Elliott 1994), then an incumbent who eradicates such inefficiencies will preside over an increase in the standard of living that will ensure reelection. An incumbent who is incompletely informed about voter preferences will have incentives to set policy in an unbiased and efficient way in order to maximize the welfare of his constituents and thereby maximize his reelection chances. If the incumbent is too short-sighted to follow this prescription, then political competition will allow society to break free from inefficient redistributive policies: A political entrepreneur who campaigns for Pareto-improving political reforms will be elected unanimously (Wittman 1989).

Of course, voters do obtain information not only about their personal wealth and well-being but also about the state of the economy and the quality of life in their society. Public opinion polls transmit information held by a subset of voters to the electorate at large (Bartels 1988; McKelvey and Ordeshook 1985). Interest groups and political candidates gather costly information and provide low-cost information cues to the general public. For example, interest groups monitor legislators' voting records and publish ratings, and challengers attack the legislative records of incumbents they seek to unseat (Lupia 1992). Once again, it is not obvious that an incumbent can afford to "shirk" by catering to a well-informed minority at the expense of a majority of voters if the latter are ill informed most of the time but in an election year "vote as if they had full information" (McKelvey and Ordeshook 1985, 57).

Building on Arnold (1990), I seek to undermine this Panglossian perspective on electoral competition, information, and shirking. First, in a complex world there is virtually an infinite amount of information competing for voters' attention. In the presence of some (possibly very small) cognitive cost of information processing, voters do not necessarily process information even if, by some measure, it is publicly available at no cost. We can expect political competition to work well in preventing an incumbent from acting against the interests of constituents only if an issue is salient in the public mind—or if it can be made salient in a future election by an interest group or a challenger; otherwise, incumbents can and do shirk.

For example, when price inflation was low, Congress supported sugar quotas that implied artificially high

sugar prices, enriching U.S. producers and (in effect) imposing a hidden sales tax on consumers. Neither price inflation nor sugar quotas were salient issues in the mind of the American voter. In 1974 and later in that decade, galloping price inflation figured prominently on the electoral agenda; Congress then repeatedly rejected bills to renew the sugar programs of the past. Sugar quotas themselves did not become electorally salient, but legislators feared a future challenger would make a campaign issue out of a vote that could easily be connected with higher prices (Arnold 1990, 123–5).

Second, the causal chain linking actions taken by an incumbent and policy effects experienced by constituents can be very complex. Interest groups and challengers may not be able to communicate effectively that an incumbent has acted against the interests of constituents. Once again, we can be satisfied that political competition will work well if the incumbent shirks in a way that can be described in a ten-second television "bite." But in many cases voters lack specialized information to understand that the incumbent acted against their interests, and they cannot acquire this specialized information at low cost.

For example, it would be relatively easy for voters to understand how direct transfers improve the income of automobile workers, but some knowledge of politics and economics is necessary to understand how Congress and the president employ a "voluntary export restraint" to save jobs in the automobile industry at the expense of American consumers:

> In February 1981, House Budget Committee Chairman James Jones announced that Congress would soon be voting to impose auto import quotas on Japan. In April, Sen. Bob Dole, chairman of the Senate Finance Committee, warned visiting Japanese legislators that Japan must either reduce its exports or face the wrath of Capitol Hill. President Reagan, "seeking to uphold free trade principles," as the *New York Times* noted, indicated that his administration would not negotiate with Japan over any specific level of restraint. But Reagan also declared that he would welcome "voluntary" action by the Japanese government to reduce auto exports. Japan announced in late April 1981 that it would restrict auto exports to the U.S. (Bovard 1991, 93).

U.S. auto manufacturers subsequently enjoyed record profits. American consumers paid the bill: The voluntary export restraint on Japanese automobiles raised the average price of a new car by $1,650 (Collyns and Dunaway 1987, 159).

Campaign Contributions and Legislative Failure

Two other sources of the policy bias toward special interests have been identified in the literature. One explanation asserts that incumbents favor organized interests in exchange for campaign contributions (Baron 1994; Grossman and Helpman 1996). Another invokes "legislative failure," that is, legislative norms or organization characteristics that are dysfunctional from an aggregate welfare perspective.[1] The canonical story has members of Congress approve one anothers' inefficient pork-barrel projects in quid-pro-quo logrolls (Weingast, Shepsle, and Johnsen 1981). Alternatively, "high demanders" are said to self-select onto powerful congressional committees that have jurisdiction over the pork-barrel projects that benefit their districts (Shepsle 1978).

There are limitations to the explanatory power of approaches that emphasize the role of monetary contributions made by interest groups or rely on specific features of Congress. The phenomenon to be explained—the policy bias toward special interests—is observed across many countries with different political processes and institutions. Inefficient forms of public policy are common not only in the United States but also in countries where monetary contributions to political candidates play an insignificant role, in some cases because campaigns are publicly financed. Similarly, pork-barrel politics is not restricted to the U.S. Congress; it also occurs in legislatures controlled by strong political parties that enforce majoritarian party-line voting rather than universalistic logrolls or deference to stacked committees.

AN INFORMATION RATIONALE

This article explains the policy bias toward special interests in a framework with rational, utility-maximizing agents. Two ingredients feed into the explanation: electoral competition and information asymmetries. The latter arise endogenously because of the Olsonian free-rider problem.

First, the theory is based on the assumption that agents are rational. This assumption allows voters to be ill informed, but it requires that they understand the nature of the game: Rational voters cannot be systematically fooled. Second, the theory demonstrates why incumbents can achieve a net gain in political support if they bias policy toward a well-informed minority at the expense of an ill-informed majority, even if the losses imposed on the general public exceed the gains enjoyed by special interests. Third, the underlying information asymmetry is derived endogenously and linked explicitly to the free-rider problem. Fourth, the theory is relatively simple and institution-free. It does not rely on specific features of political processes and institutions that govern any one policy issue or country, except for the one feature that modern democracies have in common: regular free elections in which a majority of the electorate can vote the incumbent government out of office. According to the principle of Occam's Razor (entities should not be multiplied needlessly), a simple explanation is preferable to a more complex one that invokes idiosyncratic distortions.

I argue that special interests prevail because they are better able to monitor the incumbent's activities than are diffuse interests. For example, farm households in

[1] Of course, we simply shift the problem when we invoke dysfunctional institutions to explain economic inefficiencies: It remains to be explained why Pareto-improving institutional change fails to occur.

developed countries are generally well informed about legislation dealing with agricultural price supports and subsidies, and they can assess fairly accurately whether and how their political representatives contributed to the passage of an agricultural bill or to the size of price supports and subsidies the bill promises to deliver. In contrast, the huge majority of nonfarm households is unlikely to know that an agricultural bill was passed at all, let alone its details or the contributions made by their political representatives. Members of the general public may well notice the decrease in their real disposable income caused by the increase in food prices and taxes, but they can assign political blame for the loss only very imprecisely. In developing countries, in contrast, it is the urban poor who are well aware of the (subsidized) price of bread and other basic foods, while the welfare of farmers depends on random shifts in the demand for and supply of agricultural products in domestic and international markets, which interact in complicated and opaque ways with actions taken by the government.

Because special interests are better able to monitor the quality of their political representation, incumbents have electoral incentives to bias policy toward special interests. By doing so, they can mimic an increase in quality of life for special interests at the expense of an apparent decrease in quality of life for the general public. Compared to the ill-informed majority, the well-informed minority places a higher weight on the possibility that the observed policy outcome is caused by the incumbent's action rather than other random factors. This difference arises because the signal extraction process of special interests has less confounding "noise." As a consequence, incumbents gain more political support among members of the minority than they lose among members of the majority.

The opportunistic behavior of incumbents creates competitive incentives for individuals to acquire better monitors of political performance. If the incentives to become informed are asymmetrical, then political outcomes will be biased in favor of the well informed at the expense of the ill informed. One source of asymmetry is costly information acquisition. Compared to the general public, special interests face a less severe free-rider problem because they are smaller in number and thus end up becoming better informed, with the result that they enjoy the fruits of the policy bias.

Offsetting that advantage are retrospective voting rules, which cause a welfare-improving selection bias. While elections create incentives for incumbents to follow suboptimal policies for reelection purposes, they also serve to get rid of low-quality incumbents. Incumbents of above-average quality are more likely to survive voter scrutiny and remain in office. Here, "quality" stands for some positive-valued attribute that has the potential to improve the well-being of the electorate when the candidate holds power. Examples are leadership skills, competence in forging legislative coalitions, economic or foreign policy expertise, effectiveness in dealing with corruption or government waste, and the ability to create a political environment that is conducive to economic growth.

THE FORMAL MODEL

Policy and Selection Biases

The model consists of N voters indexed $h = 1, \ldots, N$, where $N \geq 3$ and is an odd number. The set of voters is denoted **N**. The electorate can be decomposed into two homogeneous groups, a majority whose M individual members are indexed by j, and a minority whose $N - M$ individual members are indexed by i, where $j = 1, \ldots, M$, $i = M + 1, \ldots, N$, and $(N + 1)/2 \leq M < N$. These two groups will also be referred to as the general public (majority, j) and the special interest (minority, i). The members of each group are homogeneous; the parameters that are common to all members of the majority and minority are indexed by J and I, respectively.

Each voter h desires the incumbent policymaker to take a favorable action, $\hat{a}_h$; the voter's utility decreases quadratically with the distance between the action taken by the policymaker, $a \in R$, and the voter's ideal point, $\hat{a}_h$. The general public's preferences over the policymaker's action conflict with those of the special interest: $\hat{a}_J \neq \hat{a}_I$.

Political candidates are characterized by a quality parameter, q, which is randomly drawn from a normal distribution with zero mean and strictly positive but finite variance, σ_q^2. Voter utility increases with the incumbent's quality.[2] The majority and the minority thus have a common interest: to ensure that an incumbent of above-average quality sets policy.

In summary, voter h's indirect utility in each period is equal to

$$U_h = -\tfrac{1}{2}(a - \hat{a}_h)^2 + q. \tag{1}$$

Neither the general public nor the special interest can directly observe the action taken by the incumbent; they also cannot directly observe candidate quality. The public can, however, form indirect and imperfect inferences about the incumbent's quality based on observed policy outcomes. Formally, I assume that each individual observes an individual-specific outcome of the policy process:

$$\Pi_h = -\tfrac{1}{2}(a - \hat{a}_h)^2 + q + p_h, \tag{2}$$

where p_h is an individual-specific process shock, which is randomly drawn from a normal distribution with zero mean and strictly positive but finite variance, $\sigma_{p,h}^2$. The individuals' process shocks are independently distributed. In a simple way, the process shock captures the random nature of the interaction of multiple (unmodelled) actors and circumstances that affect the outcome

[2] For analytical simplicity, candidate quality enters the utility function additively. For some interpretations of candidate quality, the separability of the policymaker's action and quality might be considered implausible. Alternatively, we could have the distance between the policymaker's action and the voters' ideal points depend on the unknown state of the world. High-quality candidates would have more accurate (lower variance) estimates of the state of the world than would low-quality candidates, with positive consequences for the voters' (quadratic) utilities. The qualitative results of the analysis are robust.

of complex political and economic processes and thus have an effect on voter utility.

In practice, special interests are better informed than is the general public: $\sigma^2_{p,J} > \sigma^2_{p,I}$. They are better able to disentangle whether observed policy outcomes are caused by incumbent action, incumbent quality, or other random factors confounding their inferences. My analysis is not predicated, however, on the assumption that the minority group is better informed than the majority group. Instead, I first derive the equilibrium without specifying which group (if any) enjoys an information advantage: $\sigma^2_{p,J} \gtreqless \sigma^2_{p,I}$. Later the information asymmetry across groups is derived endogenously by assuming that each individual h can pay a cost, c_h, to lower the variance ($\sigma^2_{p,h}$) of her observation, Π_h.

Political candidates care about aggregate welfare and about holding power. Their indirect utility in each period is given by

$$\sum_{h=1}^{N} U_h + S, \tag{3}$$

where the sum of individual utilities stands for aggregate welfare, and the index variable S takes on the value $\bar{S}$ if the political candidate is in power in the period under consideration, the value zero otherwise, $\bar{S} \in (0, \infty)$. The parameter $\bar{S}$ captures the degree to which political candidates are office seeking: It is equal to zero if they are concerned only with aggregate welfare and goes to infinity as the (re)election goal becomes dominant.

The expression for aggregate welfare weighs the individual utilities equally. One distinguishing characteristic of special interests might be that they care more strongly about a certain policy issue than does the general public. This possibility is easily integrated into the model by having the incumbent weigh minority utility more heavily. The qualitative results of the analysis, which defines policy and selection biases relative to the benchmark outcome that maximizes aggregate welfare, are not affected; the benchmark would simply shift with higher weights on minority utility. Similarly, the assumption that the incumbent cares about aggregate welfare is not crucial. Alternatively, we might assume that the incumbent represents the majority group (in the sense of sharing its ideal point). The question, then, is to what extent the incumbent will disregard constituency interests and pander to the minority group to increase the chance of reelection.

Finally, I assume that candidates themselves are uncertain about their quality. One plausible interpretation of quality has both candidates and voters imperfectly informed about the fit between the candidates' known positions and the unknown characteristics of the political and economic environment. For example, candidates might differ in their ideology about the workings of the economy (Roemer 1994), in which case quality would stand for the objective accuracy of their ideology.

The assumption that incumbents are uncertain about their quality at the time of setting policy serves an important analytical purpose (Lohmann 1998). It isolates the policy bias that arises because incumbents seek to pretend that their quality is higher than it truly is; confounding policy effects would arise if incumbents knew their own quality, but voters did not. In the latter case of asymmetric information, high-quality incumbents would set policy so as to separate themselves from types of lower quality, or low-quality incumbents would mimic the behavior of higher quality types (Rogoff 1990). By excluding such asymmetric information, I demonstrate that the policy bias arises because of the strategic interaction between incumbents and their voters, not because of the strategic interaction between different types of incumbents. The policy bias is driven by incumbents' desire to make voters believe they are of above-average quality, regardless of their actual quality, and not by their desire to separate themselves from lower quality types or mimic the behavior of higher quality types.

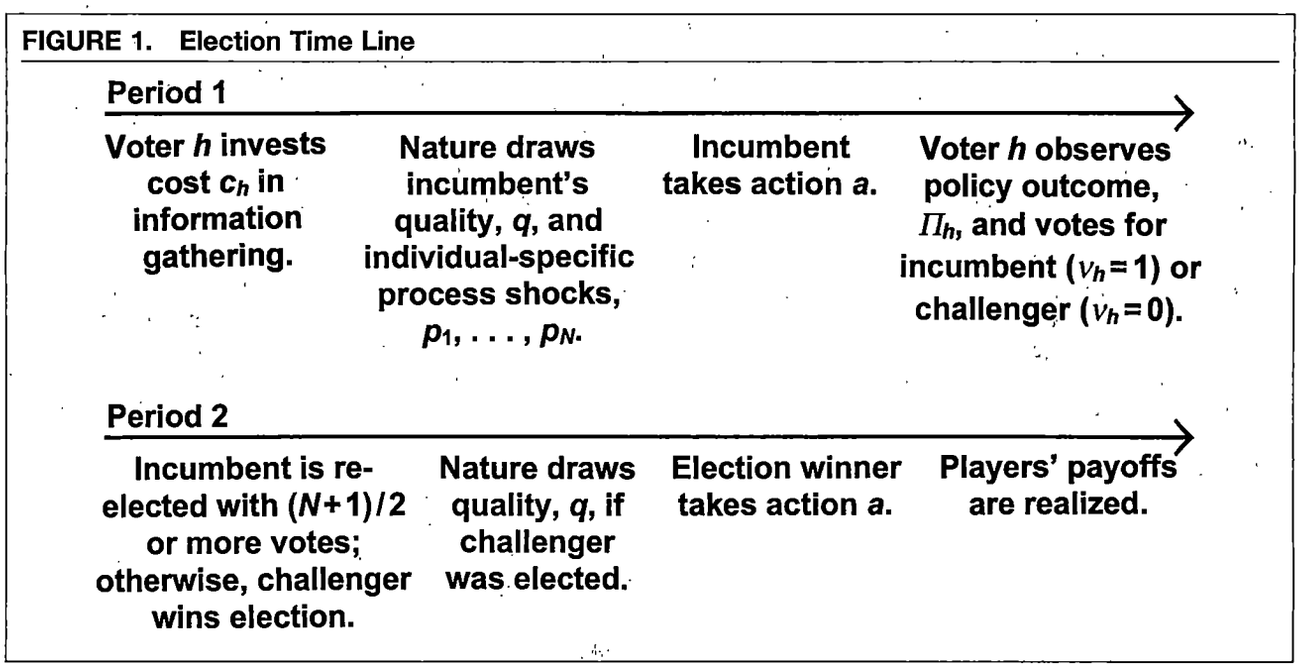
The model has two periods, indexed $t = 1,2$. Supplement 1, available upon request, argues that my conclusions (policy and selection biases and retrospective voting) are robust in the presence of an infinite horizon. Time superscripts are omitted whenever possible to avoid notational clutter. For simplicity, I assume that second-period utilities are not discounted.

Figure 1 lists the sequence of events. In the first period, the incumbent takes an action, a. Then the incumbent's quality, q, and the individual-specific process shocks, $p_1, \ldots, p_N$, are drawn. Voter h does not separately observe the incumbent's action and Nature's draws, only the policy outcome, Π_h, which is jointly determined by the policymaker's action and the quality and process shocks. Voter h then chooses whether to vote for the incumbent ($v_h = 1$) or the challenger ($v_h = 0$). The incumbent remains in power if he receives a majority of the vote, that is, at least $(N + 1)/2$ votes. Otherwise, he is replaced by a challenger. The election winner then takes an action, a. When a challenger wins, his q is a "fresh draw." Finally, the game ends, and the players' payoffs are realized.

The equilibrium concept is a refinement of perfect Bayesian Nash. Voters are restricted to use weakly undominated voting strategies. This refinement is invoked to eliminate implausible voting equilibria of the following kind. Suppose each voter believes that all other voters will vote for one candidate independently of their private information. This candidate will win the election for sure. As a consequence, each voter is indifferent between voting for or against this candidate, and she thus may choose to follow a pure strategy of voting for this candidate independently of her private information. Thus, one candidate may win the election even though it is common knowledge that a majority of voters expect to be better off if the other candidate wins.

Another complication arises in the voting setting analyzed here: the "swing voter's curse" (Feddersen and Pesendorfer 1996). Each voter has private information (the individual-specific realization of the policy outcome, Π_h) about a common value that affects the

FIGURE 1. Election Time Line



utility levels of all voters (the incumbent's quality, q). As a consequence, each individual has incentives to condition her vote on the information implied by her vote being decisive for the election outcome. Voter h's voting decision is based on her posterior expectation about the incumbent's quality, $E(q|\Pi_h, \Sigma_{k\in\mathcal{N}\setminus h}\, v_k = (N-1)/2)$, where E is an expectations operator. This expectation is conditioned not only on her private information, Π_h, but also on the information implied by exactly $(N-1)/2$ other individuals voting for the incumbent and $(N-1)/2$ other individuals voting for the challenger. (The swing voter's curse besets voters who fail to condition their vote on the information implied by their vote being decisive, analogous to the winner's curse, which besets auction bidders who fail to condition their bids on the information implied by their casting the winning bid.)

Here, I restrict attention to the case in which h votes solely based on her private information without taking into account the information implied by her casting the decisive vote. Supplement 2, available upon request, proves that my conclusions are robust with respect to the swing voter's curse.

DEFINITION. *The unique perfect Bayesian Nash equilibrium, refined to rule out weakly dominated strategies, is given by the incumbent's first-period action,* a_1^*; *the individuals' voting rule,* $v_h^*(\Pi_h)$; *and the election winner's second-period action,* a_2^*. *(The superscript * indexes equilibrium values.) The players' strategies are best responses. The voters use Bayes's rule to update their beliefs.*

PROPOSITION 1. *The unique perfect Bayesian Nash equilibrium, refined to rule out weakly dominated strategies, is given by the incumbent's biased first-period action,*

$$a_1^* = \hat{a}_J \frac{M(1+\phi_J\bar{S}\varepsilon)}{N + M\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon} + \hat{a}_I \frac{(N-M)(1+\phi_I\bar{S}\varepsilon)}{N + M\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon}, \tag{4}$$

the voters' retrospective voting rule,

$$v_h^*(\Pi_h) = \begin{cases} 1 & \text{if } \Pi_h \geq -\tfrac{1}{2}(a_1^* - \hat{a}_h)^2 \\ 0 & \text{otherwise} \end{cases}, \tag{5}$$

and the election winner's unbiased second-period action,

$$a_2^* = \hat{a}_J \frac{M}{N} + \hat{a}_I \frac{N-M}{N}, \tag{6}$$

where $\phi_h \equiv f_h(0)$, f_h *is the probability density function function of* $q + p_h$, *and*

$$\varepsilon \equiv \frac{(N-1)!}{\left(\frac{N-1}{2}\right)!\left(\frac{N-1}{2}\right)!}\left(\frac{1}{2}\right)^{N-1}.$$

Proof. The model is solved by backward induction. In the second (and last) period of the game, there are no reelection incentives, so the election winner's action maximizes second-period aggregate welfare. His first-order condition is given by

$$\frac{d\left[E\sum_{h=1}^{N} U_h^{t=2}(a_2) + \bar{S}\right]}{da_2} = -M(a_2 - \hat{a}_J) - (N-M)(a_2 - \hat{a}_I) = 0. \tag{7}$$

His second-order condition is negative:

$$\frac{d^2\left[E\sum_{h=1}^{N} U_h^{t=2}(a_2) + \bar{S}\right]}{(da_2)^2} = -N < 0. \quad (8)$$

It follows that the equilibrium second-period action, a_2^*, given in equation 6, is a weighted average of the two groups' ideal points, with the weights reflecting the groups' relative size.

At the end of the first period, each individual must decide whether to vote for the incumbent or the challenger. Her vote cannot ex post change her first-period utility—it can only influence her second-period utility. The voter supports the incumbent if she expects to be better off if he remains in power. Since the election winner's action is independent of who wins the election, the only difference between the two candidates lies in their expected qualities. The voter has no information (other than her prior beliefs) about the challenger's quality, which is zero in expectation. She cannot directly observe the incumbent's quality, but at the time of the vote she observes the individual-specific policy outcome. The voter supports the incumbent if his estimated q is greater than the expected q of the challenger, which is zero:

$$\nu_h = \begin{cases} 1 & \text{if } E(q|\Pi_h) \geq E(q) = 0 \\ 0 & \text{otherwise} \end{cases} \quad (9)$$

The estimate $E(q|\Pi_h)$ is derived using equation 2, which relates q and Π_h. There is no uncertainty about the first-period action, implying that $a_1 = a_1^*$ holds in equilibrium. It follows that equation 2 is equivalent to

$$q + p_h = \Pi_h + \tfrac{1}{2}(a_1^* - \hat{a}_h)^2. \quad (10)$$

Voter h cannot directly observe the individual components of the left-hand side of equation 10, q and p_h, but she knows that the sum $q + p_h$ is equal to the right-hand side of this equation. She is privately informed about the value of the first term on the right-hand side, Π_h; the value of the second term, $\tfrac{1}{2}(a_1^* - \hat{a}_h)^2$, is not directly observed but is known in equilibrium. Her posterior estimate of the incumbent's quality, $E(q|\Pi_h)$, solves the standard signal extraction problem (DeGroot 1970, 167). The value $\Pi_h + \tfrac{1}{2}(a_1^* - \hat{a}_h)^2$ can be thought of as a random sample from a normal distribution with unknown mean q and variance σ_q^2, where the prior distribution of the mean q is normal with mean zero and variance $\sigma_{p,h}^2$. Equation 10 thus implies that voter h's posterior expectation of the incumbent's quality is a function of the observed policy outcome, Π_h:

$$E(q|\Pi_h) = \left(\frac{\sigma_q^2}{\sigma_q^2 + \sigma_{p,h}^2}\right)[\Pi_h + \tfrac{1}{2}(a_1^* - \hat{a}_h)^2]. \quad (11)$$

Equations 9 and 11 imply that h supports the incumbent if

$$\Pi_h + \tfrac{1}{2}(a_1^* - \hat{a}_h)^2 \geq 0. \quad (12)$$

The voter thus follows the equilibrium voting rule, ν_h^*, given in equation 5.

In the first period, the incumbent trades off the first-period welfare and second-period reelection consequences of his action, a_1. His first-order condition is given by

$$\frac{d\left[E\sum_{h=1}^{N} U_h^{t=1}(a_1) + \bar{S} + E\sum_{h=1}^{N} U_h^{t=2} + \Pr(S = \bar{S}|a_1)\bar{S}\right]}{da_1}$$
$$= -M(a_1 - \hat{a}_J) - (N - M)(a_1 - \hat{a}_I) + \frac{d\Pr(S = \bar{S}|a_1)}{da_1}\bar{S} = 0, \quad (13)$$

where $\Pr(S = \bar{S}|a_1)$ is the probability that the incumbent survives as a function of his action, a_1. To examine how this survival probability varies with a_1, I analyze how h's vote is affected by a_1:

$$\Pr(\nu_h = 1|a_1) = \Pr[\Pi_h \geq -\tfrac{1}{2}(a_1^* - \hat{a}_h)^2]$$
$$= \Pr[q + p_h \geq \tfrac{1}{2}(a_1 - \hat{a}_h)^2 - \tfrac{1}{2}(a_1^* - \hat{a}_h)^2]$$
$$= 1 - F_h[\tfrac{1}{2}(a_1 - \hat{a}_h)^2 - \tfrac{1}{2}(a_1^* - \hat{a}_h)^2], \quad (14)$$

where F_h is the cumulative distribution function of $q + p_h$. Taking as given the retrospective voting rule, the incumbent can influence the probability that h supports him:

$$\frac{d\Pr(\nu_h = 1|a_1)}{da_1} = -(a_1 - \hat{a}_h)f_h[\tfrac{1}{2}(a_1 - \hat{a}_h)^2 - \tfrac{1}{2}(a_1^* - \hat{a}_h)^2], \quad (15)$$

where f_h is the probability density function of $q + p_h$. The incumbent's probability of survival is thus a function of his action, a_1:

$$\frac{d\Pr(S = \bar{S}|a_1)}{da_1} = -\varepsilon M(a_1 - \hat{a}_J)f_J[\tfrac{1}{2}(a_1 - \hat{a}_J)^2 - \tfrac{1}{2}(a_1^* - \hat{a}_J)^2] - \varepsilon(N - M)(a_1 - \hat{a}_1)f_I[\tfrac{1}{2}(a_1 - \hat{a}_I)^2 - \tfrac{1}{2}(a_1^* - \hat{a}_I)^2] = -\varepsilon M(a_1 - \hat{a}_J)\phi_J - \varepsilon(N - M)(a_1 - \hat{a}_I)\phi_I, \quad (16)$$

where $\phi_h \equiv f_h(0) = f_h(\tfrac{1}{2}(a_1 - \hat{a}_h)^2 - \tfrac{1}{2}(a_1^* - \hat{a}_h)^2)$, noting that $a_1 = a_1^*$ in equilibrium. The parameter ε is the probability that a given vote is decisive for the incumbent's survival. Individual h's vote is decisive if exactly $(N - 1)/2$ voters choose the incumbent and the remaining $(N - 1)/2$ voters choose the challenger. In equilibrium the unconditional probability that h votes for the incumbent, $\Pr(\nu_h = 1)$, is equal to $1 - F_h(0)$, or one-half. Thus, the probability that her vote is decisive is equal to the binomial probability $b((N - 1)/2; N - 1, \tfrac{1}{2})$ that $N - 1$ Bernoulli trials result in $(N - 1)/2$ successes, with the probability of a success given by one-half, that is,

$$\frac{(N-1)!}{\left(\frac{N-1}{2}\right)!\left(\frac{N-1}{2}\right)!}\left(\frac{1}{2}\right)^{N-1}.$$

The incumbent's first-order condition, equation 13, can now be rewritten as

$$\frac{d\left[E\sum_{h=1}^{N} U_h^{t=1}(a_1) + \bar{S} + E\sum_{h=1}^{N} U_h^{t=2} + \Pr(S=\bar{S}|a_1)\bar{S}\right]}{da_1}$$
$$= -M(a_1 - \hat{a}_J)(1 + \phi_J\bar{S}\varepsilon)$$
$$- (N-M)(a_1 - \hat{a}_I)(1 + \phi_I\bar{S}\varepsilon) = 0. \quad \textbf{(17)}$$

His second-order condition is negative:

$$\frac{d^2\left[E\sum_{h=1}^{N} U_h^{t=1}(a_1) + \bar{S} + E\sum_{h=1}^{N} U_h^{t=2} + \Pr(S=\bar{S}|a_1)\bar{S}\right]}{(da_1)^2}$$
$$= -M(1 + \phi_J\bar{S}\varepsilon) - (N-M)(1 + \phi_I\bar{S}\varepsilon) < 0. \quad \textbf{(18)}$$

The equilibrium first-period action, a_1^*, given in equation 4, follows. Supplement 3, available upon request, proves that the equilibrium is unique. *Q.E.D.*

Two features of the proof are worth highlighting. First, equation 11 contains the central intuition underlying the policy bias. The better informed is voter h, or the lower is the variance ($\sigma_{p,h}^2$) of the process shock (p_h) confounding her inferences, the higher is the weight she places on the observation Π_h, and the lower is the weight she places on her prior expectation of the incumbent's quality, that is, zero. The logic of the signal extraction problem drives the result that a well-informed voter reacts more sensitively to policy favors than does an ill-informed voter, which in turn creates incentives for the incumbent to grant such favors to well-informed voters at the expense of ill-informed voters.

Second, the voter in effect subtracts $\frac{1}{2}(a_1^* - \hat{a}_h)^2$ from the observed policy outcome Π_h, thereby rationally discounting the degree to which the policy outcome is favorable by the amount of the equilibrium policy bias. The incumbent cannot fool the voter: *In equilibrium* the incumbent's attempt to improve his reelection prospects is futile. The unconditional probability that h will support the incumbent's candidacy is equal to $1 - F_h(0)$, or one-half, implying that the incumbent's unconditional probability of survival is equal to

$$\Pr(S=\bar{S}) = \sum_{V=1}^{(N+1)/2} b\left(\frac{(N-1)}{2} + V; N, \frac{1}{2}\right) = \frac{1}{2}. \quad \textbf{(19)}$$

That the incumbent cannot systematically improve his reelection chances follows from the assumption of voter rationality: The voters' signal extraction problem takes into account the effect of the incumbent's manipulations on observed policy outcomes. Nonetheless, the incumbent is trapped into producing a counterproductive policy bias because voters expect him to favor special interests. If he fails to satisfy their expectations, then he reduces his reelection chances to an unacceptable level. Thus, the voters' expectations are fulfilled in equilibrium.

PROPOSITION 2. *The policy bias is zero if there is no conflict of interest or information disparity between the majority and minority, or if the incumbent places zero weight on the reelection objective:*

$$a_1^* = a_2^* \textit{ if } \hat{a}_J = \hat{a}_I, \textit{ or if } \sigma_{p,J}^2 = \sigma_{p,I}^2, \textit{ or if } \bar{S} = 0. \quad \textbf{(20)}$$

Otherwise, policy is biased toward the well-informed group at the expense of the ill-informed group:

$$|a_1^* - \hat{a}_J| > |a_2^* - \hat{a}_J| \textit{ and } |a_1^* - \hat{a}_I| < |a_2^* - \hat{a}_I| \textit{ if } \sigma_{p,J}^2 > \sigma_{p,I}^2;$$
$$|a_1^* - \hat{a}_J| < |a_2^* - \hat{a}_J| \textit{ and } |a_1^* - \hat{a}_I| > |a_2^* - \hat{a}_I| \textit{ if } \sigma_{p,J}^2 < \sigma_{p,I}^2. \quad \textbf{(21)}$$

The size of the policy bias increases with the conflict of interest and the information disparity between the two groups and with the weight the incumbent places on the reelection objective:

$$sign\left(\frac{d|a_1^* - a_2^*|}{d|\hat{a}_J - \hat{a}_I|}\right) = sign\left(\frac{d|a_1^* - a_2^*|}{d|\sigma_{p,J}^2 - \sigma_{p,I}^2|}\right)$$
$$= sign\left(\frac{d|a_1^* - a_2^*|}{d\bar{S}}\right) \gtrless 0. \quad \textbf{(22)}$$

Proof. The second-period action, a_2^*, which maximizes second-period aggregate welfare, serves as a normative benchmark. A policy bias is obtained if the action taken by an office-seeking incumbent in the first period, a_1^*, differs from this benchmark. The difference is given by

$$a_1^* - a_2^* = \frac{M(N-M)(\hat{a}_J - \hat{a}_I)(\phi_J - \phi_I)\bar{S}\varepsilon}{N + M\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon}. \quad \textbf{(23)}$$

The conditions under which there is no policy bias, listed in equation 20, follow directly from equation 23, noting that $\phi_J = \phi_I$ is equivalent to $\sigma_{p,J}^2 = \sigma_{p,I}^2$. If none of the conditions in equation 20 are fulfilled, then the incumbent systematically favors the group that is better able to monitor his performance. The conditions determining the direction of the policy bias, listed in equation 21, follow immediately from equation 23; that is, if $\phi_J < \phi_I$ or, equivalently, $\sigma_{p,J}^2 > \sigma_{p,I}^2$, then $\hat{a}_J > \hat{a}_I$ implies $a_1^* < a_2^*$ and $\hat{a}_J < \hat{a}_I$ implies $a_1^* > a_2^*$; and vice versa. The comparative statics listed in equation 22 also follow straightforwardly from equation 23, noting that $|\phi_J - \phi_I|$ is strictly increasing in $|\sigma_{p,J}^2 - \sigma_{p,I}^2|$. (To make sense of this proof, it is useful to recall that $\phi_h \equiv f_h(0)$ is inversely correlated with $\sigma_{p,h}^2$, the variance of the normal density f_h.) *Q.E.D.*

These results demonstrate that the policy bias, $a_1^* \neq a_2^*$, is driven by the conflict of interest, $\hat{a}_J \neq \hat{a}_I$, the information disparity between the majority and minority, $\sigma_{p,J}^2 \neq \sigma_{p,I}^2$, and the incumbent's reelection objective, $\bar{S} > 0$. It is interesting to note that the *direction* of the policy bias depends only on the information disparity between the two groups, while the *size* of the policy bias depends on the conflict of interest and the information disparity between the two groups as well as the weight the incumbent places on the reelection objective.

PROPOSITION 3. *The probability that the incumbent survives the election increases with his quality:*

$$\frac{d\,Pr(S = \bar{S}|q)}{dq} > 0. \tag{24}$$

As a result, the expectation of the incumbent's quality conditional on survival is strictly positive:

$$E(q|S = \bar{S}) > 0. \tag{25}$$

The size of this selection bias increases with the degree to which each voter is informed:

$$\frac{dE(q|S = \bar{S})}{d(\sigma_{p,h}^2)} < 0. \tag{26}$$

Proof. Compared with an incumbent of low quality, a high-quality type has better reelection chances because voters are more likely to experience favorable policy outcomes. Formally, equation 24 follows from

$$\frac{d\,\Pr(S = \bar{S}|q)}{dq} = \varepsilon \sum_{h=1}^{N} \left[\frac{d\,\Pr(\nu_h = 1|q)}{dq}\right]$$

$$= \varepsilon \sum_{h=1}^{N} \left[\frac{d\,\Pr[\Pi_h \geq -\tfrac{1}{2}(a_1^* - \hat{a}_h)^2|q]}{dq}\right]$$

$$= \varepsilon \sum_{h=1}^{N} \left[\frac{d\,\Pr(p_h \geq -q)}{dq}\right] = \varepsilon \sum_{h=1}^{N} \left[\frac{d[1 - G_h(-q)]}{dq}\right]$$

$$= \varepsilon \sum_{h=1}^{N} [g_h(-q)] > 0, \tag{27}$$

where G_h is the cumulative distribution function of p_h, and g_h is the probability density function.

A political survivor—a reelected incumbent—tends to be of above-average quality. Formally, equation 28 can be rewritten as

$$E\left(q|\sum_{h=1}^{N} \nu_h \geq \frac{N+1}{2}\right) > 0, \tag{28}$$

which holds if

$$\Pr(q \geq 0|\nu_h = 1) > \Pr(q < 0|\nu_h = 1). \tag{29}$$

Applying Bayes's rule, the expressions on the left- and right-hand sides of equation 29 can be rewritten as

$$\Pr(q \geq 0|\nu_h = 1)$$

$$= \frac{\Pr(\nu_h = 1|q \geq 0)\Pr(q \geq 0)}{\Pr(\nu_h = 1|q \geq 0)\Pr(q \geq 0) + \Pr(\nu_h = 1|q < 0)\Pr(q < 0)}, \tag{30}$$

$$\Pr(q < 0|\nu_h = 1)$$

$$= \frac{\Pr(\nu_h = 1|q < 0)\Pr(q < 0)}{\Pr(\nu_h = 1|q \geq 0)\Pr(q \geq 0) + \Pr(\nu_h = 1|q < 0)\Pr(q < 0)}. \tag{31}$$

Noting that $\Pr(q \geq 0) = \Pr(q < 0)$, equations 30 and 31 imply that the inequality in equation 29 holds if

$$\Pr(\nu_h = 1|q \geq 0) > \Pr(\nu_h = 1|q < 0). \tag{32}$$

Noting that $\Pr(\nu_h = 1|q) = 1 - G_h(q)$, equation 32 is equivalent to

$$G_h(-q|q \geq 0) < G_h(-q|q < 0). \tag{33}$$

This establishes the validity of equation 25.

A better informed voter can more readily disentangle the effects of candidate quality and other random factors on observed policy outcomes. Formally, equation 26 holds if

$$\frac{d[\Pr(q \geq 0|\nu_h = 1) - \Pr(q < 0|\nu_h = 1)]}{d(\sigma_{p,h}^2)} < 0. \tag{34}$$

Noting that $[\Pr(q \geq 0|\nu_h = 1) - \Pr(q < 0|\nu_h = 1)]$ is strictly increasing in $[\Pr(\nu_h = 1|q \geq 0) - \Pr(\nu_h = 1|q < 0)]$ (see equations 30 and 31) and that $\Pr(\nu_h = 1|q) = 1 - G_h(q)$, the inequality in equation 34 holds since

$$\frac{d[\Pr(\nu_h = 1|q \geq 0) - \Pr(\nu_h = 1|q < 0)]}{d(\sigma_{p,h}^2)}$$

$$= \frac{d[G_h(-q|q < 0) - G_h(-q|q \geq 0)]}{d(\sigma_{p,h}^2)} < 0, \tag{35}$$

noting that $\sigma_{p,h}^2$ is the variance of the probability density function g_h. *Q.E.D.*

PROPOSITION 4. *The policy bias reduces first-period expected aggregate welfare, and the selection bias increases second-period expected aggregate welfare:*

$$E\sum_{h=1}^{N} U_h^{t=2} - E\sum_{h=1}^{N} U_h^{t=1}$$

$$= \frac{\tfrac{1}{2}(\hat{a}_J - \hat{a}_I)^2(\phi_J - \phi_I)^2\bar{S}^2\varepsilon^2 M(N - M)}{N[M(1 + \phi_J\bar{S}\varepsilon) + (N - M)(1 + \phi_I\bar{S}\varepsilon)]^2}$$

$$+ \frac{1}{2}E(q|S = \bar{S})N. \tag{36}$$

Proof. First-period expected welfare is given by

$$E \sum_{h=1}^{N} U_h^{t=1} = -\frac{1}{2}(\hat{a}_J - \hat{a}_I)^2$$

$$\frac{M(N-M)[M(1+\phi_J\bar{S}\varepsilon)^2 + (N-M)(1+\phi_I\bar{S}\varepsilon)^2]}{[M(1+\phi_J\bar{S}\varepsilon) + (N-M)(1+\phi_I\bar{S}\varepsilon)]^2}, \tag{37}$$

and second-period expected welfare is given by

$$E \sum_{h=1}^{N} U_h^{t=2} = -\frac{1}{2}(\hat{a}_J - \hat{a}_I)^2 \frac{M(N-M)}{N}$$

$$+ \frac{1}{2} E(q|S = \bar{S})N. \tag{38}$$

In the first period there is no potential for a selection bias, only for a policy bias. The expression $-\frac{1}{2}(\hat{a}_J - \hat{a}_I)^2 M(N-M)/N$, which reflects second-period expected welfare minus the expected gains from the selection bias, serves as a normative benchmark that allows us to assess the welfare costs of the first-period policy bias. The first term in equation 38 is equal to the difference between this benchmark and first-period expected welfare. The difference is strictly positive, implying that the policy bias is associated with a welfare loss.

In the second period, there is no potential for a policy bias, only for a selection bias. The second term in equation 38 is equal to the difference between second-period welfare and the benchmark. The difference is strictly positive, implying that the selection bias increases second-period expected welfare. *Q.E.D.*

The voters' retrospective voting rules thus cause a welfare-reducing policy bias and a welfare-improving selection bias. While elections create incentives for incumbents to follow suboptimal policies for reelection purposes, they also serve to get rid of low-quality candidates.

Group Size and Endogenous Information Asymmetries

The results presented so far are based on an exogenously given information disparity between the majority and minority. My analysis is not, however, predicated on the assumption that the members of the minority group are better informed than are the members of the majority group. If, for reasons exogenous to my theory, the majority happens to be better informed, then my theory would imply that policy will be biased in favor of the majority.

This section endogenizes the information asymmetry by examining the incentives of the members of each group to become informed or, equivalently, to acquire a lower-variance monitor of political performance at a cost privately incurred by each individual. Each voter chooses how many resources to invest in gathering information. (Alternatively, we might think of voters as inhabiting a complex world in which there is virtually an infinite amount of information competing for their attention. In the presence of some, possibly small, cognitive cost of information processing, voters must choose how much costly attention to pay to political information.)

By becoming better informed, the individual achieves two ends. First, she biases policy in favor of her group or contributes toward undoing a policy bias favoring the other group. Second, she improves the selection bias.

The private cost of information gathering in conjunction with the collective implications of the policy and selection biases imply that the individuals' information-gathering decisions are subject to a free-rider problem. This problem is conceptually more complicated than the standard free-rider problem. A within-group free-rider problem is nested in a free-rider problem shared by the entire electorate. With regard to the policy bias, each individual has goals in common with one group of individuals, whereas her goals conflict with those of the other group. A free-rider problem arises because she would prefer other members of her group to bear the costs of information gathering. Yet, she has competitive incentives to gather information so as to undo the effect of the information investments made by the opposing group. With regard to the selection bias, both groups have a common goal: to gather information in order to increase the expected quality of the future incumbent. This collective enterprise is subject to a free-rider problem common to all voters.

Given the importance of group size in standard analyses of the free-rider problem (Olson 1965), it is clearly of interest to examine the degree to which a group is able to overcome this problem as a function of its size. I analyze whether asymmetries in group size translate into an information disparity that in turn causes a policy bias. From a welfare perspective, I examine whether information is under- or oversupplied in equilibrium.

At a stage prior to the game analyzed earlier, voter h decides whether to acquire costly information (see Figure 1). Her choice variable is the private cost of information gathering, c_h, which enters additively into her utility function. The aggregate costs of information acquisition affect the utility function of political candidates who care about aggregate welfare. Because the information costs are sunk at the time of the first-period incumbent's and the election winner's decisions, their equilibrium strategies are not affected.

I now turn to the specification of the information-gathering technology. For an individual h who remains totally uninformed, the variance ($\sigma_{p,h}^2$) of the process shock (p_h) approaches infinity. As the voter gathers more information, the variance decreases, with decreasing marginal returns from information gathering, and goes to zero as the individual becomes fully informed. It is useful to specify the information-gathering technology in terms of the parameter $\phi_h \equiv f_h(0)$, which is inversely correlated with $\sigma_{p,h}^2$; ϕ_h ranges from one to zero as $\sigma_{p,h}^2$ varies from zero to infinity.

For the sake of concreteness, I assume a specific functional form:

$$\phi_h = \sqrt{c_h}, \tag{39}$$

where $c_h \in [0, 1]$; the endpoints of the unit interval correspond to the cases in which voter h remains uninformed and in which she becomes perfectly informed, respectively. Formally, the assumption of positive but diminishing marginal returns from information gathering is reflected in the signs of the first and second derivatives:

$$\frac{d\phi_h}{dc_h} = \frac{1}{2}\frac{1}{\sqrt{c_h}} > 0; \frac{d^2\phi_h}{(dc_h)^2} = -\frac{1}{4}\frac{1}{\sqrt[3]{c_h}} < 0. \tag{40}$$

PROPOSITION 5. *All individuals become partially informed, but each member of the minority invests more resources in information gathering than does each member of the majority:*

$$0 < c_J^* < c_I^* < 1. \tag{41}$$

Proof. The proof makes use of the individual indices, $j = 1, \ldots, M$ and $i = M + 1, \ldots, N$, even though in equilibrium the members of a given group invest identical resources in information gathering. The reason is that each individual takes as given the equilibrium amount of information purchased by all other individuals, including the members of her own group.

Individual j chooses the cost, c_j, that maximizes her two-period expected utility:

$$-c_j + EU_j^{t=1}(\phi_j(c_j)) + EU_j^{t=2}(\phi_j(c_j))$$
$$= -c_j - \frac{1}{2}(\hat{a}_J - \hat{a}_I)^2$$
$$\frac{(N-M)^2(1+\phi_I\bar{S}\varepsilon)^2}{[N+\phi_J\bar{S}\varepsilon + (M-1)\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon]^2}$$
$$-\frac{1}{2}(\hat{a}_J - \hat{a}_I)^2\frac{(N-M)^2}{N^2} + \frac{1}{2}E(q|S=\bar{S}). \tag{42}$$

Her first-order condition is given by

$$\frac{d[-c_j + EU_j^{t=1}(\phi_j(c_j)) + EU_j^{t=2}(\phi_j(c_j))]}{dc_j}$$
$$= -1 + \frac{\partial EU_j^{t=1}}{\partial\phi_j}\frac{\partial\phi_j}{\partial c_j} + \frac{1}{2}\frac{\partial E(q|S=\bar{S})}{\partial\phi_j}\frac{\partial\phi_j}{\partial c_j}$$
$$= -1$$
$$+\frac{(\hat{a}_J - \hat{a}_I)^2(N-M)^2(1+\phi_I\bar{S}\varepsilon)^2\bar{S}\varepsilon}{[N+\phi_J\bar{S}\varepsilon + (M-1)\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon]^3}$$
$$\frac{1}{2\sqrt{c_j}} + \frac{1}{2}\frac{\partial E(q|S=\bar{S})}{\partial\phi_j}\frac{1}{2\sqrt{c_j}} = 0. \tag{43}$$

Substituting equation 39 into equation 43, and noting that $\phi_j = \phi_J$ and $\phi_i = \phi_I$ in equilibrium, equation 43 can be rewritten as

$$-1 + \frac{1}{2\phi_J}\frac{(\hat{a}_J - \hat{a}_I)^2(N-M)^2(1+\phi_I\bar{S}\varepsilon)^2\bar{S}\varepsilon}{[N+M\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon]^3}$$
$$+\frac{1}{4\phi_J}\frac{\partial E(q|S=\bar{S})}{\partial\phi_j} = 0. \tag{44}$$

The corresponding equilibrium first-order condition for an individual of type I is given by

$$-1 + \frac{1}{2\phi_I}\frac{(\hat{a}_J - \hat{a}_I)^2M^2(1+\phi_J\bar{S}\varepsilon)^2\bar{S}\varepsilon}{[N+M\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon]^3}$$
$$+\frac{1}{4\phi_I}\frac{\partial E(q|S=\bar{S})}{\partial\phi_i} = 0. \tag{45}$$

Substituting equation 44 into equation 45 and simplifying, the following equation obtains:

$$\frac{(\hat{a}_J - \hat{a}_I)^2\bar{S}\varepsilon}{[N+M\phi_J\bar{S}\varepsilon + (N-M)\phi_I\bar{S}\varepsilon]^3}$$
$$[\phi_I(N-M)^2(1+\phi_I\bar{S}\varepsilon)^2 - \phi_J M^2(1+\phi_J\bar{S}\varepsilon)^2]$$
$$= (\phi_J - \phi_I)\frac{1}{2}\frac{\partial E(q|S=\bar{S})}{\partial\phi_h}. \tag{46}$$

Equation 46 is consistent with exactly one of the following three cases: (1) $M > N - M$ implies $\phi_J = \phi_I$; or (2) $M > N - M$ implies $\phi_J > \phi_I$; or (3) $M > N - M$ implies $\phi_J < \phi_I$. Case 1 contradicts the equality sign in equation 46: $M > N - M$ and $\phi_J = \phi_I$ imply that the left-hand side of equation 46 is strictly negative, while $\phi_J = \phi_I$ implies that the right-hand side is zero. Case 2 also contradicts the equality sign: $M > N - M$ and $\phi_J > \phi_I$ imply that the left-hand side is strictly negative, while $\phi_J > \phi_I$ implies that the right-hand side is strictly positive. It follows that the only case consistent with equation 46 is case 3. Moreover, the assumption of diminishing marginal returns from costly information gathering (see equation 40) eliminates corner solutions, that is, $\phi_J = \phi_I = 0$ or $\phi_J = \phi_I = 1$. It follows that $0 < c_J^* < c_I^* < 1$, as stated in equation 41. Supplement 3, available upon request, shows that the uniqueness-of-equilibrium result continues to hold when the equilibrium play characterized earlier is preceded by an information-gathering stage as specified above. *Q.E.D.*

It is worthwhile highlighting that proposition 5 does *not* imply that the minority group as a whole gathers more information than does the majority group as a whole, nor does it exclude this possibility. Proposition 5 does imply that the information asymmetry $\sigma_{p,I}^2 < \sigma_{p,J}^2$ arises endogenously as a result of special interests being smaller in number than the general public. Along with proposition 1, this implies that policy will be biased toward the minority at the expense of the majority.

PROPOSITION 6. *As the majority increases in size relative to the minority, each member of the majority invests less resources, each member of the minority more resources, in information gathering:*

$$\frac{dc_J^*}{dM} < 0, \quad \frac{dc_I^*}{dM} > 0 \quad \textit{for fixed } N. \tag{47}$$

Proof. Equation 47 follows immediately from equation 46, noting that $\phi_h = \sqrt{c_h}$. For fixed N, the case in which an increase in M leads to a decrease in ϕ_J and an increase in ϕ_I is the only case consistent with the equality sign. *Q.E.D.*

There are three reasons a decrease in the relative size of the minority strengthens the minority's, and weakens the majority's, incentives to gather costly information. First, the within-group free-rider problem becomes less severe for the minority, more severe for the majority. Second, the competitive pressures to invest in information so as to offset the information investments made by the other side increase for the minority, which now has fewer members to make such investments, and decrease for the majority, which has more members. Third, the incumbent, who cares not only about reelection but also about aggregate welfare, now places less weight on minority welfare and more weight on majority welfare; the benchmark policy absent differential information investments now becomes more disadvantageous for the minority and more advantageous for the majority. This shift in the benchmark strengthens the minority's, and weakens the majority's, incentives.

The information asymmetry between the special interest and the general public, and thus the policy bias, attains a maximum when the special interest is a single individual ($M = N - 1$) and a minimum when the special interest and the general public are approximately equal in number ($M = (N + 1)/2$). Recall, however, that the policy bias is defined relative to a benchmark that shifts with the number of individuals in each group. In the $M = N - 1$ case, the benchmark policy lies close to the majority's ideal point; in the $M = (N + 1)/2$ case, it lies approximately midway between the ideal points of the two groups. Thus, the comparative statics relating the relative size of the minority group to the size of the information asymmetry and of the policy bias do not translate directly into comparative statics about the location of the equilibrium policy relative to the majority's and minority's ideal points.

PROPOSITION 7. *From an aggregate welfare perspective, information investments driven by the policy bias are oversupplied, while those driven by the selection bias are undersupplied by a factor of* N. *The net effect is ambiguous:*

$$c_h^* \gtrless c^W, \tag{48}$$

where c^W *is the welfare-maximizing value of* c_h.

Proof. Proposition 4 implies that information-gathering efforts driven by the policy bias lead to a welfare loss. From a welfare perspective, the voters should collect zero information for the purpose of influencing the policy bias. Thus, the deadweight cost of information gathering privately incurred by each individual must be traded off only against the collective gains generated by the resulting increase in the selection bias. Moreover, from a welfare perspective, because of diminishing returns of information gathering, it is optimal for all individuals to be equally well informed: the welfare-maximizing value of c_h, c^W, is identical for all h. This value solves the first-order condition:

$$\frac{d\left[-c_h + E\sum_{h=1}^{N} U_h^{t=1}(\phi_h(c_h)) + E\sum_{h=1}^{N} U_h^{t=2}(\phi_h(c_h))\right]}{dc_h}$$

$$= -1 + 0 + \frac{N}{4\sqrt{c_h}}\frac{\partial E(q|S=\bar{S})}{\partial \phi_h}$$

$$= -1 + \frac{N}{4\phi_h}\frac{\partial E(q|S=\bar{S})}{\partial \phi_h} = 0, \tag{49}$$

where $\phi^W = \sqrt{c^W}$. A comparison of first-order condition 49 with the equilibrium first-order conditions 44 and 45 shows that, in equilibrium, information investments driven by the counterproductive policy bias are oversupplied independent of their size. In contrast, information-gathering efforts driven by the selection bias are undersupplied by a factor of N. The comparison of equations 49, 44, and 45 shows that the net effect is ambiguous. *Q.E.D.*

The result that information may be oversupplied is interesting insofar as it contradicts the standard intuition according to which voluntary and costly contributions to a public good will be suboptimally low. The possibility of oversupply arises because each group has a competitive incentive to outdo the other's information-gathering efforts so as to create a policy bias in their favor. The undersupply of information gathering geared toward the selection bias, by a factor of N, reflects the standard free-rider problem shared by an electorate of size N.

INFORMAL OUTLINE OF THE ARGUMENT

My analysis develops a precise intuition explaining why an incumbent can achieve a net gain in political support when he biases policy toward a minority of voters at the expense of a majority, even if the losses incurred by the general public exceed the gains enjoyed by special interests. Each voter would like to reelect an incumbent of above-average quality, but she cannot fully disentangle the contribution of the incumbent's quality and other forces driving the observed policy outcome. She may experience a favorable policy outcome because the incumbent is a high-quality candidate, because policy is biased in her favor, or because she is lucky. Based on the observed policy outcome, the voter forms an inference about the incumbent's quality. She does so by solving a signal extraction problem that assigns weights to various factors that influence the observed policy outcome, where the weight placed on any one factor decreases with the degree of uncertainty associated with the factor. If the observed outcome is

sufficiently favorable, the voter casts her vote for the incumbent; otherwise she votes for the challenger. The incumbent can mimic an increase in quality vis-à-vis special interests by biasing policy in their favor, at the expense of an apparent decrease in quality vis-à-vis the general public. Compared to the ill-informed majority, the well-informed minority places a higher weight on the possibility that the observed policy outcome is caused by the incumbent's quality. This is because their signal extraction problem is subject to less "noise" confounding their inferences. As a consequence, the incumbent gains more political support among members of the minority than he loses among members of the majority. By continuity, this argument holds even if the policy bias is associated with some inefficiency. The size of the policy bias increases with the information asymmetry between the special interest and the general public.

So far, my theory does not necessarily imply that policy is biased toward special interests. If, for reasons exogenous to the theory, the majority happens to be better informed than the minority, then my theory is consistent with a majoritarian bias. My theory does allow for the possibility that the information asymmetry arises endogenously as a result of individual decisions to gather costly political information (or pay costly attention to political information). The voters' anticipation of policy and selection biases creates incentives to acquire better monitors of political performance. The private costs of information acquisition in conjunction with the collective consequences of policy and selection biases imply that the voters' information-gathering efforts are subject to a free-rider problem. With regard to the policy bias, each individual would prefer other members of her group to bear the costs of information gathering, but she also has competitive incentives to gather information in order to offset the information investments made by the opposing group. Asymmetries in group size translate into an information disparity which in turn causes a counterproductive policy bias: The smaller is the minority relative to the majority, the larger is the bias. From a welfare perspective, information is oversupplied in equilibrium to the extent that the voters' information-gathering efforts are primarily driven by the counterproductive policy bias. With regard to the selection bias, all voters have incentives to become better informed in order to improve the expected quality of the future incumbent. This collective enterprise is subject to the standard free-rider problem. Information is undersupplied in equilibrium to the extent that information acquisition is primarily driven by the selection bias.

EVIDENCE

My theory generates a host of empirical implications, of which three stand out. First, policy is biased in favor of well-informed voters at the expense of ill-informed voters. To the extent that the information asymmetry arises endogenously as a result of individual decisions to gather costly information, special interests, being smaller in number, end up better informed than the general public. Second, elections give rise to selection effects on the quality of political representatives. Reelected incumbents tend to be of higher quality than defeated incumbents or the average challenger. Third, voters vote retrospectively, supporting the incumbent, if they experience good policy outcomes, or the challenger, if they experience bad policy outcomes.

Here, I compare the implications of the theory to existing empirical findings and discuss how they might be tested empirically.

Policy Bias

Standard political business cycle theory predicts a policy bias in aggregate economic variables: Incumbents seeking reelection try to manipulate the instruments of economic policy so as to improve the state of the economy before elections (Nordhaus 1975; Rogoff 1990; Rogoff and Sibert 1988). This hypothesis enjoys a strong hold on the scholarly imagination, even though its empirical record is remarkably poor (Alt and Chrystal 1983). Implicitly or explicitly, the standard theory models the electorate as a unitary actor; by construction, it does not allow for a distributional policy bias. My theory suggests that we should be looking for evidence of preelection effects on redistribution rather than the aggregate economy. For example, Keech and Pak (1989) find an electoral cycle in U.S. veterans' benefits between 1961 and 1978. After 1979, when benefits were indexed to fluctuate automatically with the consumer price index, the cycle disappeared.

There exists abundant anecdotal evidence about preelection handouts to special interests. One month before the 1996 presidential election, the Clinton administration pressured Mexico to adopt a voluntary export restraint that would end the shipment of low-price tomatoes to the United States. The deal provided relief to tomato growers in a swing state, Florida, at the expense of tomato consumers all over the United States (Sanger 1996). Clinton followed a path well trodden by his predecessor. Four years earlier, Bush had traveled the land distributing favors to pivotal groups: In South Dakota, he awarded $1 billion in subsidies to boost local grain exports; in Texas, he announced approval of a sale of $4 billion in F-16 fighters to Taiwan, which would guarantee thousands of jobs at the Fort Worth plant of General Dynamics, the aircraft's manufacturer; in Missouri, Bush gave the go-ahead for a long-stalled sale of $5 billion in F-15 aircraft to Saudi Arabia, which would preserve local McDonnell-Douglas jobs (Cornwell 1992). This kind of "surgical" redistribution, which is carefully tailored to a narrow slice of voters at the expense of the American consumer or taxpayer, may well fail to make a statistically significant dent in aggregate measures of protectionism or government spending.

Moving beyond anecdotal evidence, my theory can be tested with a time-series analysis linking the direction and size of redistributional flows between two groups to asymmetries in the relative levels of political information held by the two groups. Consider, for example, the intergenerational distributional conflict

between the minority of retired older people and the majority of working younger people in the United States. The standard tyranny-of-the-majority argument would imply a policy bias favoring the young at the expense of the old. But by some measures the old are doing well compared to the young:

> Since 1973, the real median income of households headed by adults aged sixty-five and over has risen by over 25 percent, while the real median income of households under thirty-five has fallen by over 10 percent. Counting all sources of income, poverty is three times as likely to afflict the very young as the very old.... Per capita federal benefits to the elderly tower twelve to one over benefits to children (Petersen 1996, 142–3).

My theory implies that the federal largesse enjoyed by the old at the expense of the young will vary over time with the degree to which older people are relatively better informed about the activities of elected politicians.

To test this hypothesis, it is necessary to construct empirical measures of the redistributional flows from the young to the old and of the relative levels of political information held by the young and the old. Since these redistributional flows take on the complex form of Social Security and Medicare payments rather than the simple form of direct transfers, their measurement is not straightforward. Standard measures of political information reflect the percentage of respondents who can name their representatives in Congress or accurately position the president and various members of Congress in the "ideological" left-right spectrum. These empirical measures capture my theoretical construct only approximately.

It is also necessary to construct adequate controls for confounding factors so as to disentangle the relative explanatory power of my informational theory and other theories, competing and complementary, that identify economic and political forces shaping the redistributional flows from the young to the old. Economic theory emphasizes as important the relative number of older and younger people, or how many working people support one retiree on average, as well as the output and incentive constraints that define the "possibility frontier" for redistributing income and wealth across these two groups. Political science identifies as important the relative number of older and younger voters, with problems of collective action and the idiosyncracies of political institutions implying different "weights" for younger and older voters. The political power of the old derives from multiple sources. In the modern era, more and more people are living longer, implying that the ratio of older to younger voters is increasing; older people have higher electoral participation rates than do the young; they are better organized, namely, in the American Association of Retired Persons (AARP); they interact more frequently with members of Congress visiting their home district; and they are more likely to reside in a swing state in presidential elections (Florida) (Conway 1991, chapter 2; Roberts 1994, chapter 12).

Older voters are also favored in the game of redistributive politics because they place high weight on "selfish" (pocketbook) issues and low weight on "ideological" (policy) issues and because their policy positions are distributed around the political center. It thus makes sense for office seekers to "buy" the elderly vote by redistributing income to the old, while "paying off" in ideological currency those voters who place high weight on ideological issues and have extremist policy positions (Dixit and Londregan 1996).

I argue that the political power of older people comes about because they are relatively well informed, especially on issues with important consequences for their wealth and well-being.[3] Of two citizens who are otherwise equally well informed about politics and public policy, the citizen over age 64 is more likely to recall news about health insurance than are younger people (Price and Zaller 1993, 156).

Not only do older people have more leisure time to acquire information (that is, for reasons exogenous to my model, they have lower costs of gathering information), but also they receive more information as a result of being better organized. Older people who join the AARP because of the discounts and services it offers receive as a costless by-product of their membership the monthly newsletter, *AARP Bulletin*, and the bimonthly magazine, *Modern Maturity*. These two communications provide information about the activities of elected politicians, the issue positions of competing political candidates, members' legal rights and entitlements vis-à-vis the government, and the like. The AARP also brings together elected politicians and senior citizens and distributes information about these meetings to members.

Selection Bias

Zaller's (1998) Monte Carlo simulation of congressional career paths provides indirect evidence for the view that elections weed out low-quality candidates. Two initial candidates are assigned "skill" and "luck" scores through random draws from a lottery. The candidate with the higher net score of skill and luck wins the contest and takes on new challengers, who are "fresh draws." Successful candidates keep their skill level throughout their career, but there is a fresh draw of luck for each new contest. Zaller analyzes the patterns of wins and losses and compares the resulting "careers" to actual congressional data. The simulated and empirical data fit on several important dimensions: overall reelection rates, sophomore surges, ten-term survival rates, term-by-term increases in performance,

[3] On a tangential note, it is worth mentioning that the political information and participation levels of older people are anomalous. Americans over age 65 are less active in the labor market (they are mostly retired) and less educated than younger generations, and they are disproportionately female (men have a shorter life expectancy). Given the standard empirical correlations between labor market participation, education, and gender, on the one hand, and political information and participation levels, on the other (Conway 1991; Delli Carpini and Keeter 1996; Mayer 1992), we would expect older people to be more ill informed about politics and public policy than they are and to have lower electoral participation rates than they do.

and the relative importance of chance in shaping reelection success.

The selection effects of elections could also be tested by relating the number of times an incumbent is reelected to some measure of competence or quality. For example, we might conjecture that presidents differ in their economic or foreign policy competence. My theory then implies that two-term presidents preside over higher economic growth rates and fewer foreign policy crises, of which a greater number are successfully resolved, compared to one-term presidents. Similarly, we might interpret the quality of a congressional incumbent as standing for skills in pushing legislation through Congress or in "bringing home the pork." My theory then implies that long-term incumbents support or propose more bills, of which a greater number pass, compared to incumbents who lose office soon after their first election. Districts represented by long-term incumbents should enjoy more federal largesse than districts represented by short-term incumbents. To avoid the confounding effects of learning, lame duck status, and seniority, it makes sense to compare the performance of incumbents who subsequently are reelected to the *same-term* performance of incumbents who are not.

Retrospective Voting

The retrospective voting rules in my model are "egocentric," not "sociotropic" (Kramer 1983); that is, voters base their choice on the individual-specific policy outcomes they experience and not on their information about the state of the aggregate economy. My model takes the extreme position that voters only observe individual-specific policy outcomes; they are uninformed about other voters' experiences, and they do not have access to summary statistics reflecting the wealth and well-being of the electorate at large. This extreme assumption is made for the sake of simplicity, not for the sake of empirical realism. If this assumption were relaxed, then the voters' retrospective voting rules would respond to a mix of individual-specific and aggregate concerns.

Empirical tests attempting to discriminate between the egocentric and sociotropic voting hypotheses are beset by the difficulty of measuring how well off an individual voter is along with measuring how she cast her vote. For this reason, much of the evidence relies on survey rather than voting data. For example, Kinder, Adams, and Gronke (1989) employ survey data collected in connection with the 1984 presidential election. They conclude that voters' perceptions of the aggregate economy weighed most heavily, but they also find that individual, group, and national economic well-being constituted related but distinct criteria by which voters assessed the president's performance in office.

A more direct, and persuasive, test of my theory would examine the implication that the vote of special interests responds more sensitively to a given amount of redistribution than does the vote of the electorate at large. The empirical literature on retrospective voting provides a benchmark: An incumbent can expect to gain X votes with a $Y\%$ increase in constituency income and lose X votes with a $Z\%$ decrease in constituency income. My theory suggests that a $Y\%$ increase ($Z\%$ decrease) in farm income that is causally connected to a farm bill rather than general economic circumstances should increase (decrease) the number of votes in agricultural districts by *more* than X.

EXTENSIONS

Interest Group Organization

One function of political lobbies is to collect information about the activities of elected politicians in a centralized way and then disperse this information among their members. The members may also collect information in a decentralized way, in which case the organization serves to pool their information. Either way, organized interests end up with access to better, or less costly, observations of political performance than do unorganized voters.

Why are some interests better organized than others? My theory of endogenous information gathering can be reinterpreted to address this question. Instead of paying a cost to become better informed, individuals might pay a cost to become organized, with the consequence that they will get information about political and economic factors influencing their wealth and well-being. According to my theory, if the number of people on one side of an issue is smaller than the number on the other side, the smaller group will become better organized than the larger group.

In many settings, the explanatory power of my hypothesis is weak compared to that of an alternative hypothesis: selective incentives. To illustrate the explanatory power of the latter, consider the astonishing fact that approximately half of all Americans over age 50, or 33 million people, are members of AARP. There is a simple explanation: For a nominal fee of $8 per year, members acquire access to valuable private discounts and services.

But selective incentives alone cannot explain why some interests get organized and others do not. To the extent that older people are winning the redistributional "game" with the assistance of AARP, we need to ask why there is no comparable organization for younger people. Presumably some political entrepreneur could found the American Association of Persons Age 30–45 with at Least Two Kids. For a charge of $8 a year this entrepreneur could offer attractive discounts negotiated with hotel chains and rental car companies and an array of (money-making) services. Of course, the entrepreneur would pay himself a lucrative salary for his efforts. Persons aged 30–45 with at least two kids would be happy to join because of the selective incentives offered. Once the association has, say, ten million members, it would have the power to lobby Congress for tax exemptions and other handouts to people age 30–45 with at least two children—at the expense of everyone else who does not fit this description.

Of course, there is no end to this logic. Why not have an interest group representing people age 20–35 with at least one child? The problem I identify here is isomorphic to the problem of majority rule cycling. A legislature facing the task of distributing a pie by simple majority rule ends up cycling endlessly: Any proposed distribution that makes a majority better off relative to the previous proposal or the status quo can be beaten by another proposal that makes another majority coalition better off (Condorcet 1785). Theoretical solutions to the problem of majority rule cycling in legislatures build on one of two assumptions: Legislators respect legislative norms (for example, a universalistic norm of reciprocal deference, as in Weingast 1979) or legislative organization (for example, recognition and majority rules, as in Baron and Ferejohn 1991). But there are no analogies to legislative norms and legislative organization in collective action. Thus, selective incentives alone do not tell us why older people are organized, to their advantage, and younger people are not, to their disadvantage.

My informational theory could serve as the steppingstone to a theory of interest group organization: the degree to which people with shared interests are also informationally homogeneous may be one factor determining whether they get organized and pool information.

Special Interest Gridlock as a Multidimensional Collective Dilemma

Lohmann (1996) extends the analysis to more than one policy dimension, constructing an example with three voters and three policy dimensions. Each voter is a well-informed special interest on one dimension, where she benefits from a policy bias in her favor. On the other two dimensions, the voter is part of the ill-informed general public that suffers under the policy bias.

Now suppose that on a given policy dimension the incumbent takes away one dollar from the general public, or 50 cents from each of the two members of the general public, and gives 90 cents to the special interest, with 10 cents representing the deadweight loss of redistribution. The deadweight loss is a decreasing function of the incumbent's competence. Each voter observes, *with error*, that she was taxed a total of one dollar on her two dimensions as a member of the general public, and she observes, again *with error*, that she received 90 cents on her dimension as a special interest. Because the voter is ill informed about her two general public dimensions, she places a high weight on the possibility that the observed loss is due to the error; because the voter is well informed about her special interest dimension, she places a high weight on the possibility that her gains are due to the incumbent's competence. Because the incumbent cannot observe the errors in the voters' signal extraction problems, from his perspective the probability that a given voter supports his candidacy depends probabalistically on the voter's losses and gains on all three dimensions. Because the voter responds less sensitively to a loss on the two general public dimensions and more sensitively to a gain on the special interest dimension, the incumbent can generate a net increase in the probability that he is reelected by redistributing resources from the general public to the special interest—despite the deadweight loss of redistribution. This result holds even though, because of the deadweight loss, each voter would be better off if the incumbent refrained from redistributive activities *on all policy dimensions.*

In a stylized way, this example captures a collective dilemma within which modern democracies are trapped. Inefficient government policies favor special interests at the expense of the general public. While special interests form a minority on any one policy dimension, just about every citizen is a member—active or not—of at least one special interest group on some policy dimension. Each citizen prefers being favored by government policy even at the expense of inefficiencies imposed on the society *at large.* But relative to the status quo involving inefficiencies on all policy dimensions, each citizen would be better off if government did not cater to special interests at all. Voters clamor for government to streamline and reduce the scope of its redistributive activities. They are disgusted with special interest politics and with the limited choice of candidates, all of whom are viewed as beholden to special interests. But any serious attempt to implement the expressed wish of the general public requires cutting someone's "pet" program, and any politician who does so can count on being DOA at the polls with respect to the votes of that group.

Opaque Procedures and Inefficient Policy Instruments

Another extension of my theory allows the incumbent to choose between transparent and opaque procedures or efficient and inefficient policy instruments. By employing opaque procedures, he obfuscates the policy bias toward special interests. For example, "legislators hide their tracks by delegating authority to the executive, by combining all actions into a single omnibus bill, by meeting behind closed doors, or by acting without a recorded vote" (Arnold 1990, 119). In economic policy, opaque instruments are often inefficient. For example, opaque market interventions that increase the relative price of agricultural products so as to improve farm income are inferior to transparent direct income transfers; opaque voluntary export restraints are inferior to transparent tariffs. Reelection incentives thus explain why incumbent policymakers deliberately employ inefficient policy instruments (Arnold 1990; Coate and Morris 1995).

The analysis of Arnold and Coate and Morris complements my analysis. I examine voters' incentives to gather costly information to *reduce* the variance confounding their inferences about the incumbent's type. In my interpretation, Arnold and Coate and Morris examine the incumbent's incentives to *increase* that same variance so as to confound the voters' inferences.

Valence versus Position Issues

My analysis assumes that candidate quality is a "valence issue," that is, candidates are linked with some condition that is valued positively or negatively by the entire electorate (Stokes 1963). In contrast, "position issues" involve government actions over which voters have conflicting preferences. One extension of my theory allows candidates to be purely policy motivated and to differ in their policy preferences. Each voter is incompletely informed about the candidates' policy ideal points. She only observes the policy outcome. Based on this observation, each voter forms an estimate of the distance between the incumbent's policy ideal point and her own. She compares this distance to the expected distance between the challenger's ideal point and her own and then votes for the candidate whose ideal point is expected to be closer. The incumbent thus faces a tradeoff. On the one hand, he can set policy so as to persuade a majority of voters that his interests coincide with theirs, thereby increasing his chances of survival. Because he is policy motivated, the incumbent derives utility from being reelected because he prefers himself to set policy rather than have policy be set by a candidate with different policy preferences. On the other hand, the more the incumbent accommodates any given voter, the larger is the distance between the policy and the incumbent's ideal point, and the lower is the incumbent's first-period utility. The incumbent trades off the second-period selection effects and the first-period policy effects on his utility level. Alesina and Cukierman (1990) develop a model along these lines, albeit without allowing for informational heterogeneity among different groups of voters.

Coups, Riots, Rebellions, and Revolutions

My analysis is institution-free with one exception: I assume that a majority of the electorate can vote the incumbent government out of office in regular free elections. With some modifications, my theory also applies when the opposition seizes power by extralegal means. The logic of retrospective voting that holds in democracies (good economic performance improves the reelection prospects of incumbents) holds more generally for other forms of political participation and for nondemocracies: Good economic performance is negatively correlated with coups, riots, rebellions, and revolutions (cf. Londregan and Poole 1996). My analysis adds informational heterogeneity to the standard logic: Incumbents maintain low food prices, to the advantage of urban interests and at the expense of rural interests, to reduce the probability of urban unrest that would threaten their stay in power (Bates 1981).

Common Agency

The analysis also extends the literature on common agency in political settings (Epstein and O'Halloran 1997) by allowing for differentially well-informed political principals. I develop a principal-agent model with one agent (the incumbent) and multiple principals (the voters). The voters cannot write incentive contracts for the incumbent. Instead, they can vote him out of office if they are dissatisfied with his performance. The incumbent has one task: to supply a differentiated-benefits collective good. Voters differ in the degree to which they can monitor the incumbent's performance, which contains information about his quality. As a consequence, the incumbent has an incentive to bias policy in favor of voters who have better monitors of his performance. The logic of this argument carries over, with appropriate modifications, to other common-agency relationships in political settings—a legislator balancing the interests of voters and organized interest groups; a party leader trading off electoral pressures against the ideological demands of intraparty factions; a regulatory agency responding to pressures from Congress, the executive, and the courts.

PROSPECTS FOR POLITICAL REFORM

My informational hypothesis explains why political competition does not allow voters and political candidates to break free of special interest gridlock. At first glance, a political entrepreneur who campaigns on eliminating all the perks enjoyed by special interests will be elected by a majority. But people cannot effectively monitor whether the candidate keeps his promise. Once elected, the incumbent seeking reelection shares the incentives of his predecessors to cater to special interests.

The policy bias could be reduced if well-informed interest groups or political candidates dispersed information to the general public, but political competition is unlikely to eliminate all information disparities across voters. The interests of information providers do not usually coincide with those of the general public, and free-rider concerns may undermine their costly efforts to educate the public. But even if voters were smothered with "costless" information, it is doubtful that they would pay attention and process detailed information about the complexities of public policy they do not care much about. In contrast, special interests are "naturally" better informed; compared to the general public, they get costless information as a by-product of their specialized activities, and they have stronger incentives to invest in costly information gathering, to pay costly attention to complex information, and to invest in costly expertise that allows them to understand such information.

The question, then, is whether reform efforts are better aimed at modifying the policymaker's response. One necessary condition for the redistributive bias favoring special interests is that the incumbent cares about his reelection prospects. That motive could be removed in one of two ways: lifetime tenancy or term limits. Both solutions are imperfect, for two reasons. First, there is no guarantee that an incumbent who either holds office for life or is subject to a tenure restriction has incentives to maximize aggregate welfare as opposed to favoring special interests. The track record of dictators has not been very promising and

incumbents facing electoral termination may well accommodate special interests who control their future career paths. Second, weakening the electoral connection implies forgoing the selection gains generated by retrospective voting: An incompetent incumbent may remain in office for life, and a competent incumbent may be forced out of office after one term.

To remove electoral influences on public policy, elected politicians may choose to set policy by formula or delegate authority to independent agencies. But rigid formulas require ongoing discretionary adjustments to new and unforeseen circumstances, which opens the door to pork-barrel politics. Similarly, a formally independent agency cannot be perfectly isolated from electoral politics. Threatened with the possibility of "runaway" bureaucracies, or agency drift, politicians are reluctant to give up all political control. They set up ex ante and ex post controls that provide an opening for electoral pressures (McCubbins, Noll, and Weingast 1987). For example, interest groups and citizens disaffected by agency decisions may file complaints with their elected representatives and trigger oversight activities (McCubbins and Schwartz 1984). Here, information asymmetries between political principals and their agents create biases and inefficiencies that must be traded off against the benefits of delegation (Hopenhayn and Lohmann 1996; Lohmann and Hopenhayn 1998).

Proposals for reform are often directed at real or perceived deficiencies of the political process and political institutions. If corrupt campaign finance practices are the root of special interest handouts, then campaign finance reform is the answer. If a dysfunctional Congress is taken to be the driving force for inefficient pork-barrel projects, then the line-item veto giving the president the right to strike such projects out of the government budget offers itself as a solution. While I do not dispute that reform efforts of this kind can have a positive effect at the margin, I contend that the fundamental cause of the policy bias lies elsewhere, namely, with the information asymmetry between the general public and special interests.

Effective political reform would close this information gap by affecting people's incentives to monitor the activities of elected politicians. In a large electorate beset by the free-rider problem, however, the incentives to gather costly information are driven by the competitive race for partisan influence and not by the consensus goal of identifying and reelecting "good" politicians. From a welfare perspective, it may well make sense to accept the existence of a policy bias favoring special interests as the price of a selection process that weeds out bad political candidates and favors the good.

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Domestic Opposition and Signaling in International Crises

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This article explores the effect of domestic political competition on the escalation of international crises. It combines an incomplete information model of crisis bargaining with a simple model of two-party electoral choice. One state has two strategic actors—a government and an opposition party—both of which declare openly whether they support the use of force to alter the status quo. The rival state updates its beliefs and selects its strategy in response to both signals. The parties' payoffs depend upon a retrospective evaluation by the domestic electorate. The model shows that the inclusion of a strategic opposition party decreases the ex ante probability of war by helping to reveal information about the state's preferences. This finding has important implications for research on democracy and international conflict, since it suggests a mechanism through which democratic states can overcome informational asymmetries, which have been identified as a central obstacle to negotiation.

The claim that democracies rarely, if ever, fight wars against one another has generated a great deal of research on how democratic institutions and practices affect the outcomes of international crises. Work generally falls into two categories, one emphasizing the role of democratic norms favoring nonviolent conflict resolution (e.g., Dixon 1994; Doyle 1986; Maoz and Russett 1993; Russett 1993) and the other focusing on the constraining effect of institutions that promote accountability and competition (e.g., Bueno de Mesquita and Lalman 1992; Lake 1992; Morgan and Campbell 1991; Rummel 1979). Though they rely on different causal arguments, both schools maintain that democratic leaders prefer to settle disputes through negotiation and compromise rather than through force.[1]

Despite its emphasis on negotiation, however, much of this research has developed in isolation from the large and growing body of literature on international crisis bargaining (e.g., Bueno de Mesquita, Morrow, and Zorick 1997; Fearon 1992, 1994, 1995; Kilgour and Zagare 1991; Morrow 1989; Powell 1990). This shortcoming is particularly troubling because work in this area has generated two insights that are problematic for the democratic peace literature. First, the costs associated with war mean that all states—regardless of regime type—have incentives to reach efficient bargains that avoid violence. Thus, democratic states are not distinctive in their preference for negotiated outcomes. Second, the mutual desire for a negotiated settlement does not ensure that an efficient bargain will always be reached. If information about the states' expected value for war is distributed asymmetrically, then bargaining can fail. Hence, peaceful relations depend not only on a mutual preference for peace but also on the ability of states to overcome informational asymmetries that can cause war in spite of such preferences.

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The author thanks Bruce Bueno de Mesquita, George Downs, Kurt Taylor Gaubatz, Joanne Gowa, Stephen Krasner, and the anonymous reviewers for their helpful comments.

[1] For recent reviews of these arguments see Chan 1997 and Rousseau et al. 1996.

The primary danger associated with asymmetric information is that actors uncertain about their rivals' preferences will mistakenly take actions that bring about an unwanted war. Under conditions of incomplete information, a state may be unsure whether a challenge will lead to war or to a profitable revision of the status quo, whether rejecting an offer will bring about conflict or a more favorable offer. In such cases, states face a gamble that sometimes favors actions entailing a risk of war. Though war is suboptimal ex post, actions that lead to war can be optimal ex ante.

This situation is compounded by the fact that states engaged in international disputes generally have incentives to misrepresent their willingness to fight through bluff and bluster (Fearon 1992, 1995; Morrow 1986). Absent such incentives, a condition of asymmetric information would be trivial to overcome through simple communication. Yet, the conflict of interests inherent in a crisis situation makes communication difficult. Since each state expects its rival to engage in strategic misrepresentation, distinguishing genuine threats from bluffs is problematic. Under these circumstances, sending credible signals of resolve often requires states to take actions that entail a risk of war (Fearon 1992, 1994; Powell 1990; Schelling 1960). Such actions can separate states that have high value for war from those with low value for war, since the latter should be less willing to approach the "brink." Unfortunately, while risky actions can serve to reveal information, they also may have the effect of making war more likely. The act of signaling can thus lead to the very outcome that the signals were intended to prevent.

This logic suggests that further work must be done on whether and how domestic political institutions can help overcome the problems associated with asymmetric information. This article addresses that question by focusing on one institutional feature generally associated with democracy: regular and public competition between political parties. It combines a standard crisis bargaining game with a simple model of two-party electoral choice. In doing so, it moves away from the unitary state assumption that underlies most deterrence models by breaking down one of the rival states into two strategic actors: a governing party and an opposition party. These parties vie for the support of

the electorate through their public actions in the international crisis—in particular, the government's decision whether to threaten force and the opposition's decision to support or oppose such a threat. Because these actions are observable, they reveal to the rival state information about the government's underlying political incentives and, hence, its willingness to wage war.

The model shows that the probability of war is lower when informative signals can be sent by both parties than when the government is the lone voice of the state, as it is in polities in which competition is poorly developed or actively suppressed. Introduction of an opposition party creates two reinforcing effects. First, the opposition party can lend additional credibility to a government's threats by publicly supporting those threats in a crisis. The decision to support the government reveals that there are political incentives to carry through on a threat to wage war. Hence, the opposition can bolster the government's signal with a "confirmatory signal" of its own. Second, the existence of an opposition party forces the government to be more selective about the threats it makes. When there is weak political support for war, the opposition can reveal this fact by deciding to oppose the government's threat. Because the rival state is more likely to stand firm in the face of an opposed threat, outright bluffing is inherently riskier. As a result, a government with a domestic competitor has less opportunity to misrepresent its preferences, and the dangers associated with asymmetric information are consequently lower.

The model developed here builds on two works dealing with the informational properties of political institutions. Bueno de Mesquita and Lalman (1992, chapter 5) argue that democratic institutions help signal a state's preferences by revealing that the government faces above-average costs for using force. In this view, democracy facilitates the mobilization of opposition, meaning that democratic leaders face generally higher costs in the event that they fight a losing or costly war. The state's preferences are thus revealed not by a conscious act of the government but by the very nature of the institutions, which cannot be concealed.

This argument represents a useful first step, but it stops short of actually modeling domestic politics.[2] Opposition parties are not strategic actors but a passive source of costs. This assumption obscures the fact that opposition politicians can and do make choices about whether to support the government in a threat or use of force. U.S. experience, for example, offers the contrast between the Spanish-American War, in which the opposition party helped push the government toward an intransigent stance, and the crisis leading up to the Gulf War, in which many Democratic representatives publicly opposed the use of force (e.g., Offner 1992; Zaller 1994). Similarly, British history provides a contrast between the Fashoda crisis, in which opposition parties supported the government and helped convince France of the latter's resolve, and the Suez crisis, in which Labour opposition emboldened Egypt to resist British threats (e.g., Epstein 1964, 74–87; Wright 1951). The model presented here thus improves on that of Bueno de Mesquita and Lalman (1992) by letting the opposition party choose a policy position in response to its political incentives.

The second work on which this model builds is by Fearon (1994), who argues that states can send credible signals of their resolve if they face a powerful domestic audience that can punish them for bluffing. In this framework, political leaders expose themselves to "domestic audience costs" when they make public threats in a crisis, and they incur these costs if they back down. The costliness, and hence credibility, of threats depends on the magnitude of the punishment that can be imposed by the domestic audience. If electoral institutions provide an effective mechanism for sanctioning state leaders, then democratic leaders should be able to generate higher audience costs and thus send more credible signals of their resolve.

The model developed here goes farther by introducing a more complete framework of political competition. It shows that a domestic competitor can decrease the dangers inherent in costly signaling. In Fearon's model, exposure to sufficiently high audience costs can "lock" the government into a war that it would rather not fight. Indeed, it is the government's willingness to court this danger that makes its threats credible. An opposition party which can independently confirm the government's resolve makes it possible to reveal information in a less risky manner. Moreover, the introduction of a second signaler can lead to informative behavior even if the government's threats generate no audience costs.

The article proceeds as follows. The first section discusses the main assumptions about political competition that underlie the model. The second section lays out the game, which is then solved in section three. The fourth section discusses the model's implications for the democratic peace and the larger research program on democratic distinctiveness. Some conclusions follow.

BASIC ASSUMPTIONS ABOUT ACTORS AND INSTITUTIONS

Since the purpose of the model is to capture the effects of democratic competition on international bargaining, we first need to consider the basic form this competition takes. The theory developed here rests on three broad assumptions about the actors and the nature of the political institutions.

[2] Furthermore, the underlying argument that democracy makes war more costly for political leaders can be questioned. As Gowa (1995) points out, nondemocratic leaders also face the prospect of removal; even if the probability of their removal is relatively lower, the associated costs can be much higher, that is, death or exile rather than early retirement (see also Goemans 1995). Moreover, democratic leaders may face larger political gains from winning wars, so even if the costs of losing are greater, their expected value for war need not be systematically lower. This implies that war may be riskier for democratic leaders—in the sense that the variance in potential payoffs is higher—but not necessarily costlier ex ante.

Political Parties Seek Office

The first assumption is that political parties value holding office and therefore choose strategies designed to maximize their probability of election. This assumption means that parties are concerned with how voters evaluate their performance in international crises, since this evaluation affects their chances of holding office in the future. In basing the parties' payoffs on the electorate's response, it is not necessary to assume that foreign policy performance is the sole, or even most important, determinant of electoral outcomes. Such an assumption would fly in the face of considerable evidence to the contrary. Instead, it is only necessary to assume that parties' handling of international disputes is one factor that influences electoral fortunes. Unless voters ignore these events entirely, office-seeking behavior should carry over into the realm of crisis decision making.

On this point, there is both systematic and anecdotal evidence suggesting that political outcomes are influenced by international crisis outcomes and that politicians care about how voters assess their handling of these situations. Bueno de Mesquita, Siverson, and Woller (1991) and Bueno de Mesquita and Siverson (1995) show that losing a war or winning a costly war decreases the likelihood that a government will retain office. Cotton (1986) confirms this finding in the case of the United States. With respect to opposition parties, Regens, Gaddie, and Lockerbie (1995) show that members of Congress who voted against U.S. entry into the Mexican-American War and World War I were less likely to be reelected than those who voted in favor of those wars.[3] This finding suggests there are political costs to opposing a war that voters consider successful.

There is less systematic evidence regarding the electoral effect of nonwar crisis outcomes. Nevertheless, several studies of the United States show that voters' evaluations of foreign policy performance influence their overall assessment of candidates (Aldrich, Sullivan, and Borgida 1989; Hurwitz and Peffley 1987; Nincic and Hinckley 1991). Moreover, crisis situations with the potential for use of force have a greater effect on voter perceptions than do other areas of foreign policy, which are generally less visible and salient (Almond 1950, 70–1).

Finally, anecdotal evidence suggests that politicians take into account the expected reaction of voters when deciding to support or oppose the use of force. Zaller (1994), for example, provides an illuminating account of how U.S. politicians weighed the political ramifications of their actions before the Gulf War. This was true not only of President Bush but also of congressional Democrats, who agonized over whether to vote in favor of military action. The perception that this decision would have electoral implications was nicely expressed by one congressional aide, who called the vote "a potential career-killer" (Zaller 1994, 262). Even though we now know that the 1992 elections were more heavily influenced by other factors, politicians concerned about reelection clearly could not and did not ignore the potential voter reaction to their performance.

The assumption that parties seek office generates a conflict of interest between the government and the opposition. If we envision competition as an interaction between two parties or two cohesive coalitions, then whatever increases the probability of election for one will decrease the probability for the other.[4] Hence, government and opposition are locked in a zero-sum game for political office.[5]

This conflict between the parties plays a crucial role in ensuring that their signals are informative. The underlying intuition is straightforward: Two information sources are better than one, especially when they have competing interests (Krehbiel 1991, 84; Milgrom and Roberts 1986). Competition between two signalers constrains the ability of both to misrepresent what they know and increases the credibility of certain signals. While one actor may have incentives to engage in misrepresentation, it is unlikely that two actors with competing interests will collude in such an act. Thus, a given signal will have greater credibility if it is sent by multiple actors with competing interests than if it is sent by one actor with known incentives to lie—a phenomenon known as confirmatory signaling. Moreover, if there are penalties for lying or bluffing, the existence of another actor with incentives to expose a bluff can increase the risks of opportunism.

Opposition Parties Have Access to Relevant Information

The second assumption is that opposition parties have access to information relevant to the crisis—in particular, information about the political ramifications of war. In general, an actor's expected value for war is a function of the probability of different war outcomes

[3] In the case of the Gulf War, Regens, Gaddie, and Lockerbie (1995) find no evidence that Democrats who opposed the use of force against Iraq paid at the polls. They attribute this finding to the length of time between the war and the 1992 election and the influence of intervening factors, such as the economy. Nevertheless, it is clear that Democrats suffered a prolonged period of low poll ratings as a result of their opposition to the war. Moreover, Senator Sam Nunn has said that his vote against the use of force scuttled whatever chances he had for the presidential nomination (see Pace 1996).

[4] Introducing multiple parties would be a natural extension of this model. In principle, the conflict of interest is less stark in such a setting, since two parties could attempt to collude to the detriment of a third. This modification would only affect the logic presented here if there were opportunities for collusion between a party in the governing coalition and a party in opposition. Collusion within government or opposition coalitions is essentially assumed already by treating them as unitary actors.

[5] It should be recognized that, by basing the payoffs entirely on the probability of election, the model may overstate the conflict of interest between opposing parties. From the perspective of its electoral chances, the opposition would like nothing better than for the government to engage in a disastrous and costly war. There is a point at which unfavorable international outcomes may be undesirable even to the opposition, however. For example, a war that results in destruction of the state or its annexation by foreign powers would deprive opposition politicians of their chance to hold office—and possibly of their lives. At that extreme, then, the relationship between the international outcome and the opposition's payoff gets more complicated.

and the relative values of winning and losing. Assume that states fight a winner-take-all war over some good, whose value we normalize to one, and that all sides incur costs of fighting a war. If state i's probability of victory is p_i, and its expected costs of war are c_i, then its expected value for war is given by

$$w_i = p_i - c_i. \quad (1)$$

Notice that, in this formulation, the c_i term captures the costs of war *relative to* the value of the disputed good.

Though any or all factors in this expression may be sources of uncertainty, the model assumes that actors hold private information about the costliness of war relative to the value of the disputed good—a factor often termed "resolve."[6] Whereas p_i depends heavily on the balance of military capabilities, resolve rests in large part on the configuration of domestic political interests. How much is the good in question worth? What costs are we willing to pay? What risks are we willing to take? From the perspective of foreign states, the answers to such questions are harder to observe than, say, the number of tanks and airplanes each side possesses.

These are also questions for which the opposition party is likely to share the government's informational advantage over the rival state. Opposition parties have relevant information due to previous experience in office and access to legislative institutions. Moreover, parties out of power are still in a position to make inferences about the configuration of domestic interests. Political parties exist to aggregate information about political preferences, to anticipate the likely electoral consequences of their actions, and to choose their public strategies appropriately.[7] If they do not, then they will not last long. We thus assume that opposition parties take advantage of their resources to gauge the expected political ramifications of war, and especially how the likely costs compare with the stakes.

Competition Is Public and Unrestricted

It is crucial for this analysis that parties compete in public. Open political debate means that foreign states can "overhear" the policy statements used to build electoral support. These statements reveal information about the parties' underlying political incentives. For this communication to be meaningful, of course, there must be no legal sanctions against public dissent during international crises. Opposition parties must be able to choose their strategies based on political considerations, rather than on any normative or legal constraints. After all, support for a government's policies conveys no meaning if that support is coerced. Competition should be based on the concept of "loyal opposition," meaning that dissent over government policies does not imply disloyalty to the state.

Together, these assumptions imply a polity that has many attributes of democracy, but they do not require strict adherence to some ideal definition of democracy. Though competition is a sine qua non of democratic government, many nondemocratic polities also experience forms of competition. What is crucial for this analysis is that competition takes place in public, so that the positions of contending parties are observable to foreign states; that it is unfettered by restrictions on dissent; and that parties out of power are not so excluded from the political process that they have no access to relevant information. In terms of Dahl's (1971) classic definition of polyarchy, this model places primary importance on "public contestation" but imposes no requirements on the level of participation or inclusiveness.

A BARGAINING MODEL WITH A STRATEGIC OPPOSITION

The model considered here describes an international interaction between two states, one of which permits public competition of the kind posited above. This competitive polity is composed of two strategic actors—a governing party and an opposition party—and a nonstrategic actor—the domestic electorate. The parties take actions that both the electorate and the rival state can observe. These strategies, and the corresponding response of the rival state, determine the outcome of the international game. Since the parties are assumed to be office-seeking, their payoffs depend upon how the voters evaluate their performance in the crisis. This evaluation is based on the public positions taken by the parties in the crisis and the realized outcome.[8] This section provides a formal description of the game and the voters' evaluation of the parties.

The International Crisis

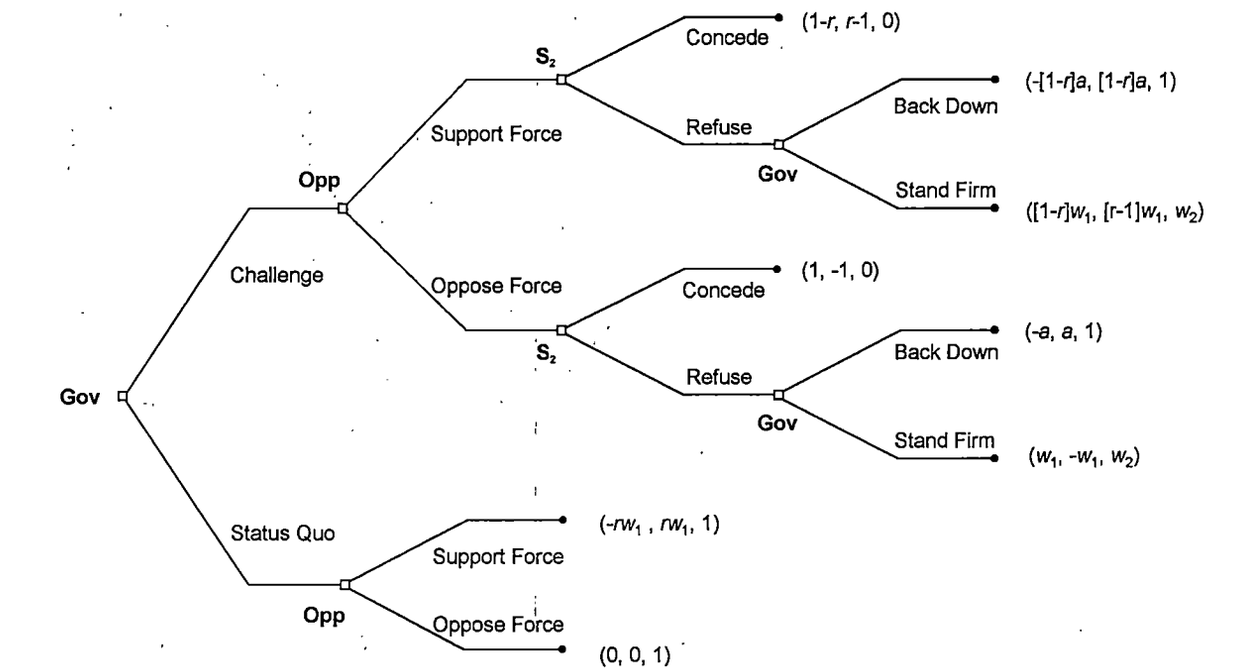
The crisis game presented here is based on deterrence and crisis bargaining models (e.g., Bueno de Mesquita, Morrow, and Zorick 1997; Fearon 1992, 1994; Kilgour and Zagare 1991; Morrow 1989; Powell 1990). Two states compete over the possession of some good. The exact nature of the good is not important and can be thought of as anything states may value: territory, a source of wealth, a policy, an institution, and so on. A crisis occurs when one state challenges the other for possession of the good. Each then has an opportunity

[6] Fearon (1992) interprets the c_i term in equation 1 as the state's resolve.

[7] Indeed, this feature is what makes the action of political parties particularly informative. Domestic political preferences in a democracy can be expressed in any number of ways by many actors. Opposition can take the form of newspaper editorials, protests, demonstrations, strikes, and so forth. From the perspective of foreign states, the plethora of political activities observable in a democracy may seem confusing, but the existence of parties whose job it is to aggregate all this information provides foreign decision makers with a way to differentiate politically relevant signals from noise. The former should be reflected in the strategies of office-seeking parties; the latter should not.

[8] In its basic structure, the model presented here is similar to that of Gaubatz (1998). In his model, government and opposition parties choose foreign policy positions in response to both international conditions and electoral incentives. In Gaubatz's model, however, there is complete information about the preferences of the domestic actors, so their policy positions do not serve as signals.

FIGURE 1. Crisis Bargaining Game with a Strategic Opposition



Note: Gov = Government, Opp = Opposition Party, S_2 = Rival State.

to stand firm or back down, thus conceding the good to the other side. When both states choose to stand firm, the result is war.

The main innovation here is that one state is decomposed into two strategic actors: the government and the opposition. Though the state itself is not an actor, it will be convenient at times to refer to this two-actor entity as S_1. The government is assumed to have full control over the foreign policy decisions of S_1. Thus, the decision to make a challenge and the decision to go to war are both made by the party in power. The opposition party cannot veto the choices of the government, but it can make a public declaration of its policy position—in particular, whether it supports the use or threat of force in order to change the status quo.[9] The rival state chooses its strategy after viewing the actions of the government and the policy statement of the opposition.

Sequence of Moves. The game begins with the government's decision either to maintain the status quo (SQ) or to issue a public challenge (CH). We assume that any challenge includes an explicit threat to use force in the event that the demands are not met. Following the government's move, the opposition party chooses its policy stance. It selects either to support (SUP) or oppose (OPP) the use of force. Notice that SUP and OPP represent the opposition's stance regarding the use or threat of force to alter the status quo, not its position relative to the government's policy. Thus, if the government chooses SQ, an opposition strategy of SUP does not imply support for the government but for the use of force.

If the government chooses to accept the status quo, then the game ends after the opposition's move. In the event of a challenge, and after the opposition makes its policy declaration, the rival state, S_2, decides either to concede the good (CD) or to refuse the challenge (RF). If the former, then the game ends peacefully, with S_1 getting the entire good. If the latter, then the government faces a choice between backing down (BD) and standing firm (SF). The former implies that S_2 gets the good and the game ends peacefully, while the latter leads to war. Figure 1 presents the extensive form of this game.

Payoffs. There are four possible outcomes at the international level: status quo, S_2 concedes, S_1 backs down, and war. Without loss of generality, assume that the value of the good is one and that S_2 possesses the good in the status quo; concession by S_2 implies that the entire good is transferred to S_1. In the event of a war, let p_i and c_i represent state i's probability of victory and expected costs, respectively.[10] Given these assumptions, the international outcomes can take on a value of zero (if the state ends up without the good), one (if the state gets or retains the good), or w_i, as defined in equation 1 (in the event of war). Since the government and opposition are assumed to be office-seeking, they do not value the international outcomes directly; instead, their payoffs are determined by the

[9] In a presidential system with divided government, the opposition party may have some veto power over the government's decisions. Since this situation is not the norm among democratic states, I choose not to examine it here.

[10] Obviously, since there are only two players and no draws, $p_1 = 1 - p_2$. Nevertheless, it will simplify the notation if these two terms are kept distinct.

electorate's evaluation of their performance, which will be described below. S_2 is treated as a unitary state, so its payoffs depend solely on the international outcome. Thus, S_2 gets a payoff of one if the government chooses SQ or backs down from its challenge, zero if S_2 concedes in response to a challenge, and $w_2 = p_2 - c_2$ if the game ends in war.

Information Structure and Beliefs. The game involves two-sided incomplete information: Each state is uncertain about the other's expected costs from war. To generate this condition, assume that nature randomly selects c_1 and c_2 from independent distributions over the ranges $[0, C_1]$ and $[0, C_2]$, respectively. Both the government and the opposition in S_1 observe the selection of c_1, and S_2 observes the selection of c_2. The distributions from which these terms are drawn are common knowledge. As it will simplify notation to express things in terms of w_i, these assumptions imply that the w_i are distributed over the range $[p_i - C_i, p_i]$. Let F_1 and F_2 represent the cumulative distribution functions of w_1 and w_2, respectively.

Notice that, with these assumptions, it is possible for both w_i to be greater than zero simultaneously. When this is the case, there exists no peaceful solution that both sides prefer to war, since the state which backs down gets no more than zero. This possibility arises because the good is treated as indivisible: One side gets all; the other, none. In bargaining models with a continuously divisible good, both states can always be satisfied by some peaceful division even if their payoffs from war are both greater than zero (Fearon 1995). Nevertheless, treating the good as indivisible makes the game more tractable and does not affect the main results, with one exception. In the equilibria discussed below, the probability that $w_2 > 0$ is assumed to be relatively low. If this probability exceeds a certain threshold, then S_2 is highly likely to refuse a challenge regardless of what the government and opposition do. As a result, the government only makes challenges that it is serious about carrying out, and the opposition's strategy adds no additional information. Since this result is largely an artifact of the indivisibility of the good, its treatment is left to the Appendix.

The Electorate's Evaluation

Since the parties in this model are assumed to be office-seeking, their payoffs from the crisis game depend upon the ex post response of the electorate in S_1. A complete model of how voters use public statements and observable outcomes to weigh the relative merits of opposing candidates is beyond the scope of this work. Instead, we rely on a simple set of assumptions that seem reasonable given the empirical patterns described above. The first is that voters prefer governments which deliver favorable outcomes to those which deliver unfavorable outcomes. Hence, the government's payoff should improve as the outcome it achieves in the international crisis improves. With respect to the opposition, it makes sense to assume that voters reward opposition parties which either support successful policies or oppose unsuccessful policies, and they punish parties which oppose successful policies.

The result is a simple retrospective voting framework, which can generate payoffs in a straightforward manner.[11] Assume a homogeneous electorate whose utility from the international game increases with the amount of the disputed good received by S_1 and whose expected payoff from war is given by w_1. The assumption of a homogeneous electorate implies that the disputed good is a public good with equal value to all voters and that the costs of war are distributed evenly. Obviously, this need not always be the case, and a possible extension of this work would be to explore the implications of a heterogeneous electorate. Since the electorate is not a strategic actor, it is not necessary to assume that it knows S_1's expected value for war. For reasons that will be clear shortly, however, we assume the electorate has unbiased expectations: Its beliefs about war are systematically neither too high nor too low. Formally, if we let w_v denote the voters' expectations about war, we require that $E(w_v) = w_1$.

Now consider the four possible outcomes of the international game. When the government makes a challenge and the rival state backs down, the government receives a favorable evaluation from the voters for having acquired the good without a fight. If V_{gov} denotes how favorably the voters respond to the government, then we assume that $V_{gov} = 1$ in this case. The corresponding evaluation of the opposition, V_{opp}, depends upon that party's announced position. If the opposition supported the successful threat, then it can plausibly claim that it, too, would have generated the same outcome and should share some of the credit. Since the opposition did not actually manage the crisis, however, and we know that the government gets most of the credit or blame for foreign policy outcomes, it is sensible to assume that voters discount the credit they give to the opposition for supporting a successful outcome. Following Fiorina (1981, 73), we let the parameter r denote how much the voters discount the policy statements of the opposition due to the fact that it was out of power, with $0 \leq r \leq 1$. Thus, if the government gets a credit of 1 when the rival state concedes, then an opposition party that supported the government gets a credit of r.

If the opposition party opposed the government's successful threat, then it should incur political costs. A natural way to generate such costs is to assume that voters make the following inference from the opposition's stand: If the opposition was against the use of force, then its announced strategy would have generated the status quo as the outcome. Since voters get a payoff of zero from the status quo, they assign a credit of zero to the opposition in this case. The result is as desired: V_{opp} is higher when the opposition supports a successful policy (r) than when it opposes that policy (0).

Similar logic generates voter responses in the event of a war. The electorate's ultimate evaluation of the

[11] Hurwitz and Peffley (1987, 238–9) argue that retrospective voting models seem particularly appropriate in the area of foreign policy.

TABLE 1. Party Payoffs

Outcome	Opposition's Stance	V_{gov}	V_{opp}	$U_{gov} = V_{gov} - V_{opp}$	$U_{opp} = V_{opp} - V_{gov}$
Status quo	Support force	0	rw_1	$-rw_1$	rw_1
	Oppose force	0	0	0	0
Government backs down	Support force	$-a$	$-ra$	$-(1-r)a$	$(1-r)a$
	Oppose force	$-a$	0	$-a$	a
Rival state concedes	Support force	1	r	$1-r$	$r-1$
	Oppose force	1	0	1	-1
War	Support force	w_1	rw_1	$(1-r)w_1$	$-(1-r)w_1$
	Oppose force	w_1	0	w_1	$-w_1$

government depends on the outcome of the war. While the government does not know ahead of time precisely what this outcome will be, in expectation, $V_{gov} = w_1$. If the opposition supports the threat that led to war, then it shares some credit or blame for the outcome; the expected evaluation of the electorate is thus w_1 discounted by r, or $V_{opp} = rw_1$. If the opposition opposed the threat, then the voters again assume that the opposition would have delivered the status quo, or $V_{opp} = 0$. These assumptions imply that, when the war outcome is favorable (i.e., $w_1 > 0$), the opposition is better off having supported it than having opposed it.

In the event that the government backs down after making the challenge, the coding has some ambiguity. Following Fearon (1992, 1994), we assume that the government incurs audience costs for failing to carry through on a threat. The evaluation of the government thus reflects the electorate's unfavorable response to this outcome; in this case, we let $V_{gov} = -a$, with $a \geq 0$.[12] As before, it makes sense for voters to ascribe the status quo payoff to the opposition party in the event that it opposed the threat, or $V_{opp} = 0$. What is unclear is how the voters should react when the opposition supports a threat that the government fails to carry out. One possibility is that the voters punish the opposition just as they punish the government; in this case, V_{opp} would equal $-ra$. An alternative is to assume that the voters ascribe to the opposition their expected value for war, w_v, which, in expectation (and minus discounting), equals rw_1. This assumption implies that, if voters are sufficiently optimistic about their chances in war, the opposition derives some benefit from having supported the use of force when the government backs down.[13] As it turns out, the solution does not depend at all on which assumption we make. To keep the payoffs parallel, then, we will let $V_{opp} = -ra$ when the opposition supports a threat from which the government backs down.

The final possible outcome is the status quo. As before, the voters' evaluation of the government is tied to the value of the actual outcome, meaning $V_{gov} = 0$. In the event that the opposition opposes the use of force, we assume that V_{opp} likewise equals zero. There is some ambiguity as to how the voters should respond in the event that the opposition calls for the use of force after the government has selected the status quo. We observe empirically that opposition politicians may criticize governments which fail to take forceful action in a crisis. Bill Clinton's criticism of President Bush during the 1992 campaign over nonintervention in Bosnia is an obvious recent example (Gow 1997, 207–8). Given that the government has decided not to use force and the opposition has no control over foreign policy, such statements are rather cheap to make. Consequently, it is not clear why the voters should attribute much meaning to them. If the voters simply read such criticism as cheap talk, then the opposition should be indifferent between its strategy choices at this node, and any observed mix of opposition or support is understandable but ultimately has no effect on the game. An alternative possibility is that when the opposition supports force, the voters ascribe to it their expected payoff from war (minus discounting), which in expectation equals rw_1. This means that, if the electorate believes it can do well in war (i.e., $w_1 > 0$), the opposition can score political points by criticizing the government's inaction. As it turns out, we will not observe this outcome in equilibrium since, whenever w_1 is sufficiently high that choosing the status quo would generate opposition criticism, the government would want to make the challenge anyway. This issue is addressed at greater length below.

We thus have a plausible set of assumptions for how the voters evaluate the two parties based on their performance in the crisis game. Since the parties are assumed to be office-seeking, their primary concern is how these evaluations will ultimately affect their probability of election at some future date. That electoral choice depends, in turn, not on the absolute evaluations, V_{gov} and V_{opp}, but on the relative assessment, or what Downs (1957, 40) refers to as the "party differential." Thus, to complete the payoffs, we assume that the government seeks to maximize $U_{gov} = V_{gov} - V_{opp}$, while the opposition seeks to maximize $U_{opp} = V_{opp} - V_{gov}$. Table 1 summarizes these payoffs as a function of the different possible outcomes. The payoffs corresponding to each terminal node are also shown in Figure 1.

[12] We assume that the audience costs are small relative to the maximum possible costs from war, so that $p_1 - C_1 < -a \leq 0$.

[13] In the Fashoda crisis, for example, the opposition Liberal Party came out strongly in support of the Conservative government's intransigent stance against France. Liberal leader Lord Rosebery made it clear that he would try to capitalize politically should the government soften its stance: "No Government that attempted to recede from or palter with [the current] policy would last a week" (Wright 1951, 41).

Clearly, the voters in this model are not fully rational; at best, they are boundedly so. Fully rational voters use retrospective evaluations as guide to future payoffs, and so they only pay attention to past outcomes if these reflect on the parties' expected future performance. For example, in Smith's (1996) work on the diversionary use of force, voters attempt to discern the government's foreign policy "competence" from past international outcomes in order to gauge the government's likely performance in the next term. In the model presented here, the electorate's war payoff, w_1, is not a function of the governing party's characteristics but of the state's capabilities and the costs and benefits of war to the voters. Consequently, it is not fully rational to evaluate the parties based on the outcome of the crisis game, since the government and opposition would have acted the same way if their roles were reversed. Technically, nothing can be learned of the parties' relative "fitness" for office.

Nevertheless, the simple framework employed here represents a useful simplification and is consistent with other recent models that involve retrospective voting on international crisis outcomes (i.e., Bueno de Mesquita and Siverson 1995; Fearon 1994; Gaubatz 1998). Introducing a fully rational electorate would require a fourth strategic actor and would greatly complicate the information structure of the game. The simple voting scheme employed here is a compromise for the sake of tractability. Moreover, the payoff orderings generated fit the empirical findings cited above. Thus, the payoffs describe a plausible rule-of-thumb decision criterion that meshes with the empirical regularities we have.

SOLUTION TO THE BARGAINING MODEL

This section presents the solution and main results of the bargaining model. Since the goal of this article is to explore how the introduction of a strategic opposition affects the dynamics of crisis bargaining, the central focus is on comparing the equilibria of the game proposed above with those of a game without an opposition party. We can thereby see how behavior and outcomes change when S_1 shifts from a polity in which competition is suppressed to one in which opposition parties openly compete for political support. In practice, capturing the former does not require solving an entirely new model. Setting the parameter r equal to zero while keeping all other assumptions as given generates a game in which the opposition party effectively does not exist.[14] Thus, the core result that interests us is the effect on the game when r shifts from zero to some number greater than zero. The formal solution to the model is presented in the Appendix. Here we present the main results.

Characterization of Equilibrium Strategies

The equilibrium strategies of the government and the opposition are described by a set of cutpoints along the continuum of possible types. The government's strategy is defined by two such cutpoints, which partition the possible types into three ranges. Let k_{gov} and b denote values in the range $[p_1 - C_1, p_1]$, with $k_{gov} \geq b$. A government of type w_1 uses the following decision rule.

(1) If w_1 exceeds k_{gov}, then the government makes the challenge and stands firm at the final node. Since types in this range will carry through on the threat to use force, their challenges can be said to be genuine. It is shown in the Appendix that this cutpoint always equals $-a$, meaning that the government makes genuine challenges whenever the payoff from war is preferable to incurring the audience costs.

(2) If w_1 falls between k_{gov} and b, the government makes the challenge but backs down in the face of a refusal. Since these types will not enforce the threat, their challenges are bluffs.

(3) If w_1 is lower than b, the government selects the status quo. Though types in this range never have to make a choice between standing firm and backing down, their off-the-equilibrium-path strategy is to back down.

When the game includes an opposition party, there is an additional cutpoint describing its strategy. Let k_{opp} denote a value in the range $[p_1 - C_1, p_1]$, such that an opposition of type w_1 uses the following decision rule in response to a government challenge:

(1) If w_1 exceeds k_{opp}, the opposition supports a threat to use force, and

(2) if w_1 is less than k_{opp}, the opposition opposes a threat to use force.

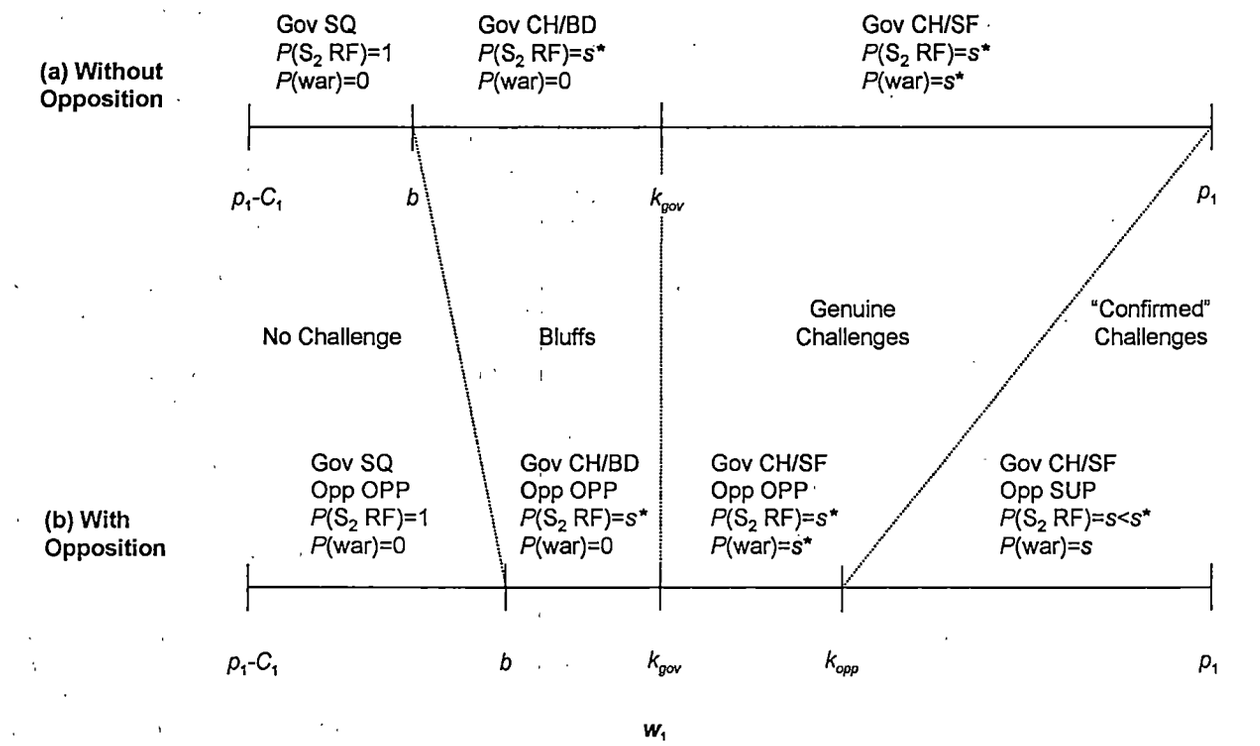
It is shown in the Appendix that $k_{opp} \geq k_{gov}$, implying that the opposition supports only genuine challenges but not all such challenges. When the government chooses the status quo, the opposition supports force if and only if $w_1 \geq 0$. Since the government always makes a challenge in these cases, however, we never see Gov SQ/Opp SUP in equilibrium.

As can be seen in Figure 2, these cutpoints divide the continuum of possible types into three or four regions, depending on whether the game includes an opposition party. When there is none (2a), the government's two cutpoints partition the continuum into three ranges: Types which make genuine challenges, types which bluff, and types which select the status quo. The inclusion of an opposition party (2b) introduces an additional cutpoint, which partitions genuine challenges into those supported by the opposition and those opposed. For reasons that will be clear shortly, we can refer to the former as "confirmed" challenges.

The intuition behind these strategies is straightfor-

[14] Alternatively, we could assume that an opposition party or faction exists but that it cannot publicly dissent from the government. In this interpretation, the opposition can oppose or support the challenge, but it does so privately, so that the rival state cannot observe its position. Formally, this can be captured by assuming that S_2's two decision nodes comprise a single information set, meaning that S_2 does not know which strategy the opposition selected. This interpretation would generate the same predictions as assuming $r = 0$, so the discussion will concentrate on the latter interpretation, since it is simply a special case of the general solution. Moreover, while there is some appeal to assuming that nondemocratic states experience private competition, it is not clear that the voting rule, derived from observations of democratic electorates, is appropriate in this context.

FIGURE 2. The Effect of the Opposition Party on Strategies and Outcomes



Note: BD = Back Down, CH = Challenge, Gov = Government, OPP = Oppose Force, Opp = Opposition Party, RF = Refuse, S_2 = Rival State, SF = Stand Firm, SQ = Status Quo, SUP = Support Force.

ward. As S_1's expected value for war increases, the government's expected payoff from making a challenge also increases. Thus, higher types are more likely to threaten force than lower types. Notice, though, that the government may sometimes bluff in equilibrium. This is a natural result of asymmetric information: There are often incentives for actors to misrepresent their types in hopes of getting a higher payoff. Only when the government's expected payoff from war is very low does it choose to forswear a challenge altogether.

Once the government has made a challenge, the choice of the opposition boils down to a decision either to "match" the government by supporting the threat—thereby sharing credit or blame for the eventual outcome—or to oppose the threat and advocate the status quo. Clearly, as the expected payoff from war goes down, the attractiveness of the former decreases; the opposition prefers to distance itself from a threat that could result in either an unpromising war or a humiliating retreat. As the payoff from war increases, the opposition would rather support the threat and get some credit for the (expected) favorable outcome.

The fact that the opposition's cutpoint, k_{opp}, is higher than the government's cutpoint, k_{gov}, implies that the opposition will sometimes oppose a threat to use force even though the government is willing to make and carry out such a threat. This does not reflect greater dovishness on the part of the opposition but a reluctance to match the government unless the credit from doing so is sufficiently high. We will see shortly that, by supporting the challenge, the opposition can increase the probability that S_2 will concede. As long as $r < 1$, however, the government gets most of the credit for such a success. Thus, the opposition has some incentive to pursue an "irresponsible" strategy: Oppose the threat and gamble that the international outcome will be unfavorable (Gaubatz 1998). This incentive goes away when the state's expected payoff from war is relatively high; in this case, all possible outcomes from the government's challenge are sufficiently attractive to the electorate that the opposition prefers to hop on board.

S_2's equilibrium strategy is a function of its beliefs, which in turn depend upon the signals sent by the government and opposition. Since S_2 only gets to move if the government makes a challenge, two possible signals are of interest: Gov CH/Opp SUP and Gov CH/Opp OPP.[15] From S_2's perspective, the important question upon viewing these signals is: What is the probability that the government will stand firm in response to a refusal? In other words, what is the probability that the challenge was made by a government of type $w_1 \geq k_{gov}$?

As noted above, the opposition only supports genuine challenges. Thus, the support of the opposition unambiguously signals that the government will fight in the event of a refusal. Given this, S_2 knows that it faces a choice between conceding the good or waging war;

[15] It is trivial to show that S_2 plays RF off the equilibrium path if the government plays SQ.

there is no chance that the government will back down. Since conceding leads to a payoff of zero, S_2 refuses only if the costs of fighting are so low that its expected payoff from war, w_2, is greater than zero. Let s denote the probability that this condition holds.

If the opposition opposes the challenge, then S_2 is left uncertain about whether the threat is genuine or a bluff. Recall from Figure 2b that both kinds of threats may be opposed by the opposition. If $k_{opp} > w_1 \geq k_{gov}$, then an opposed threat is still genuine: Governments in this range will stand firm in response to a refusal. Governments for which $k_{gov} > w_1 \geq b$ also make opposed threats, however, and these are bluffs. As a result, S_2 faces some ambiguity regarding the government's true type. The rival state can always concede the good and ensure a payoff of zero. Refusing the challenge is a gamble: There is some chance that the government is bluffing and will back down, but there is also some chance that the threat is genuine and a refusal will lead to war. Because war is only a possibility and not a certainty, even an S_2 that prefers concession to war—that is, for which $w_2 < 0$—may find it optimal to take the gamble and refuse the challenge. The uncertainty generated by the opposition party's strategy thus means that more types of S_2 are willing to refuse the challenge than are willing to do so when the opposition supports the government. The probability with which S_2 refuses when the opposition opposes the threat is consequently greater than the probability with which S_2 refuses when the opposition supports the threat. If we let s^* denote the former, then this intuition implies that $s^* > s$.

The Effect of the Opposition Party

We can now address the central question of this article: How do behavior and outcomes change as a result of the public competition in S_1? What happens to the equilibria when we go from a game without an opposition party to one in which an opposition makes informed public statements? As noted above, we can answer this question by determining how strategies and outcomes change when we move from $r = 0$ to the general case in which r is some value greater than zero. Theoretically, of course, the model can make precise predictions across the entire range of r. Yet, because of how r is defined (the degree to which voters discount policy statements from the party out of power), it is unclear how small variations in this parameter map into real-world variation among political systems. The empirically relevant question is the difference between polities that do and do not suppress competition, a difference which can be captured by comparing the $r = 0$ case with the $r > 0$ case.[16]

The shift from a game without an opposition to one with an opposition entails two important changes. First, the opposition party can increase the credibility of some challenges by publicly supporting the use of force. When S_1's expected value for war is sufficiently high (i.e., $w_1 > k_{opp}$), a strategic opposition has political incentives to back the government's threat to use force. In doing so, the opposition essentially confirms that the government is serious about carrying through on the challenge. The effect of this signal derives from the fact that the opposition is engaged in a competition with the government and supports it only if the expected value for war is sufficiently high. *Thus, while the government may have incentives to bluff, the opposition has no incentive to collude in a bluff.* The rival state interprets a supported threat as a credible threat and will consequently back down, unless it has positive expected utility from war.

Because of this effect, the existence of an opposition party permits information to be revealed more reliably than when the government is the lone voice of the state. Interestingly, the opposition can play this role even when the government's challenge generates no audience costs. When $a = 0$, the government can costlessly make challenges regardless of its type, since the worst possible outcome, backing down and getting zero, is the same as choosing the status quo. Because of this, the government's strategy conveys no information. Yet, the Appendix contains a proof showing that the opposition party still sends informative signals in this case, as its decision to support or oppose still partially separates genuine threats from bluffs. This suggests that the existence of a second information source can compensate for the government's inability to send costly signals.

The second change when a strategic opposition is introduced is a decrease in the government's willingness to bluff. Notice from Figure 2 that cutpoint b shifts to the right when the opposition party is added to the interaction, meaning that the range of types which bluff in equilibrium shrinks. The intuition underlying this result is straightforward. Just as the opposition's support increases the credibility of a threat, the opposition's dissent casts doubt on whether the challenge is genuine. If the government were to leave its strategy unchanged, then the rival state would want to refuse opposed challenges more often. In equilibrium, however, the government compensates by bluffing at a lower rate; formally, b increases. As a result, domestic opposition does not increase the probability of a refusal relative to the case in which no opposition party exists. Instead, the adjustment is made by the government, which becomes more selective about making threats and less inclined to bluff.

What does all this imply for the probability of war? There are three ways to think about this question: the probability of war for different types of S_1; the probability of war ex ante, before the game even starts; and the probability of war given that a challenge has been issued. War occurs in the model whenever the government makes a genuine challenge—that is, a threat it is willing to carry out—and the rival state refuses. Hence, there is only a nonzero probability of war when $w_1 >$

[16] Technically, the changes discussed below do not automatically operate once r moves from zero to any positive number. There may be small but positive values of r for which the game is identical to the $r = 0$ case. In particular, the introduction of the opposition has no effect on the game until r is sufficiently high that $k_{opp} < p_1$. The exact threshold at which this happens can be determined using the specification of k_{opp} given in the Appendix.

k_{gov}, because any threats made by types falling below this cutpoint would not be carried out. Given that a genuine challenge has been made, the probability of war is the same as the probability that S_2 refuses. Using the notation introduced above, this probability is s when the opposition supports the threat and s^* when the opposition opposes the threat or when there is no opposition party. Because the opposition's support confirms the credibility of the threat, s is less than s^*. Thus, for types in the range $[k_{opp}, p_1]$, the probability of war is lower in the game with a publicly active opposition than in the game without such an opposition.

This result implies that the introduction of an opposition party decreases the ex ante probability of war, that is, the probability of war prior to nature's selection of w_1 and w_2. Because the danger of war is reduced in the highest range of w_1, the expected probability of war across the entire range also falls. This result is driven by the fact that the probability of a refusal by the rival state is the same (s^*) when an opposition party opposes the government's threat and when there is no opposition party. Thus, while types in the range $[k_{opp}, p_1]$ see their probability of war decline with the introduction of competition, those in the range $[k_{gov}, k_{opp}]$ have the same probability of war in both versions of the game.[17]

How does public competition affect the conditional probability of war given that a challenge has been made? On this point, the results are ambiguous because of two countervailing effects. Once a challenge is issued, a peaceful outcome can come about in one of two ways: Either S_2 concedes the good, or the government backs down from the challenge. On the one hand, adding an opposition party to the game increases the probability of the former, on average, since the rival state is less likely to refuse challenges publicly supported by the opposition. On the other hand, introducing competition also makes the government less likely to bluff. Given that a threat has been made, the probability that the government will back down in response to a refusal is lower in the game with an opposition than in the game without an opposition. Thus, governments that face domestic competition are less likely to have their challenges refused but more likely to stand firm in the event of a refusal; governments unconstrained by public competition are more likely to have their challenges refused, but they are also more likely to have been bluffing in the first place. Which effect dominates is unclear and depends upon the actual distribution of types.

All these comparative statics make use of the finding that the probability of S_2 refusing a challenge is the same when an opposition party publicly opposes the government's threat and when there is no opposition party. One potential modification to the game would alter this result. As noted earlier, the outcome in which the government selects the status quo and the opposition scores political points by advocating the use of force never happens in equilibrium. The reason is that the electorate's views about war are always correct in expectation. Hence, whenever w_1 is such that the opposition could profitably support the use of force, the government will always make the challenge. Likewise, whenever the government chooses the status quo, the voters' expectations about war are sufficiently pessimistic that they would punish, not reward, the opposition should it advocate force. If we were to assume, however, that the opposition could systematically gain by supporting force after the government has selected the status quo, then the effect on the game would be to increase the probability that S_2 resists after seeing an opposed challenge. The reason is that, if the status quo becomes less desirable to the government because of the opposition's criticism, then equilibrium can only be restored if the rival state refuses challenges at a higher rate, thus making challenges less desirable as well. In this case, the probability of war in the range $[k_{gov}, k_{opp}]$ would be higher when the game includes an opposition party than when it does not. The overall effect on the ex ante probability of war would be ambiguous, since the introduction of the opposition would lower the probability of war in one range and raise it in another.

This caveat is worth noting, but it begs the question of why the electorate would systematically reward the opposition in such a situation. This is not to deny that we do observe cases in which the opposition scores politically by criticizing a government's inaction. Yet, my sense is that this phenomenon would be better captured by moving to a model with heterogeneous voters—in which the government and opposition could play to different segments of the electorate—than by making ad hoc amendments to the current model.

One final comment should be made. In principle, nothing in the underlying logic requires S_1 to be the first mover. We could imagine inserting an additional step at the beginning of the game and permitting S_2 to decide whether to make a challenge. This game is more complicated because it introduces a third signaler. S_2 has no reason to misrepresent its type in the current version of the game, but it would now have a more complex set of incentives. There is no reason to think that the basic insights would be fundamentally affected.

It is worth recalling a caveat mentioned earlier, however: The equilibrium changes if the ex ante probability that S_2 is committed to refusing at its final node is quite high. Thus, if S_2 were to move first and face very large audience costs for conceding, this alternative equilibrium could operate. In this case, there is a good chance that S_2 has to refuse regardless of what the government and opposition do. As a result, the government has no incentive to bluff, and since all the government's threats are genuine, the opposition's decision to support or oppose conveys no additional information. This suggests that there are some condi-

[17] The relationship between the audience cost term, a, and the probability of war is less straightforward because of two countervailing effects. Since all types for which $w_1 > -a$ can and do make genuine threats, an increase in a increases the range of types willing to make the threat and go to war if refused. An increase in a, however, decreases the probability that S_2 will refuse, since the chance that a given challenge is genuine increases with a. The net effect on the ex ante probability of war is unclear.

tions under which the introduction of an opposition in S_1 has no effect on behavior and outcomes in the modified game. As I will suggest below, this claim might generate an additional empirical implication.

IMPLICATIONS

The model suggests that open domestic political competition can decrease the danger of war due to asymmetric information. An unfettered opposition party can enhance the government's ability to make credible threats in a crisis by creating a second information source that effectively confirms the government's resolve. At the same time, the opposition party can undermine the credibility of some challenges by publicly opposing them. Since this strategy threatens to increase the probability of resistance from the rival state, it forces the government to be more selective about making threats. A government has less leeway to engage in strategic misrepresentation when it faces a domestic competitor with political incentives to reveal its low expected value from war. Together, these effects ensure that more information is revealed when a state permits open competition than when it does not.

This argument has important implications for the research program on democracy and international conflict. The model developed here demonstrates a mechanism through which institutions associated with democracy can help states overcome the difficulties inherent in bargaining. Accordingly, we should expect democratic states to have enhanced ability to reach peaceful outcomes relative to states in which competition is restricted or takes place out of public view. Unlike other arguments about democracy, this finding does not require any assumptions about the relative pacifism of democracies. The democratic state in this model is not motivated by norms of nonviolent conflict resolution, nor does it necessarily have "dovish" preferences due to the existence of domestic competition, as Bueno de Mesquita and Lalman (1992) assume. Instead, the model permits a wide range of possible preferences over war and peace. The prediction of fewer wars derives solely from the democratic state's superior ability to signal its true preferences, *whatever those may be*. As a result, this argument is consistent with Layne's (1994) observation that democracies have, in a number of cases, behaved quite belligerently, even toward one another. Indeed, this model shows that one way democratic states can bring about peace is by convincingly demonstrating unanimous support for war.[18]

Because the rival state is treated as a unitary actor, the model stops short of capturing the interaction between two democratic states. As a result, its relevance to the democratic peace—which is primarily a claim about democratic dyads—is more suggestive than conclusive. We can surmise that, if both polities were to permit competition of the kind posited here, then the probability of war would be lower than in an interaction involving one democracy or none. Given that this situation is not explicitly modeled, and given the caveat that the effect of domestic competition may be attenuated when the state is the target of a challenge rather than the initiator, the main value of the model does not lie in rederiving the democratic peace.

Instead, the model's central contribution is in suggesting general processes and patterns associated with democracy. The core logic of the argument—that public contestation facilitates the revelation of information in crises—does not depend on the constitution of the rival state. Thus, these institutions should have effects that are not confined to democratic dyads. A common claim in the democratic peace literature is that there are no such monadic effects and, in particular, that democratic states are just as war prone as nondemocratic states (e.g., Maoz and Abdulali 1989). Recent results in this area have been mixed, however, and several studies suggest that democracy has a generally pacifying influence independent of the regime type of rival states (e.g., Benoit 1996; Gleditsch and Hegre 1997; Ray 1995; Rummel 1995). This model is consistent in spirit with these results. It may also explain why studies of crisis escalation have produced weak and inconsistent results when looking for the effect of democracy on the probability that a crisis will escalate once it begins.[19] As we saw, the relationship between S_1's regime type and the conditional probability of war given a challenge is ambiguous.

In addition, two specific hypotheses emerge from the discussion in the previous section. Two effects are associated with the introduction of an opposition party: A confirmatory effect results from the opposition's ability to strengthen the government's signal, and a restraining effect results from the government's decreased willingness to bluff. The first suggests that, relative to nondemocratic states, regimes that permit public contestation are better able to convince rivals of their seriousness about using force. As a result, the probability that the rival state refuses a challenge must decline.[20] Thus, an implication of the model is that, ceteris paribus, threats made by democracies are less likely to be resisted than those made by states which do not permit competition.

The restraining effect implies that the probability of S_1 making a challenge in the first place decreases with the introduction of an opposition party. Recall that all

[18] This counterintuitive result nicely complements the finding by Bueno de Mesquita and Lalman (1992, 158–60) that states with dovish preferences may, under certain circumstances, be more likely to get involved in wars. Both findings reinforce the need to think about the role of information in mediating between preferences and outcomes.

[19] For example, Morgan and Campbell (1991) and Rousseau et al. (1996) show no or weak monadic effects of democracy on crisis escalation. Inconsistencies also appear in studies that focus on democratic dyads. For example, Rousseau et al. (1996) and Dixon (1994) find that joint democracy lowers the probability that a crisis will escalate to force, whereas Senese (1997) finds that democratic dyads are just as likely, and perhaps even more likely, to escalate to the use of force once involved in a crisis.

[20] In the game without opposition, the probability that S_2 will refuse, conditional on having been challenged, is s^*. In the game with an opposition, the probability of a refusal is the average of s and s^*, weighted by the conditional probability that the opposition supports the challenge. Since $s < s^*$, this weighted average is less than s^*.

types for which $w_1 > b$ make the challenge in equilibrium. Since the shift from suppressed to open competition implies that b increases, the likelihood that w_1 will fall in the relevant range decreases. The government's decreased willingness to bluff means that, all other things being equal, the probability of a challenge from a democratic state is lower than the probability of a challenge from a nondemocratic state. Because both of these effects derive from an argument about democratic states, not democratic dyads, both should be evident at the unit level and not be dependent on dyadic or "joint" democracy. Elsewhere (Schultz 1996, 1998), I provide preliminary evidence in support of these hypotheses.[21]

There may also be empirical leverage in the claim that domestic competition affects the game under a more limited set of conditions when the democratic state is the target rather than the initiator. This implies that, while democracies should be more selective about initiating crises, they may not be significantly more selective when choosing to resist a challenge.

Finally, the model suggests some patterns which should be observed within historical cases. Democratic governments should be sensitive to how domestic competitors may affect their ability to make credible threats in a crisis. They should be particularly careful about making threats that have tenuous political support, since the opposition party can readily undermine those threats. At the same time, foreign states should take into account the signals that emerge from the competitive process and make appropriate inferences. The finding that domestic opposition casts doubt on the credibility of the government's threats squares with a conventional wisdom that democracies are hard to read when they send multiple, contradictory signals. It is precisely under these conditions that the rival state faces the greatest uncertainty and the danger of war is highest. Yet, the model also suggests that mixed signals are only one possible outcome. When the domestic actors can agree, the result is less uncertainty than if there were only one information source. In this sense, the model tells us when we should expect democracies to speak clearly (when the payoff from war is either very high or very low) and when we should expect them to send mixed signals (when the payoff from war is in some middle range).

CONCLUSION

This article fits in the tradition of what Putnam (1988) refers to as "two-level games"—broadly speaking, games in which domestic and international interactions are mutually dependent. It is part of a growing set of formal models with an international actor and multiple domestic actors, all of whose strategies and beliefs are in equilibrium simultaneously (e.g., Milner 1997; Mo 1995; Pahre and Papayoanu 1997; Siverson 1998). Such models are particularly useful in moving beyond the unitary state assumption of traditional realism without undue loss of parsimony. Moreover, they offer a way to formalize general propositions about the effect of domestic institutional arrangements on international outcomes.

As noted at the outset, much of the literature on democracy and war has developed in isolation from the literature on crisis bargaining. Indeed, with few exceptions (Bueno de Mesquita and Lalman 1992; Fearon 1994), research in this area has failed to embed explanations of the democratic peace within a deductive framework that explains the outbreak of war. This article addresses this shortcoming by combining a model of democratic politics with a standard model of international bargaining that can account for conflictual outcomes. The resulting game shows how the introduction of public competition and a credible opposition party can enhance a state's ability to resolve disputes peacefully.

APPENDIX: FORMAL SOLUTION TO THE BARGAINING GAME

PROPOSITION 1. *The following strategies and beliefs constitute a perfect Bayesian equilibrium to this game.*

(P1.1) The government plays

$$\begin{array}{ll} CH,\ SF & \text{if } w_1 \geq k_{gov}, \text{ where } k_{gov} = -a, \\ CH,\ BD & \text{if } k_{gov} > w_1 \geq b, \text{ and} \\ SQ,\ BD & \text{otherwise.} \end{array}$$

(P1.2) If the government makes a challenge, the opposition plays

$$\begin{array}{ll} SUP & \text{if } w_1 \geq k_{opp}, \text{ and} \\ OPP & \text{otherwise,} \end{array}$$

where $k_{opp} \geq -a$. *If the government plays SQ, then the opposition plays SUP when* $w_1 \geq 0$.

(P1.3) Let q *denote* S_2*'s posterior probability that* $w_1 \geq -a$ *after observing the strategies of the government and opposition. Then,*

$$\begin{array}{ll} q = 1 & \text{if } S_2 \text{ observes } CH \text{ and } SUP, \\ q = \dfrac{F_1(k_{opp}) - F_1(-a)}{F_1(k_{opp}) - F_1(b)} \equiv q^* & \text{if } S_2 \text{ observes } CH \text{ and } OPP, \text{ and} \\ q = 0 & \text{if } S_2 \text{ observes } SQ. \end{array}$$

(P1.4) S_2 *plays*

$$\begin{array}{ll} RF & \text{if } w_2 \geq -\dfrac{1-q}{q}, \text{ and} \\ CD & \text{otherwise.} \end{array}$$

Proof. Most of the elements of this equilibrium are straightforward to derive. The opposition's strategy after the government plays SQ and the government's strategy at its final decision nodes follow readily from the payoffs. The latter turns out not to depend on the opposition's strategy, since in both cases SF is preferred to BD whenever $w_1 \geq -a$. S_2's posterior beliefs, given in P1.3, are derived logically from the strategies and Bayes's rule. Given these beliefs, S_2's expected payoff from RF is a weighted average of its payoff from war and its payoff from getting the good, or

$$EU_2(\text{RF}) = qw_2 + (1 - q). \qquad (2)$$

[21] It is interesting to note that Fearon (1994, 585) generates similar predictions based on the assumption that democratic governments can generate higher audience costs—an assumption that is not required here. Though the causal stories leading to these predictions are different, their similarity reflects common reliance on the logic of signaling.

Comparing this value to S_2's certain payoff from CD, zero, yields the decision rule in P1.4.

What remains is to show that the strategies given in P1.1 and P1.2 are sequentially rational and to derive expressions for k_{opp} and b. To do so, we need to derive the two parties' expectations about S_2's behavior. If the opposition supports the threat, S_2 knows that the government will stand firm in the face of a refusal, or $q = 1$. Given this, S_2 plays RF only if $w_2 > 0$—that is, if it prefers war to conceding the good. The probability of this is

$$s \equiv \text{Prob}(w_2 > 0) = 1 - F_2(0). \quad (3)$$

If the opposition opposes the threat, then S_2 updates as shown in P1.3 and plays RF if the condition given in P1.4 holds. The probability with which this condition holds is

$$s^* \equiv \text{Prob}\left(w_2 > -\frac{1-q^*}{q^*}\right) = 1 - F_2\left[\frac{F_1(b) - F_1(-a)}{F_1(k_{opp}) - F_1(-a)}\right]. \quad (4)$$

Since $b < -a$, the term in brackets is less than zero. It follows that s^*, the probability with which S_2 will refuse when the opposition opposes the threat, is always greater than s, the probability with which S_2 will refuse when the opposition supports the threat.

For the government's decision rule to hold, it must be the case that a government of type $-a$ is indifferent between CH and SQ. Since it was proposed that an opposition of type $-a$ will play OPP, the expected value of a challenge for a government of this type is

$$EU_{gov}(\text{CH}) = s^*(-a) + (1 - s^*). \quad (5)$$

The value for choosing SQ is zero. Setting equation 5 equal to zero, we find that

$$s^* = \frac{1}{1+a} \quad (6)$$

must hold for the government's cutpoint to be rational.

Note that S_2's strategy leaves all governments of type $w_1 \leq -a$ indifferent between CH and SQ. Because of this, the cutpoint b separating types that make the challenge from types that choose the status quo can be drawn anywhere in the range $[p_1 - C_1, -a]$. In equilibrium, b must be such that equations 4 and 6 hold simultaneously. Setting these expressions equal to each other, we find that

$$F_1(b) = [F_1(k_{opp}) - F_1(-a)]F_2^{-1}\left(\frac{a}{1+a}\right) + F_1(-a). \quad (7)$$

This expression need not be simplified further for now, since it is sufficient to conduct the comparative statics of interest to this article. It is necessary to point out, however, that for the cutpoint at b to be lower than the cutpoint at $-a$, it must the case that

$$F_2(0) > \frac{a}{1+a}.$$

This implies that the probability with which S_2 has negative expected utility from war must be sufficiently high relative to the audience cost term, a. Proposition 2 (below) considers the equilibrium that results when this condition does not hold.

The opposition's cutpoint must be such that the opposition is indifferent between OPP and SUP when $w_1 = k_{opp}$. For an opposition of this type, expected payoff from supporting the government is

$$EU_{opp}(\text{SUP}) = s(r-1)k_{opp} + (1-s)(r-1). \quad (8)$$

The expected payoff from opposing the government is

$$EU_{opp}(\text{OPP}) = s^*(-k_{opp}) + (1-s^*)(-1). \quad (9)$$

Setting these expressions equal to each other, we find that

$$k_{opp} = 1 - \frac{r}{s^* - s(1-r)}. \quad (10)$$

To check that k_{opp} is indeed bounded at the lower extreme by $-a$, we find that k_{opp} is decreasing in r, meaning that it is minimized when $r = 1$. Substituting $r = 1$ into equation 10 yields

$$k_{opp} = 1 - \frac{1}{s^*} = -a.$$

Both k_{opp} and b can be reduced to functions of the parameters, but the algebra is needlessly messy. The expressions in equations 7 and 10 are sufficient for the comparative statics. In particular, we note that b is a decreasing function of k_{opp} and, hence, an increasing function of r. *Q.E.D.*

PROPOSITION 2. *If* $F_2(0) < a/(1 + a)$, *then the equilibrium derived in proposition 1 has the following characteristics.*

(P2.1) $k_{gov} = b = -F_2(0)/(1 - F_2(0)) > -a$.

(P2.2) $k_{opp} = k_{gov}$.

(P2.3) Keeping the definition of q *from above, then*

q = 1 *if S_2 observes CH (regardless of the opposition's strategy), and*

q = 0 *if S_2 observes SQ.*

(P2.4) S_2 plays

RF if $w_2 \geq 0$, *and*

CD otherwise.

Proof. Assume that the government's cutpoint, k_{gov}, is as given in P2.1. Because $b = k_{gov} > -a$, a threat by the government is always genuine. Hence, regardless of the opposition's strategy, $q = 1$ following a challenge. As before, when $q = 1$, S_2 plays RF only if its expected utility from war is greater than zero. The probability with which S_2 will refuse is thus equal to s, as defined in equation 3.

The government's cutpoint must be such that a government of type $w_1 = k_{gov}$ is indifferent between CH and SQ. Since $k_{gov} > -a$, a government of this type will go to war in the event of a refusal by S_2. Hence,

$$EU_{gov}(\text{CH}) = s(k_{gov}) + (1-s). \quad (11)$$

Setting equation 11 equal to zero, we find that the government is indifferent between SQ and CH if

$$k_{gov} = -\frac{F_2(0)}{1 - F_2(0)},$$

as proposed above. Moreover, $k_{gov} > -a$ whenever the condition stipulated in the proposition holds. Since all types for which $w_1 < k_{gov}$ strictly prefer the status quo, there is no bluffing in this equilibrium, or $b = k_{gov}$.

Finally, the opposition's cutpoint is derived as in equation 10. Since S_2 plays RF with the same probability regardless of the opposition's strategy, however, we derive k_{opp} by setting $s^* = s$. Doing so generates $k_{opp} = k_{gov}$. *Q.E.D.*

The final proposition demonstrates that this game can generate informative signaling in equilibrium even when the government's threat generates no audience costs, or $a = 0$. There are actually two such equilibria. The first is the same as that described in proposition 1, since there is nothing in the

above derivation which requires $a > 0$. In this case, both the strategies of the government and opposition are informative, since not all types of government make the challenge, and the opposition confirms some threats and not others. The second equilibrium takes a slightly different form, because, when there are no costs to backing down at the final node, all possible types of government may make the challenge in equilibrium. In this case, the government's signal is uninformative, but the opposition's signal still helps separate some genuine threats from bluffs.

PROPOSITION 3. *The following strategies and beliefs describe a perfect Bayesian equilibrium to this game when* $a = 0$.
(P3.1) The government plays

$$\begin{array}{ll} CH,\ SF & \text{if } w_1 \geq 0, \\ CH,\ BD & \text{otherwise.} \end{array}$$

(P3.2) The opposition plays

$$\begin{array}{ll} SUP & \text{if } w_1 \geq k_{opp}, \text{ and} \\ OPP & \text{otherwise, where } k_{opp} \geq 0. \end{array}$$

(P3.3) Let q *equal* S_2*'s posterior probability that* $w_1 \geq 0$ *after observing the strategies of the government and opposition. Then,*

$$q = 1 \qquad \text{if } S_2 \text{ observes } CH \text{ and } SUP, \text{ and}$$
$$q = \frac{F_1(k_{opp}) - F_1(0)}{F_1(k_{opp})} \equiv q^* \qquad \text{if } S_2 \text{ observes } CH \text{ and } OPP.$$

(P3.4) S_2 *uses the decision rule shown in P1.4.*

Proof. The beliefs given in P3.3 follow directly from the equilibrium strategies, and S_2's decision rule is identical to that derived in proposition 1. The strategy of the government follows from the fact that $a = 0$. Because there are no costs to making a challenge and then backing down, only types for which $w_1 \geq 0$ will stand firm at the final node, but all types are willing to make the challenge, since the worst possible payoff from making a challenge (0) is the same as the payoff from playing SQ. Thus, the signaling strategy of the government is uninformative.

To show that there is informative signaling in this equilibrium, we need to show that the opposition's cutpoint, k_{opp}, is greater than or equal to zero, so that the opposition's strategy separates at least some of the types that make genuine threats from those that are bluffing. Determining k_{opp} requires that we first calculate s^*, the probability that S_2 will refuse after the opposition plays OPP. Using the definition of q^* from P3.3, s^* is derived as in equation 4:

$$s^* = 1 - F_2\left[\frac{-F_1(0)}{F_1(k_{opp}) - F_1(0)}\right]. \qquad (12)$$

The calculation of k_{opp} follows exactly as in equations 8–10 above. Substituting equation 12 into equation 10 generates an unwieldy and recursive expression for k_{opp}. Nevertheless, it can be shown that there exists an equilibrium solution in which k_{opp} is greater than or equal to zero. To see this, we note that, if k_{opp} is nonnegative, then it is decreasing in r. It is equal to one when $r = 0$ and hits its minimum when $r = 1$. Thus, if we can show that $k_{opp} \geq 0$ when $r = 1$, it follows that this is true for all values of r.

We conjecture that, when $r = 1$, $k_{opp} = 0$ in equilibrium. If this is true, then the opposition's strategy perfectly separates genuine threats from bluffs: All the former receive support, and all the latter are opposed. In this case, S_2 interprets OPP as a sure signal that $w_1 < 0$, or $q = 0$. As a result, all types of S_2 have an incentive to refuse an opposed challenge, or $s^* = 1$. Substituting $r = 1$ and $s^* = 1$ into equation 10 confirms that $k_{opp} = 0$, as conjectured.

Since k_{opp}, at its minimum, equals zero, it follows that there is some nonnegative solution for k_{opp} for all values of r. Because of this, the opposition continues to send informative signals even when $a = 0$, since its strategy separates some genuine challenges from bluffs. *Q.E.D.*

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Thoreau's Alternative Economics: Work, Liberty, and Democratic Cultivation

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Henry Thoreau's Walden *is frequently read as the tale of a disaffected Romantic individualist escaping the rigors of community and modern society to seek refuge in a pondside idyll. I argue that this is a fundamental misreading which misses the political import of the book.* Walden *reveals itself to be a democratic advice-book focusing on the tensions between the political ideal of free self-direction and the unfavorable work conditions that laborers often face. Thoreau's goal is to set out a strategy by which economically vulnerable citizens may enact their liberty and autonomy in threatening employment conditions. To that end, Thoreau reformats themes and practices from various ancient philosophical traditions.* Walden *is one of the few texts in our tradition in which strategies are thought through for the individual laborer attempting to maintain freedom while at the same time making a living. It therefore merits our closest attention.*

The constant choices and self-direction entailed in meaningful work enliven all our capacities. On a stroll through New York's Upper East Side as people catch their morning taxi, it is hard not to be impressed by the sleekness and fervor you find there—the alert eyes, purposeful movements, and general liveliness of a caste of individuals with invigorating and demanding employment. One great advantage of the modern era is the array of stimulating occupations for those fortunate enough to have the tastes, capacities, and fortune of birth that allow them to find meaningful work. For so many others, work is of another sort: arduous labor, loud machinery, or confinement in some cubicle, unable to leave because an aging parent depends upon them for support. In its contrasting roles as our foundational source of empowerment and as one of our most invasive afflictions, work plays a dominant role in shaping the energies and capacities we bring to our families, our communities, and political action.

The role that work plays has become greater in recent decades. In the United States, the number of hours spent at work each year has increased significantly since the early 1970s. According to the U.S. Bureau of Labor Statistics (1997, 1), women worked an average of 20% more in 1996 than in 1973, adding a full 233 hours to their working year, while men added 100 hours to theirs. The proportion of people with very long workweeks has risen significantly in every occupation (Rones, Ilg, and Gardner 1997, 8–10). There have also been important changes in the quality of work. Corporate downsizing and a shift in organizational structure have made careers much less stable, and there has been a marked rise in contingent work and temporary employment (Belous 1989). In America, the conditions that permitted a person to envision a stable career are vanishing for many people (Harrison 1994, 214–24). In a society that enshrines labor as a calling, these transformations in the shape and place of work are likely to have many far-reaching effects.

Political theorists have long given attention to the fundamental role that work plays in political life (Arendt 1958; Elster 1986; Lane 1991; Marx [1867] 1967; Murphy 1993). But very few voices in our tradition discuss employment from the point of view of the individual laborer attempting to maintain freedom and equilibrium in relation to the world of work. A major exception is Henry David Thoreau's ([1854] 1995) *Walden*. Some critics have dismissed it as the work of a petulant and disaffected Romantic individualist, but this is not an accurate reading of the text.[1] More careful attention reveals *Walden* to be a carefully constructed study of the tensions between liberty and employment in times of economic change.[2] *Walden* is one of the rare works of political theory that asks us to think critically about our relation to the work we do, and it gives advice as to strategies and practices we

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An earlier version of this article was presented at the American Political Science Association meetings in August 1997. I would like to thank the following for their comments: Philip Abbott, Tom Augst, Laurence Buell, Blair Campbell, Peter Cannavo, Douglas Crase, Robert Dawidoff, Michael Goodhart, Douglas Long, Robert Needham, Anne Norton, Daniel O'Neill, Karen Orren, Carole Pateman, Tracy Strong, Victor and Judy Wolfenstein and the anonymous reviewers for this journal.

[1] For example, compare Morton and Lucia White's (1962, 41) classic description of *Walden* as "a diatribe against the life of the village [by an] isolated individual . . . free of all social attachments" with Thoreau's chapter, "The Village," in which he emphasizes that "*every day or two* I strolled to the village to hear some of the gossip . . . which, taken in homeopathic doses, was really as refreshing in its way as the rustle of leaves and the peeping of frogs" (Thoreau [1854] 1995, 108–9, emphasis added) and with his comment that "I had more visitors while I lived in the woods than at any other period of my life" (p. 93) Thoreau constantly reminds us of his avid taste for society, "for I dearly love to talk" (p. 53). Or compare the Whites' (1962, 155) statement about a "Thoreauvian . . . antipathy to civilization as such" with Thoreau's ([1854] 1995, 19) comment: "It is asserted that civilization is a real advance in the condition of man—and I think that it is."

[2] The vision of *Walden* as a guidebook for malingerers and political free-riders ignores Thoreau's constant reference to the needs and predicaments of his fellow citizens, his compassion for their plight, and the frequency with which he explores themes central to the democratic theory of the Jacksonian era—liberty and dependency, serfdom and nobility. The apolitical reading of Thoreau also ignores his political activism and aid to fugitive slaves (Richardson 1986). On the various ways in which political theorists have misread Thoreau and missed the deeper political purposes of his works, see Taylor 1996.

might adopt if we want to elaborate more flexible responses to the shifting economy of work in which we find ourselves.

Thoreau's central theme is that working conditions in a market democracy can easily undermine liberty and erode autonomy. His goal in *Walden* is to set out strategies by which people might enact their freedom when they face working conditions that are likely to threaten their autonomy and well-being. After a careful reading of *Walden* we may still be uncomfortable with many of Thoreau's strategies—his idea of voluntary poverty will not appeal to everyone—but at least we will no longer underestimate *Walden*'s complexity and subtlety as a reflection about the preconditions for democratic enactment in market societies.

WALDEN AND EMPLOYMENT

We have had few political philosophers like Henry David Thoreau who have made their living as day laborers. Thoreau worked as a house painter, mason, land surveyor, gardener, and pencil maker so that he would have the free time to be a writer, naturalist, and abolitionist lecturer. His insights about the centrality of employment stem in part from the strenuous ideal of democracy that Thoreau absorbed while growing up in Jacksonian America and in part from his vulnerability as a day laborer within the swiftly restructuring economy of his time.[3] *Walden* is filled with references to economic transformations that parallel those of our era in so many ways: a new interconnectedness brought about by the development of transportation infrastructure; diminished distances owing to the growth of railways, canals, and roadways; the rise of newspapers; the increasing power of large corporations; and the swift changes in working conditions brought about by the spread of factory labor (Lebeaux 1977; Neufeldt 1989; Sellers 1991).[4] The commercial spirit had taken hold of Concord, as it had so much of the republic in the postwar period (Wood 1992, 325–47). Thoreau's mother took in boarders, his father ran a struggling pencil manufacturing business, and the family lived in "honest poverty" (Lebeaux 1977, 38). Thoreau attended Harvard as a scholarship student, but as Edward Emerson wrote (Lebeaux 1977, 43): "The comparatively small amount which it cost to maintain a boy at Harvard . . . was enough seriously to strain the resources of the family."

Given this background, it should not be surprising that Thoreau considered the question of employment to be crucially important; in his words, it was "the most practically important of all questions." In 1851 he wrote:

> There is little or nothing to be remembered written on the subject of getting an honest living. Neither the New Testament nor Poor Richard speaks to our condition. I cannot think of a single page which entertains, much less answers the questions which I put to myself on this subject. . . . Is it that men are too disgusted with their experience to speak of it? Or that commonly they do not question the common modes? The most practically important of all questions, it seems to me, is how I shall get my living, and yet I find nothing to my purpose in any book. . . . I consider that society with all its arts, has done nothing for us in this respect (Thoreau [1906] 1951, 164).

Walden was an attempt to fill this lacuna and to explore how citizens forced to fend for themselves in a market democracy might get their living without scotching their liberty.

A careful reading of *Walden* leaves little doubt that employment is the enframing theme of the book. In the opening section, "Economy," in which Thoreau gives reasons for reading his book, he repeatedly underlines the occupational problems facing his fellow residents in Concord. Images of particular forms of employment and their drawbacks crowd the first pages of *Walden* (pp. 1–52): in which Thoreau writes of his fellow citizens in their roles as farmers, teamsters, slave-drivers, surveyors, herdsmen, town officers, inspectors, sailors, factory operatives, merchants, masons, housewives, and carpenters. When Thoreau writes of visiting Concord he speaks primarily of workspaces—of "shops and offices, and fields" (p. 2)—and of the desperation he finds there. The word "labor" and its cognates is mentioned dozens of times in the opening section of *Walden*; seven times on the first three pages alone. When Thoreau wrote a précis of the book to deliver on the lyceum circuit, he first entitled the speech "Getting a Living." There can be little doubt as to what this book is about.

DEMOCRACY AND EMPLOYMENT

Thoreau argues that whatever one may think of the American political system (he writes of it as "comparatively free," p. 3), at the level of everyday work life it has produced only limited improvement over the feudalisms of Europe. Mortgaged to the banks, deeply in debt, threatened with financial failure, and toiling without cease, the men and women of Concord are, for all their seeming independence, still living as if they were "serfs of the soil" (pp. 1–3). "The mass of men lead lives of quiet desperation. What is called resignation is confirmed desperation" (p. 4). Compared to the rest of the world, certainly, America is free, but working conditions consistently reproduce some of the worst aspects of the old tyrannies. Despite the ostensible liberties of the American nation, Thoreau's audience still finds itself "lying, flattering, voting, contracting yourselves into an atmosphere of thin and vaporous generosity" (p. 4). *Walden* is about the ways in which the American project of democratization may be seen as blocked and disabled by everyday working conditions.

Thoreau makes four sorts of argument about the tensions among democracy, autonomy, and employment. First, to be free means to have time for one's proper pursuits; time to figure out what one wants to

[3] On economic restructuring in Jacksonian America, see Sellers 1991.

[4] For his views on changes in transportation in his time, see Thoreau ([1854] 1995, 34, 60, 75); on corporations, p. 17; on factory labor, pp. 16, 22.

do and then go out and do it. Yet, earning a living can easily fill up or overshadow the time for such pursuits.[5] "The laboring man . . . has no time to be anything but a machine" (p. 3). Second, there is a tension between work and democratic capacities. Employment conditions frequently entail a "contraction and dilation" (p. 4) of the self, which undermines autonomy and upsets our ability to make the rational calculations necessary for the meaningful enactment of liberty (pp. 3, 22, 61). Third, there is also the problem of the narrowing of our choice-set. Seeking success in the terms of the current work environment means accepting what public opinion at any given moment values as the best life. But individuals may well have tastes that lead them to want a sort of life different from the narrow subset which their society values and promotes. "The life which men praise and regard as successful is but one kind. Why should we exaggerate any one kind at the expense of others" (p. 12)? Individuals who have tastes and/or capacities which lead them to develop their talents other than through the socially approved work-paths thus need strategies for protecting their own pursuits in the face of these socially sponsored exaggerations about what is admirable and what is not. Fourth, a concern with maintaining employment may prevent people from taking principled political positions and thus make them acquiesce in immoral policies. As Thoreau (1993, 4) suggests in "Civil Disobedience":

> Practically speaking, the opponents to a reform in Massachusetts are not a hundred thousand politicians at the South, but a hundred thousand merchants and farmers here who are more interested in commerce and agriculture than they are in humanity, and are not prepared to do justice to the slave and to Mexico, *cost what it may*.

Thoreau carefully draws our attention in *Walden* to the fact that he moved to his cabin "on Independence day, or the fourth of July, 1845" (p. 55), and the book is studded with references to the political nature of his explorations of independence and freedom. George Kateb (1992, 82) writes of Thoreau and his generation as the intellectual inheritors of the founding; they "disclose its fuller meaning." Thoreau shares with other writers of his time the idea that the republicanism of the founding period has established only the institutional preconditions for democracy and liberty, and the real work of creating a democratic culture remains to be done.[6] The founding opened up a new possibility, but the crucial challenge is to enact democracy, to flesh it out in concrete attitudes, practices, and institutions. People too easily mistake the legal preconditions of liberty for the real thing. Here is how Thoreau ([1854] 1993, 22) puts the problem in "Slavery in Massachusetts":

> Now-a-days, men wear a fool's cap, and call it a liberty cap. I do not know but there are some, who, if they were tied to a whipping-post, and could get but one hand free, would use it to ring the bells and fire the cannons, to celebrate *their* liberty. So some of my townsmen took the liberty to ring and fire; that was the extent of their freedom; and when the sound of the bells died away, their liberty died away also; when the powder was all expended, their liberty went off with the smoke.

Thoreau is not alone in his belief that we need to think through how we might establish real liberty in the United States. Much of the work of Emerson, Margaret Fuller, George Ripley, Elizabeth Cady Stanton, Walt Whitman, Bronson Alcott, and other antebellum reformers can be usefully read within this perspective of a hortatory literature of democratic enactment. There is a widespread recognition in this era that most of the values democrats take seriously—liberty for all, equality among people, personal autonomy—are very hard to realize in an everyday world long formatted by more hierarchical visions and in working conditions that produce both opportunities and severe challenges.[7] Democratic capacities are fragile and evanescent. They tend to fade from sight amid the bright temptations of everyday life unless they are enacted in concrete practices, strategies, and dispositions. This belief in the evanescence of the central democratic capacities is clearly an echo of republicanism, with its vision of human liberty constantly threatened by the corruptions of power and money and by general inattentiveness.[8] *Walden* is a particularly interesting work within this tradition because of the way it works out the theme of democratic enactment in relation to the economy of everyday life and the trade-offs of work and leisure.

DEMOCRACY AND SELF-CULTIVATION

Walden is also important because of its focus on individual laborers trying to navigate the dilemmas of their work life. The book reflects many of the conventions of a broader democratic-advice genre that rose to prominence in the antebellum period. In the United States in the 1830s and 1840s there was a widespread exploration of how citizens might prepare themselves for their new roles as democratic citizens, while as fathers and sons they attempted to orient themselves in a market economy, and as mothers they trained future citizens (Wood 1992, 357). Bookstores were filled with manuals of advice, young men's guidebooks, periodicals for mechanics, and exemplary biographies of famous Americans, such as Ben Franklin and George Washington (Neufeldt 1989, 102–5). The Boston area, in particular, was a center for this movement of democratic self-cultivation, with its popular lyceum lectures, young men's mercantile libraries, evening

[5] For some, of course, work is their proper pursuit, and the dilemma does not arise. Thoreau explicitly exempts such fortunates from his discussion: "I do not speak to those who are well-employed" (p. 10).

[6] Entirely typical of this tradition is a comment by Ralph Waldo Emerson (1983, 953–4) in "Fate": "Nothing is more disgusting than the crowing about liberty by slaves, as most men are, and the flippant mistaking for freedom of some paper preamble like a Declaration of Independence, or the statute right to vote, by those who have never dared to think or act."

[7] See the opening chapters of Wiebe (1995). Also see Curti (1943, 134–43, 295–317) and Tyler (1944, 5–22, 47–67).

[8] This constellation of concepts is famously traced in Pocock (1975).

schools, and literary societies (Cayton 1987, 605; Curti 1943, 357). A wide range of writers advised ordinary Americans as to how they might give themselves some of the skills and capacities that the upper classes could count on as their birthright.[9] The question that guides this literature is how individual citizens might teach themselves the skills they need to make full use of the new material and political conditions they find around them. That these writers should stress self-cultivation is not surprising in an era when public education was still the preserve of the upper classes (Curti 1943, 363ff).

Modern scholarship has not been particularly sympathetic to the antebellum literature of democratic self-cultivation. For example, Cayton's (1987) influential essay on Ralph Waldo Emerson stresses what she finds to be elements of social control and class domination within the movement for democratic self-culture. Cayton (pp. 606–7) argues that local boosters and businesspeople in many American cities wanted to maintain moral order among the young men in their employ, and the self-culture movement was a means to inculcate business-friendly values. Cayton's work is typical of much recent scholarship on nineteenth-century moral didacticism in its debunking tone and its unwillingness to see that there might have been something more going on in this movement than mere moral indoctrination. Modern middle-class scholars who have had the advantages of a free public education and access to colleges and universities have not generally been very sensitive to the predicaments facing clerks and mechanics who were struggling to rise in society without the benefits of adequate training.[10]

Perhaps another reason for the modern scholarly disdain for this movement (and writers like Thoreau) is that the focus on the individual in the democratic self-cultivation movement seems to them to foreshadow the narcissism, privatism, and self-concern detected in our own era by some scholars (Lasch 1979; Putnam 1995). Certainly, one can never be too vigilant about the narcissism that individualistic societies tend to engender. But an anachronistic reading of our own reputed insufficiencies into the writers of the Jacksonian era underestimates the important functions democratic self-cultivation plays in periods of social and economic upheaval. Writers of that time were aware of the movement from a society in which people were given their social rank by an accident of birth to one in which a new mobility was available to most adult white males. The challenges and opportunities of mobility made pressing the question of self-cultivation, and of education in general. Aristocrats had tutors, rhetoric instructors, and a college education to prepare them for the public sphere. How were those born to an average lot to achieve the capacities necessary for full civic participation? We have "no schools for ourselves," Thoreau writes in *Walden*, "our education is sadly neglected" (p. 71).

Recent scholars also may tend to underestimate the benefits of the self-cultivation literature if they do not recognize the importance of self-preparation in nonaristocratic societies. In market democracies, a great deal is at stake in the way dispositions and habits are formed. As Thoreau shows, the successful practice of self-cultivation can easily mean the difference between a life spent in freedom and a life caught in a spiral of desperation. Aristocrats can rely not only on an elaborate educational infrastructure but also on relatively secure liberties, since they are assigned these freedoms along with their estates and rank in society. By contrast, the liberties of citizens in a market democracy are often won by their own efforts and capacities. Not all individuals concerned with their dispositions and self-training are narcissistic; for some it may be a matter of survival or preparation.

As Neufeldt (1989) has pointed out, Thoreau's work has deep resonances with the literature of democratic self-cultivation. Thoreau owned a number of "young men's guides," and a passage in the opening section of *Walden* parodies this self-help literature (Neufeldt 1989, 103–5). But *Walden* stands out within the literature on democratic self-culture because it sees democratic citizenship as existing in tension with—rather than as supported by—the practices involved with everyday work, business, and enterprise. As Shklar (1991) and Wood (1992) have shown, the social revolution inaugurated by the founding generation had a radical influence on the way Americans understood the place of labor in life. The attack on aristocracy after the Revolution was cast in terms of an assault on idleness and leisure and brought with it a "heightened appreciation of the significance and dignity of labor" (Wood 1992, 277). Shklar (1991) has argued that the standing of American citizens was, from the beginning, tied up with their status as workers: Free democratic citizens neither labored in the degraded conditions of the slave nor loafed in the idleness of the aristocrat. When Thoreau writes in *Walden* of "serfs of the soil," of the need to move from noblemen to "noble villages of men,"[11] or of the servility of businessmen who contract and dilate their personalities (p. 4), he is wielding tropes taken from a discourse about aristocracy and slavery that was central to the language of democrati-

[9] See Channing's "Self-Culture" in Robinson (1985). There is a good review of the literature of self-culture in Neufeldt (1989, 23–52) and Curti (1943, 355–60).

[10] Cayton's (1987) article is illuminating but one sided. Her own sources offer an alternative view to the position she sets out. Cayton quotes the clerks and young lawyers of the Cincinnati Literary Society, who describe what they are doing as expanding "the wider culture of our intellectual, moral, and social powers" (p. 608). They see their efforts as the development of their capacities and skills, but Cayton imposes on this straightforward self-description a narrative about outside domination and business boosterism. She pays insufficient attention to the possibility that when the young men said that their literary society was a means to expand their own powers they knew exactly what they were doing.

[11] The passage is worth quoting in full, since it shows how entirely Thoreau shares his generation's vocabulary of democratic preparation and enactment: "As the nobleman of cultivated taste surrounds himself with whatever conduces to his culture, —genius, —learning, —wit, —books, —paintings, —statuary, —music, —philosophic instruments, and the like; so let the village do, —not stop short at a pedagogue, a parson, a sexton, a parish library and three selectmen. . . . Instead of noblemen, let us have noble villages of men" (p. 72).

zation in his day.[12] The radicalism of Thoreau's perspective consists in his suggestion that, far from generalizing access to the liberty and dignity that the aristocratic classes enjoyed, everyday working conditions in America might merely be generalizing toiling desperation. This is why *Walden* is a work of democratic-advice literature. Thoreau sees the conditions of American employment as the most stubborn obstacle to the enactment of real freedom.

It should be noted that Thoreau does not think that all Americans—not even all those who are economically vulnerable—will necessarily need the strategies he sets out. He explicitly exempts from his arguments all who can, on reflection, describe themselves as well employed, however that might be (p. 10). And some people, he suggests, are so strong and valiant that they would come out well anywhere. To these, he emphasizes, he will not speak (p. 9) Thoreau also excludes from his concern those who have inherited (or dishonestly attained) wealth (p. 164). By exempting from his discussion anyone who feels a comfortable meshing between work and liberty, Thoreau traces out an audience of those who labor *without* a calling, a group rarely addressed within American literature.

AN EXPERIMENT IN PHILOSOPHY

How does one address the predicament of the mass of people who try to enact liberty and their own particular vision of the good without being able to rely on inherited wealth or standing? How might the myriad blessings of modern technology and productivity be unlocked for the class of people apparently so ill-served by modern developments? "I wish to show at what a sacrifice [the advantage of modern civilization] is at present obtained, and to suggest that we may possibly so live as to secure all the advantage without suffering any of the disadvantage" (*Walden*, 20). At issue is how those who have trouble getting a living nonetheless can come to achieve what Thoreau believes he has achieved, namely, the ability to escape misery and desperation, and how they can learn to find "encouragement and inspiration in the present condition," even to the extent of "cherish[ing] it with the fondness and enthusiasm of lovers," as Thoreau says that he himself does (p. 10).

After passing in review a number of the typical occupations of his day, Thoreau introduces the strategy which he believes offers the best alternative to the "enslavements" (p. 4) that everyday work life so frequently entails. He suggests that moderns who seek to enact democratic liberty have much to learn from the ancient philosophical schools of various nations. "The ancient philosophers, Chinese, Hindoo, Persian and Greek, were a class than which none has been poorer in outward riches, none so rich in inward" (p. 9). What Thoreau finds particularly attractive in these schools is their strong emphasis on bringing wisdom to bear on the practices of everyday life. "To be a philosopher is not merely to have subtle thoughts, nor even to found a school, but so to love wisdom as to live according to its dictates, a life of simplicity, independence, magnanimity and trust. It is to solve some of the problems of life, not only theoretically, but practically" (p. 9). The "independence" in this passage is important. In developing his democratic experiments outside of schools and other institutions, Thoreau sees himself as returning to a philosophical independence abandoned by many modern philosophers. As Thoreau puts it, "the success of great scholars and thinkers is commonly a courtier-like success" (p. 334). The worry that philosophy may be undermined if the working conditions of philosophers become too courtier like—toadying to superiors, courting public reputation, establishing power blocs, and so on—is part of a general criticism in *Walden* of those forms of life in which participants' stated goals (love of wisdom in philosophy, love of freedom and independence among everyday citizens) are mangled in their enactment and are thus not adequately embodied in the occupations people adopt to realize them. He thinks more "Yankee" shrewdness needs to be applied to resolving the tensions between our higher order values and the practices of our everyday work life.

The idea that ancient philosophy may offer the key to modern work predicaments goes back very far in Thoreau's thought. Early in his *Journal* (Lebeaux 1977, 80) he wrote that "Zeno the Stoic stood in precisely the same relation to the world as I do now.... Bred a merchant... he strolls into a shop and is charmed by a book by Xenophon and straightaway becomes a philosopher." The key to *Walden* is the way it combines ancient philosophical practices and modern economic calculation to set out a strategy by which citizens may realize their liberty.[13] The seriousness with which the ancient philosophers took the challenge of maintaining their poise in the pursuit of the good, and their willingness to reform the practices of their everyday life so as to reflect their higher values, become the central attitudes that can enable a democratic citizen to reopen the space of freedom at the level of daily life.

Thoreau's description of a philosopher as someone who solves "the problems of life" may at first reading appear to be a fundamental misunderstanding of ancient philosophy. After all, in the works of Plato and Aristotle, which are the focus of our contemporary curriculum in the field, the philosopher's development of wisdom and insight leads him away from political involvement and creates a marked tension with the political life of the *demos*. Philosophy is defined in contrast to, and in many ways as an alternative to, everyday democratic life.

Thoreau was a well-trained classicist, one of the best of his generation (Sattelmeyer 1988, 7), but the Harvard curriculum of his day gave less exposure to Plato and Aristotle than to such writers as Cicero and the

[12] Dawson (1996, 36) has pointed out that the first English usage of "democrat" was as a term of contrast with aristocrats during the French Revolution and in that era was generalized to refer to all those with antiaristocratic sentiments.

[13] To my knowledge, the only critic who has noted this is Vernon Parrington (1958, 392ff).

Stoics, who emphasized a more complex relation between philosophic practice and political life (pp. 7–8).[14] Thoreau's principal classics teacher, Eliot Professor of Greek Literature C. C. Felton, encouraged his students to see the close connection between classic literature and the everyday life of ancient peoples (pp. 7–8). Moreover, Thoreau's vision of ancient philosophy comprised Chinese texts, such as the *Analects* of Confucius, which are quoted throughout *Walden* (Cady 1961), as well as numerous Indian texts, such as the *Laws of Menu*, the *Bhagavadgita*, and the *Vedas*, all of which put a strong emphasis on philosophy as a form of advice on how to live as an ordinary householder.[15] All these traditions gave the philosopher a large role in acting as an example and in setting out pathways for others to follow as they confronted the challenges of everyday life.

As many recent scholars have stressed, the relation between the ancient philosopher and the political realm is considerably more complex than the simple question of whether the philosophically enlightened citizen will participate in politics.[16] While one strain of ancient thought encouraged a turn away from political life, another looked on philosophy as ideal training for public affairs, since it taught how to preserve moral integrity amid the tumult of political struggle (Foucault 1984; Sandbach 1989, 107). Philosophers frequently found employment as tutors and advisors in powerful households, teaching politically involved citizens how to balance political position, public functions, activities, and obligations (Sandbach 1989, 16–8). Given the constant fluctuations and reversals of political life in the ancient world (in some ways comparable to the fluctuations and reversals of work life in modern times), the citizen needed to stabilize his desires and sense of self so as to maneuver with maximum flexibility, to show himself capable of ruling, of being ruled in turn, and of holding high office while resisting the temptations of corruption and immorality (Foucault 1984, 114–5).

One reason philosophers came to be seen as fitting advisors to citizens and rulers was that they were versed in exercises by which individuals learned to bring their passions and desires under control (Rabbow 1954, 112–50). To operate well as a citizen or ruler one had to learn attention, concentration, and resistance to temptation. None of these capacities are ever easy for humans to attain, plagued as they are by anxiety, greed, and fear. Thus, much of the training in the ancient schools was designed to help people circumvent the temptation of their passions. For example, the Epicureans formulated various aphorisms to quell fears and realign the individual with the natural world. One of these, discussed in Hadot's illuminating book on ancient philosophical practices, is the so-called *tetrapharmakon*: "God presents no fears, death no worries. And while good is readily attainable, evil is readily endurable" (Hadot 1993, 26; author's translation). Philosophers could teach a range of exercises and strategies to help people navigate the everyday temptations and obstacles of political life while preserving their moral integrity and political reputation. *Walden* is a modern revival of this tradition. Thoreau uses the narrative of his experiment at Walden Pond to show how the resources of ancient philosophy can be used by those who find themselves vulnerable within the modern market system. As I show in the next section, the similarity lies not just in general attitude; the practices and exercises Thoreau sets out in *Walden* are functionally identical to the techniques found in ancient philosophy.

EXERCISES, STRATEGIES, AND REPLACEMENT PRACTICES

Thoreau advocates a number of exercises and practices as means of offsetting the forces that threaten autonomy, liberty, and happiness. (1) Thought experiments can clarify our relation to our fundamental needs and thereby counteract anxiety. (2) Maxims can be coined to offset the autonomy-hampering effects of public opinion. (3) Household accounting methods, centered on autonomy, can establish an equilibrium between one's higher order goods and the practices of everyday life, particularly with regard to the relation between work and leisure. (4) Replacement practices can be developed to encourage individual flourishing while offsetting both economic dependency and social relations of exploitation. (5) Ways of approaching nature and the physical world can be designed to expand and train the self and especially the body as well as to open to nature in a way that makes it more than just a tool of human needs. (6) Dietary and sumptuary strategies can be adopted to promote the various goods set out above.

In line with my suggestion that *Walden* is a work about democratic enactment for those facing uncongenial employment choices, these practices and exercises can be separated into two categories. On the one hand are those meant to liberate the time and tranquillity needed to elaborate projects expressing one's personal autonomy (practices 1 through 4). On the other hand are those more exclusively tied up with Thoreau's own individual sense of the good life, the particular perfectionistic doctrines to which he adheres. These include the observation of nature by living in a cabin in the woods rather than in a small apartment in the city, and so on.

I make this distinction because each category admits to different degrees of generalizability. Practices in the first group are generally relevant to most citizens who are interested in preserving their freedom under conditions of market democracy. Many in the second group can only be interpreted as reflections of Tho-

[14] On Thoreau and the Greek ethical schools, see Richardson (1986, 104).

[15] On the secular focus of Chinese political philosophy, see Ames (1994, 16–22), Fingarette (1972, 26–8), and Hall and Ames (1987, 156–92). For a highly illuminating discussion of the Confucian tradition of pragmatic self-cultivation, see Cheng (1991, 84–6). On Hindu discussions of philosophy for householders, and on Hindu influences on Thoreau, see Christy (1932, 187–233).

[16] See Foucault 1984; Hadot 1993; Nussbaum 1994; Rabbow 1954; and Sandbach 1989.

reau's personal tastes and inclinations, such as his dislike of tea or preference for country living. These are not relevant to as many people as is the advice Thoreau gives about the maintenance of liberty in the world of a potentially overwhelming labor environment. Thus, I concentrate primarily on the first category of practices in claiming Thoreau as a philosopher of democratic enactment. They illustrate how Thoreau reformats ancient philosophical practices as elements of a democratic politics of example to address the tension between work and autonomy in the everyday life of market democracies.

Thought Experiments to Offset Anxiety

Thoreau believes that one of the great obstacles to autonomy is anxiety. Self-governance, which is to say the ability to guide ourselves by the lights of our own developing sense of the good, requires a certain poise and presence of mind that is easily lost in a complex existence with many cares. For example, worries about long-term economic security can easily lead to a harried life that soon subverts the ends sought through pursuing such security.

Thoreau shows that, for a correct balance of ends and means, our calculus of liberty must take as its base point a well-understood knowledge of our most fundamental physical needs. These play such a large role in our struggle for freedom that Thoreau capitalizes them as if they were deities: "The necessaries of life for man in this climate may, accurately enough, be distributed under the several heads of Food, Shelter, Clothing, and Fuel; for not till we have secured these are we prepared to entertain the true problems of life with freedom and a prospect of success" (*Walden*, 7).

In the second half of the "Economy" section Thoreau guides the reader through a set of thought experiments designed to sensitize them to the border between what they do and do not need in terms of fuel, shelter, clothing, and food. These exercises cultivate a disposition to distinguish between what one needs as a physiological organism and what public opinion and habit make one think one needs. An ability to step away from the sense of the socially necessary then allows room for maneuver; one is no longer locked into a set of trade-offs between anxiety-producing complexities and an unrevisable set of goods. An example of Thoreau's strategy here can be found in his section on shelter:

> If one designs to construct a dwelling house, it behooves him to exercise a little Yankee shrewdness, lest after all he find himself in a workhouse, a labyrinth without a clew, a museum, an almshouse, a prison, or a splendid mausoleum instead. Consider first how slight a shelter is absolutely necessary. I have seen Penobscot Indians, in this town, living in tents of thin cotton cloth, while the snow was nearly a foot deep around them, and I thought they would be glad to have it deeper to keep out the wind (p. 18).

There are numerous noteworthy elements in this passage, but I will point out just a few. Note the parallel between Thoreau's strategy and the second part of the Epicurean *tetrapharmakon*, which states that the good is readily attainable, evil is readily endurable. Thoreau tries to remove the sting of autonomy-harming anxiety by stressing that it is easier than we think to have the goods we need and endure even the greatest local evils, which in New England is foremost the winter's cold. Thoreau addresses each of the necessities of life in turn and through a similar set of reflections gives readers a more realistic vision of what they really need and what is a luxury (a luxury, that is, if they are concerned above all to maintain their autonomy). Ancient philosophy was replete with such therapies aimed at clarifying our vision of nature so as to release us from potentially harmful passions and anxieties.[17] Thoreau's strategy is the same as in the ancient techniques, although Thoreau is careful to cast his arguments in terms of values his audience already holds; for example, he asks why the Yankees who are so shrewd about building machines and forming commercial enterprises nonetheless permit their lives to sink into desperation.

Notice also the didactic use Thoreau makes of the Indians. He consistently refers to Native Americans as having managed to work out a relation with the necessities of life which did not force them to lose their liberty—which allowed them to live in the realm of stark necessity while being ingenious enough to have maintained forms of real richness (p. 19), liberty, and comfort (p. 21). Throughout *Walden*, Native Americans exemplify a population living in relation to the New England climate in a way that allows them to maintain their freedom and happiness without the problems Thoreau sees his fellow countrymen facing.

Maxims to Offset the Effects of Public Opinion

In the Hellenistic schools and in other ancient philosophical traditions, such as Confucianism and Buddhism, adherents memorized maxims that summed up the complex moral and physical attitudes advocated by their school.[18] These short sayings were used as a guide in daily life, as a source of moral orientation amid constant choices, dilemmas, and stress. Maxims in *Walden* play a similar role. Thoreau is elaborating a new ethos of everyday life, and *Walden* has a place in it analogous to *Poor Richard's Almanac* in the ethos he is calling into question. The maxims in *Walden* sum up and reinforce a particular attitude toward work and time, rendering the ethos into portable form for use in daily life.

Thoreau employs maxims as a means of stabilizing the new ethos he is developing in the face of public opinion, which continually threatens to upset it: "The cost of a thing is the amount of what I will call life which is required to be exchanged for it" (p. 19). "Money is not required to buy one necessary of the soul" (p. 213). "How can he remember well his igno-

[17] See, for example, the magisterial discussion of Lucretius's therapy of the passions in *De Rerum Natura* by Nussbaum (1994, 239–79).

[18] On maxims in Greek ethical schools, see Hadot (1993, 41). On the role of precepts in Confucianism and Buddhism, respectively, see Waley (1938, 51) and Foulk (1995).

rance—which his growth requires—who has so often to use his knowledge" (p. 3)? "Simplify, simplify" (p. 60). These maxims are meant to stick in the mind and to generate the dispositions that ground the replacement ethos.

Thoreau mirrors John Stuart Mill in his sensitivity to the autonomy-hampering effects of public opinion in a mass democratic society and to the threats that public censure poses to liberty. But Thoreau's economic vulnerability as a day laborer led him to insights Mill missed. Most important is Thoreau's suggestion that the most dangerous influence of public opinion may well be in the way it shapes our attitudes as consumers, misleading us into thinking that we can afford things we cannot (not, at least, if we want to maintain our autonomy). Public opinion can create within us autonomy-harming ideas about what our needs truly are. The maxims studding Thoreau's text are designed to help readers remind themselves that, from the perspective of their fragile freedoms, many more items may turn out to be luxuries for them than they may at first think.

An Alternative Household Economics

Thoreau's reflections are based on the assumption that people are forced to be self-supporting, but that freedom entails an ability to maintain contact with one's ideals, with what Thoreau terms "proper pursuits" (*Walden*, 18). In modern conditions, he suggests, that may be possible only with the most simplified life, since the means by which one gains the resources for a complicated existence are very likely to destroy the time and psychic poise needed to keep a firm grip on what are and are not one's proper pursuits. Thus, individuals who are threatened with a desperation-producing work life may find it useful to revise their basic wants and needs downward.

Thoreau advances a number of formulas and maxims to help the individual achieve an acceptable equilibrium between the need to preserve autonomy and the need to gain the necessities of life (Stull 1984). Some were cited above. But there are also more general algorithms for determining what is affordable if one ranks the maintenance of autonomy as a high priority. One such equation is the following: "The cost of a thing is the amount of what I call life which is required to be exchanged for it, immediately or in the long run" (p. 19). As Richardson (1986, 166–7) has pointed out, Thoreau was a careful reader of the works of Adam Smith, Ricardo, and Say, and the aforementioned formula is a virtual paraphrase of Smith's definition of cost in *The Wealth of Nations*. As Thoreau says in *Walden*, "economics is a subject which admits of being treated with levity, but it cannot be so disposed of" (p. 18). "He must be a great calculator indeed who succeeds" (p. 60). Thoreau gives sample account books for a household economy centered around creating a space for liberty, offering an example of an accounting practice that takes freedom seriously (pp. 31–40). He shows how an everyday practice (drawing up the household accounts) can be reformatted as a means of enacting liberty in the daily life of average citizens.

A Pattern-Book of Practices

Much of *Walden*, and of Thoreau's work elsewhere, focuses on the creation of practices which might serve as a better means to realize autonomy than the customary work practices of Concord. *Walden* is a pattern-book of replacement practices to help people elude the financial dependency that leads to desperate lives within ostensibly free societies. Much of Thoreau's ingenuity in *Walden* and in his miscellaneous essays is devoted to showing how the entirely banal practices of everyday life—walking, talking with friends, botanizing, reading and writing, doing day labor—can be rethought and reconstructed to enhance autonomy. *Walden* is an attempt to work out an acceptable culture—that is, practices, exercises, and dispositions—that will render poverty livable. Replacement practices aim to make poverty lush rather than barren, and thus disclose it as a position which individuals might choose if they felt their liberty threatened by the standard work life they encounter. Native Americans and ancient philosophers were able to create fully satisfying lives in conditions of great material poverty, and it is not unreasonable to believe that amid the much greater material wealth of bustling America such savage comforts (p. 21) may be even more easily attainable.[19]

Using the material advantages of our society, we can work out replacement practices that take advantage of any comforts available to us within a position of poverty. The aim is to escape boredom, avoid despair, maintain our financial independence, extend our faculties, and open up a space to express our autonomy. Thoreau's discussion of replacement practices generally follows a standard pattern. After examining the customary way of achieving particular ends, Thoreau explains why the standard practices should be questioned and perhaps rejected. He then offers his own approach and discusses how it enhances autonomy.

A good example is the practice of walking, which Thoreau discusses both in the essay *Walking* and in *Walden*. He concentrates not on walking for exercise or as part of business but on the most banal of all forms, strolling around out of doors with no real purpose in mind: "I think that I cannot preserve my health and spirits, unless I spend four hours a day at least—and it is commonly more than that—sauntering through the woods and over the hills and fields, absolutely free from all worldly engagements" (Thoreau [1862] 1993, *Walking*, 50–1).

The customary practice to which Thoreau contrasts his ideal of sauntering is that of staying inside all day—not so much inside a building as inside the cares and problems of one's work and home life. *Walking* and the numerous other passages in Thoreau's works devoted to the benefits of sauntering are really about the

[19] "Though we are not so degenerate but that we might possibly live in a cave or wigwam or wear skins to-day, it is certainly better to accept the advantages, though so dearly bought, which the invention and industry of mankind offer. . . . With a little more wit we might use these materials so as to become richer than the richest now are, and make our civilization a blessing. The civilized man is a more experienced and wiser savage" (pp. 25–6).

relation between labor and leisure and about setting out strategies so that one may work without occluding one's self and sense of the good.

Thoreau also contrasts beneficial walking to the sort during which one just ruminates about housework or other cares: "Of course it is of no use to direct our steps to the woods, if they do not carry us thither. . . . It sometimes happens that I cannot easily shake off the village. The thought of some work will run in my head and I am not where my body is,—I am out of my senses. In my walks I would fain return to my senses" (*Walking*, 52). He argues that walking can be used to regain contact with one's bodily senses, and with nature, so as to remove oneself mentally from the world of work. "What business have I in the woods, if I am thinking of something out of the woods" (*Walking*, 52)? In Thoreau's work the exercises that return us to everyday experience are valued because they refresh the faculties and give the self a ballast outside public opinion and standard custom (pp. 72–3). This, in turn, allows one to make better judgments about the choices that come up in everyday life. This is but one example of the many replacement practices Thoreau envisions and recounts.

My argument is that something more went on at Walden Pond than mere moral perfectionism. To read *Walden* as nothing more than Thoreau's working out of "private business," his personal attempt to answer the question "How then shall I live?" misses the large sections of the book that are sincere and nonparodistic attempts to write a workable piece of democratic-advice literature aimed at those whose liberty is threatened by their employment. The first five and the last five pages of Walden are very straightforward about addressing an audience made up of the poor and others whose work lives threaten to subvert their "proper pursuits." One can only see *Walden* as a parody of advice literature if one focuses on a few satirical sections and ignores the harrowing discussion of human toil and desperation on its opening pages. *Walden* is a democratic-advice book for the poor and work-weary (p. 1).

THOREAU'S ALTERNATIVE ECONOMICS

In 1851, Thoreau ([1906] 1951, 164) wrote that the question of getting a living is "the most practically important of all questions" because everyone must answer it before going on to explore and then express the possibilities of freedom. The very structure of *Walden* mirrors and reinforces this message. It opens with "Economy," a section in which Thoreau focuses on mundane worries, such as employment, food, shelter, and livelihood. But once these self-oriented issues are addressed, he turns to the world outside—the community in Concord and the contemplation of nature. Thoreau is one of the first moderns to develop an idea of the physical environment as an autonomous presence toward which human beings have some accountability, a dialogic partner with its own interests and processes rather than just a tool for human ends (Buell 1995, 7–8). The book may begin with a certain degree of self-concern, but this is just a stepping stone on the way to a greater objectivity.

The rhetorical conventions of *Walden*, with its quasi-allegorical history of the individual soul's progress from the mundane world of business and self-centered worries to participation in a suprahuman realm beyond, with "new, universal and more liberal laws" (p. 209), links it to other philosophical allegories, such as Plato's *Republic*. But in contrast to the philosopher-sage Socrates, who justifies an aristocratic politics of guardianship, the democrat-sage Thoreau illustrates strategies that can make the individual self-reliant.

Perhaps the best way to understand the exercises and practices advocated in *Walden* is to see them as a form of ethico-political athleticism that Thoreau hopes may inspire and liberate others. In the same way that a great athlete can inspire us to greater effort and grace, so Thoreau acts out writ-large versions of democratically pertinent skills, such as self-direction, the maintenance of ideals, economic awareness, and frugality. He writes frequently of the importance of setting examples, even if they may seem dauntingly strenuous: "Be sure that you give the poor the aid they most need, though it be your example which leaves them far behind" (p. 49). An 1851 journal entry reads: "If you would convince a man that he does wrong, do right. But do not care to convince him. Men will believe what they see. Let them see" (Thoreau [1906] 1951, 46). "Does Wisdom work in a tread-mill? or does she teach how to succeed by her example?" (Thoreau [1862] 1993, 79). Through recounting his experiments by the pond, Thoreau hopes to inspire his readers to question choices and decisions they make unthinkingly and perhaps adopt a more rigorous household economics, one that takes their life goals into account. Thoreau reformats the familiar republican vocabulary about preserving liberty through resisting luxury and renders it applicable to the case of individual citizens attempting to make their way as laborers in a modern market economy.

But Thoreau was well aware that the strenuous practices and exercises he advocates will not appeal to all. He carefully includes in *Walden* an episode of failed advice-giving through which he meditates on the stringency of his example. In the chapter "Baker Farm," Thoreau encounters an Irish immigrant and his family in a shack near the pond. The Irishman, John Field, labors day and night for a local farmer hoeing a bog but never seems to get ahead. Thoreau summarizes for him an autonomy-oriented household economics based on extreme simplicity.

> John heaved a sigh at this, and his wife stared with arms a-kimbo, and both appeared to be wondering if they had capital enough to begin such a course with, or arithmetic enough to carry it through. It was sailing by dead reckoning to them, and they saw not clearly how to make their port so; therefore I suppose they still take life bravely, after their fashion, face to face, giving it tooth and nail, not having the skill to split its massive columns with any fine entering wedge, and route it in detail;—thinking to deal with it roughly, as one would handle a thistle. But they fight at overwhelming disadvantage,—living, John Field, alas! without arithmetic, and failing so (p. 134).

Thoreau, it is obvious, was well aware of just how radical his austere household economics would appear to some of the readers he was addressing.

We might think that Thoreau overestimates the degree of freedom open to people who live in conditions of poverty and does not adequately recognize the predicaments poverty can bring; the way in which the attainment of freedom is complicated by having to fend for both self and family, for example. We might also find that Thoreau does a poor job of accounting for the importance of cultural capital. Not all poor people have an intact family on which to rely, draw benefit from a Harvard education, and live in a community as vibrant as that of antebellum Concord. To be completely satisfying, Thoreau's alternative economics would have to take all these things into consideration and also would have to reflect more articulately on the weight of structures and institutions. Still, no book can do all things, and *Walden* does bring considerable illumination to the issues it touches. The book insists that America is "comparatively free" and that strategies to circumvent any disadvantages in its institutions are thus both thinkable and pursuable. Thoreau's strategy is certainly not based on any illusions about the grimness of poverty. Indeed, few northern writers of this time paid as much attention as Thoreau did to the fate of poor laborers, to the people living in human sties by the railroad tracks: "The forms of both old and young are permanently contracted by the long habit of shrinking from cold and misery, and the development of all their limbs and faculties are checked" (p. 22).[20]

Thoreau clearly does not romanticize poverty, but he does refuse to see all poor people as similarly situated with regard to life and choice. He forcibly reminds us by his own example that there is a great diversity in the experience and capacities of poor people, that economic poverty can coexist with widely varying degrees of social capital, and that poverty is not in all cases the worst of afflications. Thoreau is less likely than some middle-class writers to see poverty as intrinsically and in all cases a barren and liberty-destroying condition.[21] *Walden* questions the automatic connection we make between economic improvement and increased liberty by pointing to groups such as Native Americans and ancient philosophers, who maintained liberty in conditions of poverty, and to the workers of Concord, whose freedom often diminished as their economic life improved. These arguments are not frequently heard in American political thinking.

CONCLUSION

The best way to read *Walden* is to see it as addressing people in all classes who have talents and tastes that are not adequately mirrored in the lifeways deemed to be profitable in their time. *Walden* is designed to render readers more sensitive to their choices about the place of work in their lives and about the trade-offs they make between their freedom and their consumption goals. Thoreau's strategy of voluntary poverty as a means to liberty and the development of one's own projects is designed for people with a strong sense of their own autonomy who might conceive of projects that cannot be realized through their work. Certainly, not all people have these characteristics. But as social scientists from Weber onward have shown, the modern employment system tends to produce highly disciplined individuals able to eschew short-term comfort in the name of more evanescent goals (Weber 1958, 180–3). The system that produces the sorts of dilemmas Thoreau addresses is also likely to reproduce the forms of character that would allow the strategies in *Walden* to make sense to people.

Thoreau's strategies are but one part of the political theory necessary to address the tensions between employment and freedom found in modern democracies. We also need to explain the structural changes in economic and political systems that allow these tensions to persist. But in terms of the immediacy of its advice and of its utility for individual citizens, *Walden* is thus far without parallel. In *On Liberty* Mill suggests that it is important for democratic societies to produce individuals who might make experiments in living and thus serve as generative centers of innovation for their society, illustrating escape routes from the tyranny of majority custom (Collini 1989, 69–70). Thoreau's *Walden* is an exemplary account of one such experiment in living, and one which shows a constant awareness of the exemplary functions of its innovations. It contains an array of suggestions for practice that readers might take up as part of their own democratic activity. As a democratic-advice book, *Walden* has a proximity to action that contemporary political theory often lacks. Much modern political philosophy is a philosophy of postponement, in the sense that the extension of liberty entails waiting for a collective social actor willing to put radical changes into practice—the proletariat finally coming to consciousness or a constitutional convention finally being convened and bringing about the redistributions of Rawls's (Rawls 1972, 195–201) difference principle. The thrust of much modern political theory of this sort is to postpone our political activity to some distant day. There is a utility in Thoreau's focus on immediate enactment, on the possibility that we might "adventure on life now" (p. 9), and in his emphasis on showing us how it might be done. Citizens, after all, have to fight for their freedom in the here and now, in the (it is hoped) brief period before the institutional

[20] Nor did most abolitionists draw explicit parallels between wage labor in the North and slavery in the South, as Thoreau did. "I sometimes wonder that we can be so frivolous, I may almost say, as to attend to the gross but somewhat foreign form of servitude called Negro Slavery, there are so many keen and subtle masters that enslave both north and south" (Thoreau [1854] 1995, 4). This gives us some idea of the gravity with which Thoreau observed northern work conditions, since such comparisons were usually the stock of proslavery discourse in the South (Fitzhugh [1857] 1960; Grayson 1856).

[21] Indeed, Thoreau believes that in some cases a fear of poverty keeps individuals from adopting a principled political position and leads them to acquiesce in immoral policies. Adopting poverty may thus be a crucial step toward enacting one's liberty, as is suggested in the passage from *Civil Disobedience* quoted above (Thoreau [1862] 1993, 4).

designs of political philosophers and activists are finally realized.

Walden also has a number of advantages as a work through which to understand democratization in nineteenth-century America. Unlike some European visitors, Thoreau clearly saw the inequalities in the United States in the antebellum period, and he specifically warned against ignoring the poor: "It is a mistake to suppose that, in a country where the usual evidences of civilization exist, the condition of a very large body of the inhabitants may not be as degraded as that of savages. I refer to the degraded poor" (p. 22). Nonetheless, Thoreau refuses to see poverty as in itself an affliction and believes that we underestimate the possibility of rendering it tolerable. He also shows great attentiveness to the social and political ramifications of such economic changes as the expansion of the commercial press, the growth of new means of communication and transport, and the development of new forms of labor. Much of his thinking is focused on themes that later writers, such as Habermas (1987), theorize as the colonization of the lifeworld—the expansion of economic and political systems into the dense web of unreflective practices and tacit understandings that make up our customary life.

Thoreau's analyses of this process are neither apocalyptic nor overly optimistic, and his concern throughout is practical. His central goal is to show how the disadvantages of the modern era can be circumvented while its advantages are secured (p. 20). It is this willingness to celebrate the conveniences of a market society and at the same time recognize the realization of liberty within its conditions requires high levels of imagination, creativity, and self-control that makes *Walden* such an interesting work for democratic theorists in an era of economic restructuring. Thoreau brings serious thought and sustained experimentation to bear on an area of life seldom examined within our tradition—the border between our individual life-projects and the means we adopt to make our living. In a time such as ours, which has seen the spread of employment to new populations and a deep restructuring of work conditions, *Walden* may well be one of our most resonant and useful works of political theory.[22]

[22] *Walden* is a work of political theory because a central concern is the realization of liberty for citizens of modern market democracies, and it explores this question with frequent references to the language of democratization current in the antebellum period.

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When the Party Breaks Up: Exit and Voice among Japanese Legislators

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In exploring the conflicts between individual interests and the conditions that facilitate the disintegration of a political party, this article modifies the exit, voice, and loyalty framework developed by Hirschman. The utility of that framework is examined using the recent change in Japan, that is, the demise of the so-called 1955 system, in which the predominant conservatives confined the socialists to the status of a major but perennial opposition party. The quantitative analysis focuses on the split of the Japanese Liberal Democratic Party and the internal dispute in the Democratic Socialist Party of Japan. These cases provide an interesting comparison of how individual characteristics and contextual conditions affect members' decisions to exercise exit and voice. At the same time, they illuminate how party-level changes have been influenced by intraparty factors.

Parties are important agents of political change in contemporary democracies. Some studies focus on interaction and bargaining among parties as catalysts of systemic change (Budge and Keman 1990; Downs 1957; Laver and Schofield 1989); others are concerned with the distinctive influence of parties on the shape of national agenda and policies (Blais, Blake, and Dion 1993; Cameron 1978; Hicks and Swank 1984, 1992). The existing research, because of its ultimate concern with systemic consequences and changes, tends to regard the party as the unit of analysis and rarely examines individual politicians, whose interests and behavior should be important determinants of intraparty dynamics. Their motivation to be members of a party is closely related to party unity and coherence and thus ultimately influences the entire political system. In a parliamentary democracy, for example, the legislative majority forms a cabinet, and so the unity of parties as well as interparty bargaining are important factors in the durability of party government. Conversely, in a presidential system with less coherent and loosely organized parties, as in the United States, a politician's motivation to be affiliated with a party is itself a subject of interest, because the party coexists with the pluralistic, atomistic competition of individual legislators.

This article explores the conflicts between legislators' interests and the conditions that facilitate the disintegration of a political party. The analytical framework focuses on members' motivations to stay or quit the organization and is based on the exit, voice, and loyalty explanation developed by Hirschman (1970). The original model is significantly modified, however, in terms of how member preferences affect exit and voice. The framework is examined in the context of the recent demise of the so-called 1955 system in Japan, a system in which the predominant conservatives confined the socialists to a major but perennial opposition party. The analysis focuses on the split in the Japanese Liberal Democratic Party (LDP) in 1993 and the internal dispute in the Social Democratic Party of Japan (SDPJ; formerly the Japan Socialist Party, JSP) in 1994 and 1995.[1] They provide interesting cases for a comparison not only of how the two political parties coped with the crisis but also of how intraparty factors influenced the system-level changes. As the following section will explain, the LDP's split brought about a major shift in Japan's postwar party system, that is, the end of conservative predominance. Also, the SDPJ's internal disputes have been an important cause of the rapid rise and fall of coalition governments since then.

The LDP broke up in June 1993. Despite intraparty disagreements about election and political finance reforms as well as party management, the split appeared premature. Because majority power in a parliamentary system gives an enormous advantage to individual members, it is considered unlikely that they will dismantle their own party. The LDP case challenges this conventional wisdom. The party had experienced no significant decline in popular support, despite public criticism of its implication in a political scandal. In contrast, the socialists managed to maintain party unity until the formation of the Democratic Party in September 1996 caused a major split. This was notable given an internal dispute about traditional party policy positions and a decline of electoral support.

Through a quantitative analysis, this article explores how the behavior of individual politicians is related to party dynamics.[2] This perspective is still new in the study of Japanese politics. In addition to the motivation of individual members, the utility of the exit, voice, and loyalty framework will be examined. More specifically, I will argue that the ability and/or propensity to exit magnifies voice in political organizations, and that the members who are the first to exit are most active in

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Part of this research was supported by a grant from the Ministry of Education in 1995. Earlier versions of this paper were presented at the annual meeting of the American Political Science Association, Chicago, August 31–September 3, 1995, and the Conference on Democratic Institutions in East Asia at Duke University, November 8–9, 1996. I acknowledge comments and suggestions from John C. Campbell, Gary Cox, Takeo Hoshi, Sadafumi Kawato, Ikuo Kume, T. J. Pemple, Susan Rose-Ackerman, Frances Rosenbluth, Kenneth Shepsle, Myungsoon Shin, and Michael Thies. All remaining shortcomings and mistakes are mine.

[1] The party changed its name in early 1996. Hereafter, I refer to it as the SDPJ.

[2] The aim is not to provide a comprehensive explanation for the LDP split or the disharmony of the SDPJ. On the LDP, see Cox and Rosenbluth 1995.

exercising voice.[3] Empirical evidence from Japan is used to demonstrate the characteristics of a political organization, especially a party, that contribute to this tendency. My argument is diametrically opposed to the trade-off relationship between exit and voice in Hirschman's original formulation. In the following section, I will set out a general framework for analyzing party politicians' organizational behavior and clarify the critical modification of Hirschman's model made in this analysis.

EXIT, VOICE, AND LOYALTY: AN ALTERNATIVE APPROACH TO PARTY ORGANIZATION

Hirschman's (1970) exit, voice, and loyalty framework is based on consumer response to the deterioration of quality in a product. Some people cease to buy that product and switch to an alternative (exit); others continue to buy it and inform the producer of their demand that quality be restored or improved (voice). The decision hinges mainly on rational calculation of the expected costs of exit and voice but is also influenced by loyalty to the product and, ultimately, to the firm. The normative implications of Hirschman's framework are that the postponement of exit and the promotion of voice are important for halting quality decline in the first place, and that an effective combination of exit and voice is ultimately required for the recuperation of the organization (in his original case, a firm). My modifications of the model make it applicable to organizational behavior in politics.

Responses to Organizational Performance and a Cleavage between Activists and Nonactivists

Hirschman's formulation implicitly assumes universal agreement on the definition of product quality. In the analysis of consumer behavior, for example, the deterioration is assumed to be reflected in a decline in demand, which suggests that consumers uniformly respond to a quality change as they do to a price change (Hirschman 1970, Appendix A). Therefore, exit is a primary response to quality deterioration, but loyalty may encourage consumers to use voice. This premise becomes problematic when the model is applied to a political organization. The service or gain that members enjoy from their association with a political organization (a party in this study) is analogous to the product in the market situation. In the political context, however, members may have completely different ideas about the kind and level of service the organization should provide as well as different evaluations of the current quality. There is an important demarcation between those who actively respond to the "deterioration" through exit or voice and those who support maintaining the status quo (due to their insensitivity or their different point of view) and do neither.[4] When a political organization engages in the provision of a public good, this demarcation becomes even more salient than when the organization's members are concerned only with the consumption of a private good.

A Public Good and a Collective Good

Hirschman (1970, 98–105) gives the example of government officials who stay in the organization to provide a public good, that is, a government policy. Legislators who belong to a political party produce public policy, and members of the dominant party make policy that is very likely implemented as government policy. In both cases, exit and voice calculations can be greatly affected by a person's attitudes toward the public goods (or bads) that the organization produces.

> The member will compare, at any one point in the process of deterioration, the disutility, discomfort, and shame of remaining a member to the prospective damage which would be inflicted on him as a prospective nonmember and on society at large by the additional deterioration that would occur if he were to get out (Hirschman 1970, 103).

These members weigh the personal loss incurred by a decline in their organization's reputation (a collective good for the organization members) and the loss to society (including themselves) that would ensue from the deterioration of a policy (a public good). In this regard, the quality of the public good *produced* (a policy) by the organization matters as much as the quality of the collective good *consumed* (the reputation) in the decision to choose between exit and voice.

These behavioral patterns would change, however, if two assumptions implicit in Hirschman's framework were abandoned. The first is that members as well as society at large respond uniformly to the declining quality of the public good. Thus, in Hirschman's formulation, all the members recognize the quality decline in both the public good and collective good at the same time. Conversely, I assume that the evaluation of both the public and collective good differs from one individual to another. Furthermore, attitudes toward them tend to be more varied than attitudes toward a private good, such as a salary. For example, whereas some may be concerned about deterioration in an organizational policy, others may see none, and thus the reputation of the organization would not necessarily be seen as declining. It is possible to imagine a case in which members are worried about the declining reputation of an organization but believe it is providing a high-quality public good. The primary difference among members, therefore, lies in their perceptions of overall

[3] This is the argument presented by Bates and Lien (1985). They point to the tendency in developing nations for the owners of mobile factors, who can evade heavy taxation by exit, to give voice more actively than the owners of immobile factors, who cannot exit. For a comprehensive criticism of Hirschman's Framework, see, for example, Barry (1974).

[4] Hirschman (1970, 50–1) believes that such a demarcation becomes salient in the exceptional case of quality-conscious consumers responding sensitively to a quality decline. He also suggests that these consumers closely parallel members of a political organization who resort first to voice when they disagree with organizational activity.

organizational performance (including both its policy and reputation) rather than their response to it. The present framework assumes that "active" members will try to close the gap by halting the deterioration in either policy (a public good) or reputation (a collective good), while "inactive" members will do neither. In the present analysis, the demarcation between the active and inactive members is the most important determinant of organizational dynamics.

The second assumption to be abandoned is a basic seesaw pattern of voice and exit. Those who exit do not voice and vice versa. Thus, in order to improve organizational performance and prevent exit, it is essential to encourage members to exercise voice without exit. This assumption is not appropriate, however, for organizations that provide a public good. Former party members have both the opportunity and incentive to exercise voice as members of society who object to the declining quality of a public good (the party's stance on a policy). Exit need not be the final response to the organization; voice after exit may still be effective in changing its policies. If the option of voice after exit exists, then voice within the organization can be regarded as a predecessor to exit rather than an alternative to it. Hirschman (1970, 104) admits the exception that, in such situations, "the alternative is not so much between voice and exit as between voice from within and voice from without (after exit)," and instead of the seesaw pattern, exit and voice are both used by the same individuals.[5] Voice within the organization is an important factor, especially if subsequent exit by those who exercise voice could have a significant effect and their threat to exit is credible. Even if change does not occur and they eventually exit, their exercise of voice helps recruit other members to exit with them, which puts more pressure on the organization. Voice within also augments the legitimacy of forming or participating in an alternative organization after exit. This calls into question Hirschman's view that loyalty is an intervening factor in the rational calculation of costs and benefits on which members base their exit or voice decision.

The Concept of Loyalty

Hirschman's idea about loyalty is meaningful as long as the trade-off between exit and voice is maintained, voice is regarded as a superior way to influence organizational performance, and voice is considered loyal behavior. Yet, the effectiveness of voice after exit and the variety of perceptions about organizational performance challenge this notion. We cannot equate loyalty with the strength of one's intention to stay (and exercise voice) inside the organization, nor can we associate a specific response to organizational performance with loyal behavior.

I define *loyalty* as a willingness to act when one recognizes a problem with organizational performance. The greater one's loyalty, ceteris paribus, the greater the likelihood of taking action. Theoretically, the strength of one's willingness can be measured before a situation arises, that is, with no knowledge about individual or contextual specifics. In reality, however, the willingness to take an action cannot be separated from rational cost-benefit calculations and contextual conditions. Thus, in the empirical analysis presented here, the level of loyalty is treated as a black-box factor (see Appendix), but the concept is important in understanding certain organizational dynamics that are likely to increase member loyalty.

Effect of Organizational Activities

Several organizational conditions influence a member's choice of exit and voice: barriers to exit; proximity and availability of attractive alternatives; responsiveness of authority to voice; and the ease with which individuals can exercise voice (Hirschman 1970, 39–41, 79–82). According to Hirschman, these systematically affect loyalty among organizational members, but I do not assume that they are stable constraints: The members' own activities change these conditions more frequently and effectively in politics than in a market situation. For example, defectors from a certain organization often form a new one, that is, provide a viable alternative, rather than join an existing alternative. While this changes external conditions after exit, there are also factors that influence internal conditions. Organizational activities and their accompanying intense intraorganizational interactions encourage members to resort first to voice instead of exit. Unlike Hirschman's assumption that the promotion of voice in such cases is the means to prevent exit, some members use these strategic opportunities to persuade others to remedy organizational performance by resort to voice and then exit. These effects will be demonstrated in the empirical analysis of the SDPJ case.

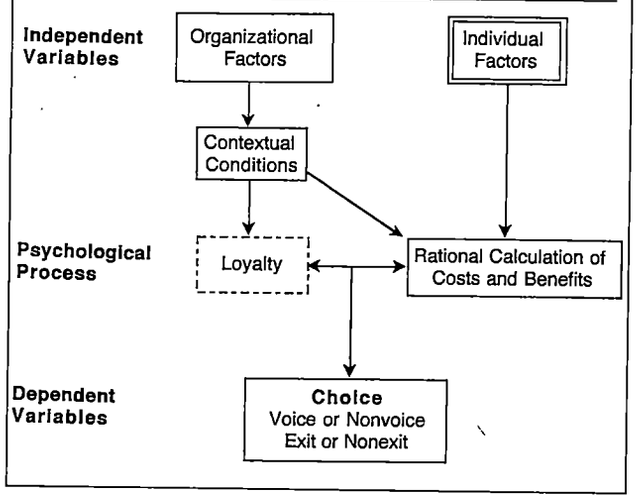
The framework of exit, voice, and loyalty discussed here is summarized in Figure 1. Both organizational and individual factors are independent variables, but there is a distinction. Whereas individual factors figure exclusively in cost-benefit calculation, organizational factors may generally affect the level of member loyalty through contextual conditions. The dependent variables are the choice between voice or nonvoice and between exit or nonexit. Although loyalty is not independently operationalized in the quantitative analysis, it is assumed to affect each dependent variable via the organizational factors. The model of the quantitative analysis is presented in the Appendix.

THE LIBERAL DEMOCRATIC PARTY SPLIT IN 1993

When the LDP split in 1993, it single-handedly initiated a major change: the demise of a predominant

[5] Recently, Hirschman has posited a variety of exit-voice relationships, especially one in which "enlarging the opportunity for exit can on occasion make for *more* rather than less participation and voice," based on the communist regime's collapse in the German Democratic Republic in 1989 (Hirschman 1993, 177). He convincingly shows that this mechanism led to the demise of the regime. The above point made by Hirschman is distinct from the discussion here, however. He looks at the concurrence of exit by self-interested citizens and voice by loyalists who stayed in the country; I emphasize that individuals who exercise voice are more likely to resort to exit.

FIGURE 1. A Revised Hirschman Framework



party system. Aside from its importance for Japanese politics, this case is an interesting example of how intraparty dynamics can affect the party system.

From 1955 to the early 1990s, Japan was dominated by the conservative LDP (Pempel 1990, 4). It substantially controlled the cabinet and both houses of the Diet from 1955 to 1989. When the LDP failed to win a majority of seats in the House of Councilors in 1989, this proved to be a temporary setback, as its popularity was restored in the general elections the next year. The sudden breakup in 1993 was not due to the interfactional rivalry for which the LDP was famous but to defections across (not along) factional lines. The split also caused the LDP to lose its majority in the House of Representatives. Although the LDP had come under public criticism because of a newly disclosed political scandal, opinion polls did not indicate a decline in support.[6] In other words, internal forces split the LDP, not a sharp drop in popularity or any real challenge from other parties.

Since the late 1970s, the LDP had institutionalized five factions as a system of command and control. Another division occurred in December 1992, when the largest of these, the Takeshita faction, split into two groups. The political influence of two previous faction leaders, Noboru Takeshita and Shin Kanemaru, had declined because of their implication in political scandals. Hostility among seven leaders in the next generation divided the Takeshita (formerly Tanaka) faction into two groups (Obuchi and Hata) almost equal in size in terms of House of Representatives members.[7] The breakup of the largest faction weakened the system of intraparty control.

The dispute between the Obuchi and Hata groups, which originated with a power struggle over management of the former Takeshita faction, became entwined with the issue of political reforms. Since the end of the 1980s, when a bribery scandal related to securities transactions of the Recruit Company was disclosed, the LDP cabinets (under Kaifu and then Miyazawa) had tried to reform the election system and political finance, but they had failed mainly because of opposition from within the party.[8] When additional scandals were disclosed in the early 1990s, the public was critical of the incumbent LDP and demanded remedies. Opposition parties as well as many LDP members, especially those newer to the Diet, were eager to respond because they had fewer vested interests in the current system and were more sensitive to public criticism, since their electoral base was weaker than that of the more prominent LDP politicians. Many of these young members organized various groups across party lines to discuss and demand reforms.[9]

The Hata group, which had a weaker position than the Obuchi group in the Miyazawa cabinet,[10] capitalized on the situation, stating that the reforms were necessary to prevent political corruption and criticizing the slow progress. The Obuchi faction took a more cautious position. In June 1993, when a reform proposal failed, the Hata group joined opposition parties in a no-confidence vote against the Miyazawa cabinet. Subsequently, the Hata group left the LDP and formed the Japan Renewal Party (JRP: Shinseito). In addition, ten junior members from other LDP factions, also dissatisfied with the lack of political reforms, left to form another party, the Harbinger (Sakigake). Several other members also quit the LDP and remained unaffiliated or formed an independent legislative group.

Most of the defectors expressed dissatisfaction with the LDP by supporting the no-confidence motion (most of those who subsequently joined the JRP) or by being absent from the vote (most of those who subsequently joined the Harbinger). It is interesting that these two responses correspond to subsequent relationships with the LDP: the more hostile attitude of the JRP and the relatively compromising one of the Harbinger. Not only was voice exercised by most of those who eventually exited, but also the manner of expression continued to be reflected in their dealings with their former organization. The factors that influenced

[6] LDP support declined when the consumption tax was introduced in 1989, but monthly opinion polls by the *Jijitsushinsha* showed a recovery in the early 1990s. Immediately before the breakup the LDP maintained a level of 30–35%, which was almost the same as the level from 1986 to 1988, when the party controlled both houses by a large margin.

[7] The group headed by Keizo Obuchi and two other leaders extended its influence over former Takeshita faction members in both the House of Councilors and House of Representatives. The group headed by Tsutomu Hata and three other leaders claimed almost half the faction members in the House of Representatives.

[8] The electoral reform was expected to institute a system of single-member election districts and/or proportional representation; the financial reform was expected to tighten public control over contributions to political parties.

[9] Those familiar with Japanese politics may consider the membership of such groups an issue that should be addressed here, but it is not easy to determine the adherents. To avoid repercussions or to remain uncommitted, many junior members listed their names not only in these groups but also in the discussion groups organized by party leaders critical of reform.

[10] The Hata influence was weaker because it had less presence in the House of Councilors and because Seiroku Kajiyama, of the Obuchi faction, had obtained the key position of LDP secretary general.

LDP members' decision to stay or quit (exit) and the way they expressed their discontent with the party leadership (voice) will be the first focus of the qualitative analysis.

After the breakup, the LDP retained a plurality in the Diet but lost its majority in the House of Representatives. In the general elections in July 1993, following House of Representatives dissolution after the no-confidence vote, the LDP kept almost the same number of seats but could not restore the majority lost by the defections. Five former opposition parties,[11] along with the two LDP splinters (JRP and Harbinger), formed the first non-LDP cabinet, headed by Morihiro Hosokawa, who had launched the New Japan Party about a year earlier. The formation of the first non-LDP coalition government since 1955 would not have happened at this juncture if the LDP had maintained its unity.

Meanwhile, Japanese politics continued to revolve around the issue of political reform, especially changes in the general election system. The LDP experienced further exits as well as expressions of noncompliance with the party leadership until January 1994, when the Diet passed bills dealing with political reform. Exit and voice of newly elected or reelected LDP members during this period after the LDP lost dominance (August 1993 to January 1994) will be analyzed below.

In summary, the LDP was perceived to have deteriorated by certain members. They believed the party had become paralyzed by political corruption and collusion with special interest groups, and they advocated political reform. Although those who exercised voice supported strong reforms, others denied the need altogether or advocated very moderate change. The members most likely to resort to voice and exit were those most concerned about declining party reputation (a collective good), due to the LDP's failure to implement political reforms (a public good) demanded by the public.[12]

Data Sets and Variables

The quantitative analysis is based on two data sets. The first consists of behavioral, personal, and organizational characteristics of LDP House of Representatives members in June 1993. The second was obtained from the party's House of Representatives members in January 1994, immediately after passage of political reforms.[13]

Dependent Variables. An LDP member's choices about whether to exit and whether to exercise voice are the dependent variables. In the 1993 group, these are labeled, *Voice93* and *Exit93*, which represent members' voice/nonvoice and exit/nonexit, respectively. The probit model gives the value of 1 or 0 (dummy variable) to such binary data. Specific scoring rules for the dependent variables are as follows.

Voice93 = 1 when a member of the LDP was either absent from the no-confidence vote on the LDP cabinet or voted for the no-confidence motion (noncompliance with the party).[14]

Voice93 = 0 when a member of the LDP voted against the no-confidence motion (compliance with the party).

Exit93 = 1 when a member quit the party after passage of the motion in June 1993 (exit).

Exit93 = 0 when a member stayed in the party after June 1993 (nonexit).

In the 1994 groups, the two dependent variables are *Voice94* and *Exit94*, which represent the following values and behavior.

Voice94 = 1 when a member of the LDP showed noncompliance with the party from August 1993 to January 1994.[15]

Voice94 = 0 when a member complied with the LDP during that time.

Exit94 = 1 when a member of the LDP quit the party between August 1993 and January 1994.

Exit94 = 0 when a member stayed in the party during that time.

Independent Variables. There are nine independent variables. Seven are individual factors that refer to personal characteristics of LDP members or their districts. Two are organizational factors.

Among the individual factors is *Terms*, which expresses the number of legislative terms served. Senior members have more vested interest in the party and are more likely to resist change, whereas junior members of the party are more apt to use voice and exit to bring about changes. The expected sign of the coefficient of terms for both dependent variables (voice and exit) is negative. *Urbanization* shows the extent of urbanization in a member's election district.[16] A less organized

[11] The Social Democratic Party of Japan (SDPJ), Clean Government Party (CGP), Democratic Socialist Party (DSP), Federation of Democratic Socialists (FDP), and New Japan Party (NJP).

[12] Some may question whether all defectors, especially the experienced members of the former Takeshita faction, were idealistically motivated. The key point is that political reforms were at least consistent with their interests, even if some defectors used the issue to better their own political position by currying support from voters.

[13] For consistency, the 1993 data set includes House of Representatives members who were elected in 1990 as LDP candidates and remained party members until the no-confidence vote in June 1993, and the 1994 set includes those who were elected in 1993 as LDP candidates. There is much overlap between the two periods, but a substantial number quit the party, and new members were elected in the general elections held during the interval.

[14] No distinction is made in the analysis between absentees and noncompliers. It is true that a no-confidence vote expressed stronger criticism of the party than did absence, but a division into these two categories would yield so few observations that the probit analysis could be distorted. I apply the same rule to the categorization of the other dependent variables.

[15] Noncompliance (Voice94 = 1) is defined as a member who (1) was absent from the prime minister's election without supporting the LDP candidate in August 1993; (2) left a faction; (3) was absent or voted for the non-LDP government's political reform bill in November 1993; or (4) was absent or voted against the political reform bill proposed by the LDP in November 1993.

[16] The categorization is based on the index provided by a Japanese newspaper (*Asahi Shimbun*), which measures "population concentration" by the percentage (P) of the electorate living in residential, industrial, and commercial areas in each election district. According to P, 130 election districts are divided into four categories. (1) If $P \geq$

urban electorate is expected to base its voting on a politicized issue, such as political reform. Thus, LDP members from urban districts are likely to be more sensitive to the electorate's concerns. The expected sign of the coefficient is positive. *Electoral Strength* indicates how easily the LDP member won in the most recent election.[17] Because a stronger candidate is expected to be less dependent upon the party in elections, and thus less compliant, the expected sign of the coefficient is positive. *Assets* measures the personal property of a member.[18] The expected sign of the coefficient is positive, since a member with substantial wealth is more likely to resist the party.

Three dummy variables are included among the individual factors. *Relatives* takes the value of 1 for a member with family relations currently or previously in the Diet, 0 otherwise. The expected sign of the coefficient is negative, since family connections to the LDP are likely to increase a member's commitment to the party. *Local Politics* is a dummy variable that takes the value of 1 when a member is a former local politician, 0 otherwise. The expected sign of the coefficient is positive, since a local career implies strong support in one's election district and hence less dependence on the party. *Bureaucrats* is a dummy variable that takes the value of 1 when a member is a former bureaucrat, 0 otherwise. The expected sign of the coefficient is negative, because the propensity to support the party's rule should be relatively stronger among those who worked in LDP governments prior to becoming politicians.

There are two organizational factors. *Faction Size* refers to the number of people in the Diet who belong to a member's faction.[19] Members of a large faction are more likely to criticize or quit the party. Indeed, the split in the Takeshita faction triggered voice and exit through the LDP. Thus, the expected sign of the coefficient is positive. *Nonfaction Dummy* takes the value of 1 when a member does not belong to a faction, 0 otherwise.

85, then the category is big city. Thirty-five districts with 145 seats fall in this category, and the urbanization index score is 4. (2) If $P \geq 65$, then the category is city. Thirty-seven districts with 148 seats fall in this category, and the urbanization score is 3. (3) If $P \geq 50$, then the category is local city. Forty-three districts with 175 seats fall in this category, and the urbanization score is 2. (4) If $P < 50$, then the category is noncity. Fifteen election districts with 44 seats fall in this category and are scored 1.

[17] This index is obtained when the votes received by the winner are divided by the votes of the candidate who finished first among the losers.

[18] The data are published occasionally by the Ministry of Internal Affairs (especially for new members after a general election). The unit is ¥10 million (=$100,000; assuming $1 = ¥100 in 1993 and 1994).

[19] Diet membership in 1993 and in 1994 was used to create the index for each data set. The following numbers give the 1993 figure first, then the 1994 figure: Takeshita faction (66, 31), Miyazawa faction (60, 55), Mitsuzuka faction (58, 56), Watanabe faction (45, 52), and Komoto faction (24, 21).

Analysis of LDP Case Results

Results of the probit analysis of exit and voice among LDP members in 1993 and voice in 1994 are summarized in Table 1.

Exit and Voice in 1993. In Table 1, equation 1 involves all LDP members in 1993. The result shows that two variables, terms and faction size, are the two most important determinants of behavior. It is possible that the Takeshita faction, because of its size, swamps the all-LDP data. That is, the causes of exit and voice in the Takeshita faction, by their dominance, may conceal the influence of other factors on the behavior of other LDP members. Thus, all LDP members and the Takeshita faction will be analyzed separately.

There is a clear contrast between exit and voice within the Takeshita faction (equation 2) and among other party members (equations 3 and 4). The dependent variable of equation 2 can be defined either as voice and exit (because the Hata group of the Takeshita faction either voted for the no-confidence motion or was absent and then subsequently quit the party) or as neither voice nor exit (by all other Takeshita faction members). Among the remaining LDP members, there was no such clear demarcation, and thus exit and voice are regarded as distinct dependent variables in equations 3 and 4. The coefficient of terms has a negative sign and is statistically significant in equations 2 and 4. This concurs with the observation that the LDP split in 1993 was caused by newer legislators (one or two terms). The coefficient of local politics has a positive sign and is statistically significant in equations 3 and 4. Although urbanization is statistically significant in all these equations, its sign is positive in equation 2 and negative in equations 3 and 4.

Let me first interpret the coefficient of urbanization. Close examination of the individual data reveals that many in the Takeshita faction who were serving their first or second term at this point were elected from nonrural areas. Certainly, junior members from urban districts were overrepresented.[20] Because almost all the junior members in the faction joined the Hata group and then the JRP, the positive and statistically significant sign for urbanization in equation 2 is likely to be influenced by this coincidence.[21] The negative signs in equations 3 and 4 require more speculation. At the time of the LDP split, the election system and political finance reforms began to be politicized, but the members could not judge whether public sentiment for reforms was sufficient to affect electoral support. It is possible that members from rural districts, where electoral support is generally more stable and organized than in urban areas, felt they could take the risk of supporting reforms.

If exit and voice by nonmembers of the Takeshita

[20] Almost 70% of one- or two-term members in the Takeshita faction were elected from big-city or city election districts. Among the entire faction, about half came from nonrural districts.

[21] Junior members representing predominantly urban interests may have formed a group, which could have reinforced their support for political reform, but there is no empirical evidence of this.

TABLE 1. Probit Regression Estimates for Selected Groups of LDP Members

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Political Group or Subgroup						
	All LDP Members (N = 272)	Takeshita Faction of LDP (N = 66)	All LDP members except Takeshita Faction (N = 206)		LDP Members (Non-Takeshita) with 1–2 Terms (N = 76)	LDP Members (Non-Takeshita) with More than 3 Terms (N = 130)	All LDP Members (N = 226)
	Dependent Variable						
	Voice93	Exit93 (Voice 93)[a]	Exit93	Voice93	Voice93	Voice93	Voice94
Constant β_0	−1.8044	1.01215	−2.3995	−0.3853	5.3232	−5.3114	−1.0882
	(−3.6822**)	(1.8354*)	(−3.1054**)	(−0.6553)	(2.4615**)	(−2.4147**)	(−1.7090*)
Terms	−0.0981	−0.1541	−0.0476	−0.1153	−1.7612	0.1227	−0.0839
	(−2.8113**)	(−2.3955*)	(−0.8946)	(−2.3083**)	(−2.7427**)	(−1.325)	(−2.0996*)
Urbanization	−0.2249	0.9607	−0.9907	−1.4572	−1.4839	−4.0834	0.5914
	(−0.9644)	(1.7460*)	(−1.8564*)	(−2.6226**)	(−1.7138*)	(−0.1253)	(1.9313*)
Electoral strength	0.0681	0.0099	0.2169	0.2822	−0.9037	0.3994	0.2749
	(−0.547)	(−0.0512)	(−1.138)	(−1.575)	(−0.7865)	(−1.5741)	(1.7468*)
Assets	0.0225	−0.0035	−0.1285	0.0572	0.1132	−0.0153	0.0443
	(−0.9807)	(−0.0615)	(−0.6554)	(−1.6188)	(−0.8400)	(−0.0873)	(−1.4665)
Relatives	0.0933	−0.3655	0.5499	0.8122	0.4983	1.3937	0.1966
	(−0.4338)	(−0.8846)	(−1.6496)	(2.3530**)	(−0.6953)	(1.9638*)	(−0.6601)
Local politics	0.1516	−0.3496	0.6087	0.6975	0.8740	0.5084	−0.0965
	(−0.6553)	(−0.8502)	(1.7223*)	(1.8784*)	(−1.1120)	(−0.6912)	(−0.2822)
Bureaucrats	0.0282	−0.0544	−0.1042	−0.0589	−0.1560	−0.7661	−0.0729
	(−0.1142)	(−0.1042)	(−0.2737)	(−0.1470)	(−0.2106)	(−0.8469)	(−0.2100)
Faction size	0.0226	—	0.0151	−0.0259	−0.0673	0.0283	−0.0124
	(2.8544**)	—	(−1.1280)	(−2.4258**)	(−3.2375**)	(−0.7523)	(−1.1478)
Nonfaction dummy	−2.4291	—	−2.2747	−4.4551	−7.4666	−1.5696	0.3784
	(−0.0788)	—	(−0.0749)	(−0.1529)	(−0.1777)	(−0.0314)	(−0.6047)
χ^2 (probability)	26.752	16.093	16.757	30.414	33.244	17.407	20.449
	0.0015	0.0242	0.0526	0.0003	0.0001	0.0427	0.0153

Note: T-values appear in parentheses below estimated coefficients. $^*p \leq .05$, $^{**}p \leq .01$, one tailed tests.
[a]The dependent variable of equation 2 can be defined either as voice and exit or as neither voice nor exit by all Takeshita faction members.

faction are regarded as risk-taking behaviors, then the positive signs for local politics in equations 3 and 4 can be interpreted in the same way as urbanization. That is, a strong local base makes a member less dependent on the party and potentially less compliant. The same reasoning may explain the unexpected positive sign for relatives in equation 4. Family connections in the political world, rather than reinforcing compliance with the party, apparently contribute to independence by providing "inherited" electoral support and political resources.

The negative and statistically significant coefficient for faction size in equation 4 also merits attention. When the Takeshita faction is excluded, members of other relatively large factions are revealed to be more important. This refutes the contention in the previous section that larger factions have difficulty imposing party discipline, but more detailed examination is necessary before reaching this conclusion. The negative sign in equation 4 contrasts sharply with the positive sign in equation 1. If the Takeshita faction is included, the coefficient has the expected sign (equation 1). This result can be explained by considering the relative size of each faction and the voice incentives unrelated to faction size.

In 1993, the Takeshita faction was the largest in the LDP, followed by three of medium size (Mitsuzuka, Miyazawa, and Watanabe), and then the small Komoto faction. Setting aside the Takeshita faction, if faction size decreases compliance, then members of Komoto would be much less likely to exercise voice and/or exit than members of the other three, which were about double in size. Yet, among twenty-four Komoto members, seven were absent from the no-confidence vote (voice), and two quit the party (exit). This high proportion of noncompliants in the smallest faction is

consistent with the unexpected sign for faction size in equation 4. The behavior is attributed to a general sentiment for reforms within the group, which was inherited from the former faction head, the late Takeo Miki, who had advocated the necessity for remedies for political corruption and led political finance reforms in the 1970s. Thus, the current Komoto leaders, who had been influenced by Miki, were more lenient toward members who exercised voice and exit. Five of the absentees remained in the party without punishment, and two joined the Harbinger. These characteristics of the Komoto faction also may explain the statistically significant negative sign for faction size in equation 5.[22]

Equations 5 and 6 enable comparison of voice between newer and more senior LDP (non-Takeshita faction) members.[23] The terms variable has a statistically significant and negative coefficient in equation 5 but does not appear to be a meaningful determinant in equation 6. This implies that length of service was not a special influence on voice among senior members, whereas it was a factor among newer members. In other words, those with longer service tended to toe the line, while relative newcomers were less disciplined. The urbanization coefficient is statistically significant only in equation 5, and the coefficient for relatives is significant only in equation 6. This indicates that the effect of these factors in equation 4 is derived from different subgroups of the entire sample. Generally speaking, it is difficult to characterize those who exercised voice among the members with more than three terms.

Comparison of Voice in 1993 and 1994. Equation 7 in Table 1 concerns voice among LDP members in 1994.[24] Whereas the effect of terms remains the same as in 1993,[25] the coefficient of urbanization becomes positive, and the positive coefficient of electoral strength becomes statistically significant. Both changes can be interpreted as follows. Intensive politicization of the reform issue encouraged members from urban districts to resist the party and impress voters with their eagerness for reform; urban constituencies were less partisan and had keener interest in the issue than did rural voters. Members also became more conscious of their electoral strength. The institution of small districts and proportional representation was anticipated, so members who had been weak in previous elections were less likely to defy the party. They would have to compete with colleagues for party nominations in small districts and/or for a priority spot on a party list in proportional representation. Both the assets and bureaucrats variables are statistically insignificant in all models. Although the neutral influence of the latter can be taken at face value, it is possible that the voluntary disclosure of assets by legislators may not be completely reliable; thus, assets continues to be statistically insignificant because of poor measurement of the variables.

In terms of the distinction between individual and organization factors and their relationship to exit and voice, the results of the quantitative analysis suggest the following. Although an organizational factor such as faction size has a limited influence, various individual factors consistently explain members' behavior. An important finding concerns the strategic use of voice. In the case of the Takeshita faction in 1993, those who used voice all subsequently exited to join the JRP (hence the dependent variable is written either as voice or exit in equation 2). Similar use of voice also occurred among the LDP members who joined the Harbinger, although instead of voting for the no-confidence motion they were absent from the voting. Both these acts defied the LDP executive and thus can be regarded as the exercise of voice, but their strategic effects were different. Whereas a no-confidence vote was a decisive step toward leaving the LDP, absence expressed resistance to the executive but left room for compromise in the future. This contrast in behavior corresponds to the different approaches of the two splinter parties formed by the defectors. The JRP, whose members had voted for the motion, continued its confrontation with the LDP for several years, whereas the Harbinger, whose members were absent from the vote, allied with the LDP after about a year.

The LDP returned to office in June 1994 by forming a coalition with the SDPJ and Harbinger. This caused a change in LDP members' behavior and requires a different analytical model from the one presented here.

DISCORD AMONG THE SOCIALISTS: 1994 TO 1995

A good case for comparison with the LDP split in 1993 is the discord within the SDPJ from 1994 to 1995. The friction was exacerbated after the coalition with the LDP in 1994, but it failed to develop into the split that had been expected before the House of Councilors elections in July 1995. To understand why discord intensified within the SDPJ, one needs to examine the last three months of the Hosokawa cabinet, the point at which the empirical analysis of the LDP ended in the previous section.

The first non-LDP coalition government, headed by Hosokawa, implemented reforms of the election system and political finance at the beginning of 1994. Immediately afterward, the issue of tax reform emerged. The prime minister, who had strengthened his connection with coalition parties, the JRP and the Clean Government Party (CGP), abruptly proposed the people's welfare tax (*kokumin fukushizei*), which would increase the rate of the existing consumption tax. The proposal caused friction among the coalition parties. The SDPJ and Harbinger especially opposed

[22] It is possible that the Komoto leaders even welcomed insubordination to facilitate political reforms, although they did not desire the LDP breakup. To avoid distortions caused by too many dummy variables, it was not possible to operationalize this kind of faction characteristic in the quantitative analysis.

[23] The probit analysis of the exits of each group does not produce meaningful results (perhaps because of the relatively small number of exits in the sample), and those results are not presented here.

[24] At this point, several members quit the party, but the probit analysis of exit does not produce a meaningful result (perhaps because of the relatively small number of defectors in the sample), so only voice is analyzed here.

[25] The members who were in their first or second term in the 1993 data set were in their second or third term in the 1994 data set.

the idea, not only because they were cautious about the tax increase but also because Hosokawa had failed to consult the leaders of the two parties about his proposal.

Although the plan was immediately withdrawn because of public opposition, the antagonism within the coalition government remained. When the SDPJ and Harbinger walked out, the remaining parties had to form a minority government, in May 1994, headed by Tsutomu Hata. At the end of June, the Hata cabinet resigned, and all the parties sought coalition partners with whom to form a government. The result was an alliance of the LDP and SDPJ, with Harbinger as broker. Aside from Harbinger's effort, contributing factors were the LDP's political entrepreneurship and deep distrust by the SDPJ's left wing of the other non-LDP coalition parties, especially the JRP and the CGP. The left-wing Social Democrats also opposed the party's right wing, which was eager to modify its leftist positions and strengthen the non-LDP coalition. After the SDPJ dropped out of the Hosokawa government, the LDP capitalized on this disharmony. It attempted to win over the SDPJ's left wing by offering the position of prime minister to its leader, Tomi'ichi Murayama. This left-wing "pragmatist" became head of the LDP-SDPJ-Harbinger coalition government.[26]

Phase 1: September–November 1994

The arrangement of the Murayama cabinet was destined to intensify conflict within the SDPJ. Soon after cabinet formation in July 1994, it became apparent that the Socialists had divided into two camps. At the end of August 1994, the Democrats, a right-wing group of the SDPJ, recruited additional members from the center and right and formed the New Democratic League (NDL: Shinminren), which claimed more than 60 Diet members. It sought to resume the alliance with the DSP, the JNP, and other liberal groups outside the party. Among the other SDPJ Diet members, more than 50 active members of the left-to-center wing formed a loose group, termed here the Murayama Support Group (MSG).

At the SDPJ annual meeting held on September 3, 1994, the party formally decided on major shifts in policy orientation: the constitutionality of keeping a self-defense force and the official status of a national song and a national flag, both of which dated back to imperial Japan. The resulting conflict became more complicated because neither group fully welcomed the change. The center-to-right wing wanted a reversal of leftist policies but not the LDP alliance, preferring instead a coalition with the non-LDP parties and formation of a new liberal party. In contrast, the center-to-left wing reluctantly agreed to the shift in order to secure the top post for their leader and to help the party survive by securing governing power through alliance with the LDP. More important, the SDPJ at the local level opposed the national party's unexpected coalition and the policy reversals. In this sense, there was a large gap in perception between the national and local levels. At the national level, the members supported either the Murayama cabinet or the policy changes, whereas those at the local level opposed both.[27]

After the September meeting, the NDL faction began to argue for disbanding the SDPJ in January 1995 and creating a new party. This not only would end the Murayama cabinet but also would make the left wing a small remnant party. Meanwhile, the national umbrella organization of labor unions, Rengo, became involved. It had exercised significant influence in formation of the non-LDP coalition and subsequent Hosokawa cabinet in 1993, which had changed the image of labor as a weak force in Japanese politics. But now the unions were divided into those who favored a new party and those who did not.[28]

On November 26, the NDL held a symposium to express its resolution to form a new party and invited several members of Harbinger, some liberals in the Diet, and union supporters. Thirty-seven SDPJ members in the House of Representatives publicly supported this symposium and were regarded as NDL core members. To examine what factors divided them from the other Socialists, a quantitative analysis of the SDPJ was conducted, which will be described later.

Phase 2: November 1994–January 1995

The NDL symposium was not a great success. Deep skepticism about the NDL's resoluteness encouraged the group members to compromise with others in the SDPJ and agree to shelve the proposal. Yet, many Socialists, including non-NDL members, admitted the need to revitalize the party in the face of declining popular support. Thus, Prime Minister Murayama and others in the party accepted the NDL's suggestion of a provisional meeting in January 1995, on the condition that it would not lead to an immediate dissolution and formation of a new party. After the November symposium, the prime minister and SDPJ secretary-general Kubo made this agreement, which marked a compromise between the two camps.[29] It was certainly facilitated by a desire to maintain the SDPJ's position in the coalition government; both the LDP and Harbinger were concerned about their ally's internal dispute and the inflexibility of its traditional policy position.

[26] Austen-Smith and Banks (1988) provide a formal case in which two extreme parties may form a coalition government.

[27] At the meeting, another conflict emerged over two important party posts. Both camps called for votes to determine the deputy chair and the chair of the Party Diet Management Committee, both of which were usually determined by negotiation. The right wing won both by a small margin.

[28] For example, the NDL was supported by four major unions: the Japan Postal Workers' Union (Zentei), the Japanese Federation of Iron and Steel Workers' Unions (Tekkororen), the Federation of Telecommunications (Zendentsu), and the Japanese Federation of Electrical Machinists Unions (Denkirenngo). The All Japan Prefectural and Municipal Workers' Union (Jichiro), who had a strong tie to the prime minister, sided with the Murayama Support Group.

[29] Because the SDPJ secretary-general, Wataru Kubo, had a close relationship with the NDL, he negotiated with Prime Minister Murayama, who was also a party chair.

The compromise did not last long. When party executives decided on December 22 to postpone the provisional meeting from January to February, the radicals in the NDL regarded this as breaking a promise. On January 17, 1995, seventeen NDL members of the House of Representatives and seven from the House of Councilors, headed by Sadao Yamabana, gave notice to the SDPJ that they would form a new legislative group in the Diet and subsequently secede from the party. To examine the factors that distinguish this group from the other SDPJ members in January 1995, a quantitative analysis will compare data at this time with data on the NDL symposium in the previous November.

Phase 3: The Demise of the New-Party Movement

January 17, 1995, was also the day on which Japan was hit by the largest earthquake of the postwar era. The secession of the NDL radicals was postponed so that the coalition government could concentrate on disaster relief. At the beginning of March, the defectors resumed plans, and Yamabana decided the group would secede in April, but there was an unexpected shock when the labor unions began to withdraw their support.[30] In May 1995, Yamabana, four colleagues of the House of Representatives, and two from the House of Councilors tried to secede from the SDPJ, but party executives accepted only their succession from a legislative group of the party in the Diet. The SDPJ neither experienced a major split nor became a new party before the elections of the House of Councilors in July 1995. The analysis ends here, although internal discord continued, and the final breakup of the SDPJ in 1996 may be regarded as an extension of voice originally activated by the NDL. Nevertheless, the formation of the Democratic Party, which triggered the breakup, was initiated from outside the SDPJ, as will be explained in the concluding section.

Before presenting the quantitative analysis, I will briefly interpret the NDL members' attempt to recreate the SDPJ. The group members wanted a new liberal party (i.e., a public good) that would include SDPJ members as well as members of other parties. They viewed this as serving the long-term interests of the SDPJ, whose popularity was declining. The NDL members actively used voice to win other Socialists to the idea. Nevertheless, when the NDL members demanded the immediate formation of a new party, they were viewed as a threat to party unity, which would further weaken the Socialists' position.

[30] Zentei first revoked its support for Yamabana's new party. Because the rank-and-file in Japanese labor unions is not well organized, decision making is dominated by the leadership, especially regarding politics. But the delay in forming a new party allowed time for pressure from below to persuade leaders to withdraw backing.

THE DATA AND VARIABLES FOR THE SDPJ CASE

The two quantitative analyses of the SDPJ will use the same data set, the Socialist House of Representatives members from November 1994 to January 1995.

Dependent Variables

The SDPJ members' decisions to use voice are the dependent variables. The NDL's attempt to change the SDPJ and form a new party is defined as an exercise of voice that failed to develop into exit. The two dependent variables are *Voice94* and *Voice95*, each of which refers to voice/nonvoice. The probit model gives the value of 1 or 0 (dummy variable) to such binary data. The values of the dependent variables are as follows.

Voice94 = 1 when the name of the SDPJ member appears on the list of coorganizers of the NDL symposium in November 1994, otherwise 0.[31]

Voice95 = 1 when a member of the SDPJ had intended to secede from the party in January 1995, otherwise 0.

Independent Variables

Among the six independent variables, three are individual factors and three are organizational factors. Several variables differ from the LDP case because the same ones are not relevant. For example, very few SDPJ members were former bureaucrats. The most important difference was terms, since the SDPJ's conflict was not intergenerational.

Among individual factors, *Conflicting Electoral Interest* is a dummy variable indicating a possible election contest between an SDPJ member and a member of the LDP or Harbinger. Specifically, if the SDPJ member wanted to be a candidate in a single-member election district in which a coalition member(s) also wanted to be elected, the value of 1 is assigned, 0 otherwise.[32] The expected coefficient sign is positive; a member in a potential conflict with the coalition parties would be more likely to resist Murayama's leadership and to demand a new party. *Electoral Strength* is the same measure as in the LDP analysis, which indicates how easily a member won in the most recent election. The expected sign of the coefficient is positive, since a stronger candidate is expected to behave more independently of the party. *Assets* measures the personal assets of each member as explained in the LDP analysis. The expected coefficient sign is positive, because members with strong financial resources are less dependent on the party.

As for organizational factors, *Local Politics* is a

[31] I do not distinguish between those who attended the symposium and those who listed their name but were absent, since the aim is to identify the signatories.

[32] Since incumbents all wanted to run in new single-member election districts that overlapped as much as possible with their electoral base in the older medium-sized districts, there were many conflicts among them over the party nominations. The data are from *Asahi Shimbun*, January 1995.

dummy variable that takes the value of 1 when a member is a former local politician, 0 otherwise. The concept differs from the LDP case, in which this variable represented a local base of electoral support that could foster independence from the party (the expected coefficient sign was positive). In the SDPJ case, the variable expresses the extent to which a Diet member is sensitive to local interests. Given the local-national difference in attitudes among SDPJ adherents regarding a new party, Diet members who were former local politicians are expected to be more sensitive to opposition in their district to the new-party movement. Since Voice94 indicates support for the NDL's efforts in this direction, the expected coefficient sign is negative. *Labor Support* is a dummy variable that takes the value of 1 when a member had the support of labor unions that favored the formation of a new party, 0 otherwise. The expected coefficient sign is positive. Finally, *Aichi-Hyogo* is a dummy variable that takes the value of 1 if a member came from an election district in Aichi or Hyogo prefectures, 0 otherwise. Because two prominent NDL leaders from these prefectures supported the new-party movement, their personal influence would most likely encourage Diet members from these areas to do the same. Thus, the expected sign of the coefficient is positive.

RESULTS AND INTERPRETATION OF THE SDPJ CASE

The results of the probit analysis of voice among SDPJ members in 1994 and 1995 are summarized in Table 2.

Factors That Led to Exercise of Voice

Table 2 presents the results of two different probit regressions. Equation 1 relates to the NDL symposium in 1994. The results show that three variables—local politics, labor support, and Aichi-Hyogo—were statistically significant determinants in the decision to list one's name among coorganizers of the symposium. It is interesting that individual factors had little influence, and there are two plausible reasons. First, since the final decision had been made for the reform of the general election system, that was not a major political issue during the period analyzed, in sharp contrast to the LDP case. Second, and more important, because the elections under the new system had not yet been scheduled, there was great uncertainty for SDPJ members about which decision would increase their electoral advantage.

Local politics has the expected negative coefficient and is statistically significant. This shows that the antipathy of local branches toward the new-party movement was reflected in the behavior of the SDPJ members who were former local politicians. Coefficients of the other organizational factors, labor support and Aichi-Hyogo, are positive and statistically significant. This indicates that strong ties with the labor unions that favored the new party and the influence of NDL leaders in specific prefectures were important for predicting members' behavior.

TABLE 2. Probit Regression Estimates for SDPJ Members

	(1) All SDPJ Members (N = 67)	(2) All SDPJ Members (N = 67)
	Dependent Variable	
	Voice94	Voice95
Constant β_0	−0.7720 (−0.9036)	−3.1124 (−2.7775**)
Conflicting electoral interest	0.0692 (−0.2886)	−0.5568 (−1.2226)
Electoral strength	0.6206 (−0.9782)	1.5238 (1.9197*)
Assets	0.0121 (−0.3301)	−0.0307 (−0.5703)
Local politics	−0.9836 (−2.6041**)	−0.4943 (−0.8700)
Labor support	1.6398 (2.8156**)	2.2516 (3.3994**)
Aichi-Hyogo	1.3385 (2.2675*)	3.3258 (4.1973**)
χ^2 (probability)	22.926 0.0008	41.282 0.0000

Note: T-values appear in parentheses below estimated coefficients. $^*p \leq .05$, $^{**}p \leq .01$, one-tailed tests.

The results of the analysis for January 1995, when seventeen NDL members were about to quit, differ in one important respect from November 1994 results. Although labor support and Aichi-Hyogo have positive signs and are statistically significant, local politics has a negative sign but is no longer statistically significant. Thus, only two organizational factors influenced the behavior of members in 1995.

A Time Bomb That Never Exploded

Although the LDP case exemplifies the effectiveness of voice when closely tied with the threat of exit, the SDPJ case illustrates that the threat of exit can lose credibility. Did the great earthquake affect the outcome? The results of the quantitative analyses suggest not; the members' motivations to exercise voice in 1994 differed from those in 1995. When the NDL started its activities, there appeared to be a clear distinction in the vision of the party between those who exercised voice and others. Yet, as the description of the process has shown, their difference of opinion came to be centered on a tactical matter—when and how the SDPJ would metamorphose. This shift is explained by an increasing desire inside the SDPJ, especially among local branches, to renew the party. The national-local cleavage embedded in the conflict at the beginning had disappeared during the process. More local branches came to support party renewal after the NDL sym-

posium[33] The change in their attitude corresponds to the results of the quantitative analysis showing statistical significance for local politics in 1994 but not in 1995.

Ironically, the voice demanding a new party lost its legitimacy as the idea of party renewal came to be accepted. The distinction remained between those who demanded the immediate formation of a new party and those who saw it as a more remote possibility. The ties to specific labor unions and to the new prefectures from which the strong NDL leaders came were the factors that distinguished the radical NDL members from other faction members as well as from other Socialists. NDL members without strong ties to unions and/or prefectures did not try to quit the SDPJ in January 1995. Because of the quake, the radical NDL members could not leave the party on the day they had planned. Three months later, waning union support ensured the demise of the new-party movement.

This SDPJ case result also has an interesting implication for the distinction between individual and organizational factors. The latter contribute to the activation of voice, but they do not guarantee that it will occur. In the case of the SDPJ, voice neither reformed the organization nor evolved into exit. Those who had exercised voice failed to redefine their goal in such a way that the formation of a new party came about. This process contrasts clearly with the LDP case, in which individual factors were more important in the activation of voice and exit.

CONCLUSION

The analysis of the two Japanese cases shows that exit and voice by party members can be predicted by personal characteristics and organizational conditions. I conclude by comparing the two cases and examining the implications, which confirm the central argument of this article, that is, the successive activation of exit and voice by the same individuals. Both the LDP and the SDPJ experienced intense internal discord almost simultaneously, but the consequences were quite different. The LDP broke up at an early stage, whereas the unity of the SDPJ continued until 1996, despite two major electoral defeats (the House of Representatives election in 1993 and the House of Councilors election in 1995) and a decline of support in public opinion polls. What factors explain the different consequences, and what implications can be drawn?

First, we need to pay attention to the public good, the goal of those who exercised voice in both cases. In the LDP case, electoral and political finance reforms were considered public goods by those dissatisfied with the party. Their aim was clear—change the election and political finance systems—and the LDP members were divided into supporters and opponents of such reforms. These two groups had distinct personal characteristics. In comparison, the definition of the public good was not very clear in the SDPJ case. At the beginning, the issue appeared to be whether a new party should be formed to revitalize the party. Later, when everyone recognized the need to revitalize, the issue shifted to what kind of action at what specific time was appropriate to achieve party renewal.

Second, although the factors that reflect individual electoral conditions (political career and economic and political resources) were dominant influences on the choice of LDP members, organizational factors appeared more influential in decisions of the SDPJ members. In the LDP case, faction affiliation (including faction size) had a limited effect. But in the SDPJ case, strong ties with specific intraparty organizations or outside supporters were critical determinants of behavior. Organizational factors may strongly constrain member behavior, but at the same time their influence may be modified by contextual changes. Interaction between organizational factors and contextual changes are important in understanding why the activation of voice failed to develop into exit in the SDPJ case in 1995.

The final breakup of the SDPJ in 1996 is consistent with the arguments presented here, though it took place after the period of analysis. The LDP-SDPJ-Harbinger coalition formed a new cabinet, headed by Ryutaro Hashimoto of the LDP, at the beginning of 1996. SDPJ executives sought to merge with Harbinger and potential defectors from the New Frontier Party (which was formed by non-LDP coalition parties) into a unified center-left party, while the NDL initiative waned. In September 1996, when it was certain that general elections under the new system would take place the following month, Yukio Hatoyama of Harbinger and Kunio Hatoyama of the New Frontier Party (brothers from a well-established political family) decided to form a new center-liberal party but refused to accept the entire SDPJ. At this point, the SDPJ split into a rump group that retained the name and a group that joined with the new Democratic Party. Sixteen former SDPJ members joined the new party, all except three of whom were former NDL members. All the House of Representative members who had been involved in the NDL movement eventually quit the SDPJ and competed in general elections under the flag of the Democratic Party. The labor unions supported that party and kept their distance from the SDPJ.[34]

The SDPJ case reveals two different relationships between the behavior of party members and labor unions that give electoral support to the members. In 1994–95, legislators committed to the new-party movement because labor unions supported it; in 1996, labor unions supported the new Democratic Party because

[33] There is evidence that local branches increasingly perceived the need for a new party. For example, in inquiries conducted between November 26 and 30 by *Asahi Shimbun*, many local branches of the SDPJ expressed a cautious attitude about the formation of a new party. Among 47 surveyed, 17 opposed the idea, and 25 supported it. A few weeks later, at a meeting of local representatives of the SDPJ on December 18, 33 branches favored it, and only 3 explictly opposed it (*Asahi Shimbun*, December 19, 1994).

[34] The local branches of the SDPJ were divided into a group supporting the SDPJ candidates and one supporting the DP candidates.

the legislators with whom they had strong ties had already moved to that party. This difference implies that the formation of the new party by politicians outside the SDPJ shifted the relative strength of influence from the labor unions to the SDPJ members, and ultimately contributed to the breakup of the SDPJ in 1996. In this sense, the formation of the Democratic Party may be regarded not only as an attractive alternative for potential SDPJ defectors but also as a factor that changed an important organizational element, the leverage of the labor unions over the legislative members. This change caused by the formation of the Democratic Party demonstrates that organizational dynamics cannot be reduced to the rational calculation of costs and benefits expected from personal characteristics. The constraints on individual members may be different if the context changes. More specifically, the same organizational connection (such as the tie with the labor unions) may have a different effect on individual behavior if the context alters (such as formation of the Democratic Party).

The breakup of the SDPJ also is interesting in terms of individual factors. Eventually, 27 former SDPJ members sought reelection as members of the Democratic Party in the October 1996 elections, that is, about a month after the formation of the party. Only five were reelected, an even worse showing than that of their former colleagues who remained in the SDPJ. This is indirect but convincing evidence that many SDPJ members' decisions to affiliate with the new party were not motivated by short-term electoral interests.[35] Just as in 1994–95, individual factors did not determine members' participation in the new-party movement in 1996.

Finally, the activation of voice and exit by the same individuals is confirmed in the case of the SDPJ. Because many defectors came from the NDL faction, the breakup of the SDPJ was regarded as an outgrowth of the new-party movement. The latent potential for a party split was there, triggered from outside the party. Although the use of voice was immediately followed by exit in the LDP case, there was some time lag in the SDPJ case.

Empirical evidence from Japan points to the successive activation of exit and voice by the same individuals in a political organization, which is contradictory to Hirschman's model. The members whose threat of exit is credible are also able to exercise voice effectively, but whether they resort first to exit (without voice) or first to voice within (and then possibly outside) the organization is influenced by contextual conditions. In conclusion, the modified framework of exit, voice, and loyalty provides a perspective for looking beyond the party as a unitary actor and for analyzing the effect of intraparty politics on the party system.

[35] For example, an important member of the NDL, Kanju Sato, quit the SDPJ, ran for reelection under the Democratic Party, and lost. He might have been reelected if he had remained in the SDPJ (his name had been at the top of the SDPJ list in a proportional representation district, and the SDPJ won a seat there). Others who moved to the Democratic Party and lost may have had dim prospects for reelection anyway.

APPENDIX: THE MODEL

According to the revised framework presented in this article, party members' rational calculation of costs and benefits and their loyalty determine whether they exit or exercise voice. The cost-benefit calculation is influenced by the acquired characteristics of individual politicians, such as political career and economic background, whereas loyalty is a personality trait. This means that one weighs cost and benefits of voice by considering one's electoral strength, financial standing, reputation, and so on, while personal inclination influences whether one is loyal to an organization. Consider Y_i^* as an index of propensity to voice. Y_1^* is determined by characteristics of the ith member, that is, by her rational calculation of costs and benefits by her loyalty. In this context, nonvoice is regarded as loyalist behavior. If the latent (unobservable) variable Y_1^* determines the behavior of the ith individual (i.e., to voice or not to voice), then the above relationship is summarized as follows:

$$Voice = Y_i^* > 0;$$

$$Nonvoice = Y_i^* \leq 0.$$

We approximate Y_i^* by a linear function of personal characteristics of the ith individual. Among such characteristics, the level of a certain individual's loyalty to the organization is assumed to be innately determined (or acquired independently from one's political background) and to work against the exercise of voice. If the kth characteristic of the ith individual is expressed as $X_{ki}(k = 1, \ldots K)$, the above equation is rewritten as follows:

$$Y_i^* = \sum b_k X_{ki} + b_0 + e_i.$$

Thus Y_i^* is a function of personal characteristics of the ith individual, though Y_i^* cannot be observed. Consider the variable Y_i, which takes the value 1 or 0 according to the behavior of the ith individual, voice or nonvoice. Y is determined by the sign of Y^* as follows:

$$Y_i = 1 \quad \text{if } Y_i^* = \sum b_k X_{ki} + b_0 + e_i > 0;$$

$$Y_i = 0 \quad \text{if } Y_i^* = \sum b_k X_{ki} + b_0 + e_i \leq 0.$$

The model ultimately aims to find a probability that Y_i equals 1 (i.e., a probability that i exercises voice). From the above, when $\Sigma b_k X_{ki} + b_0 > -e_i$, $Y_i = 1$. Therefore, $P\ (Y_i = 1 | X_{ki}, k = 1, \ldots K) = F\ (\Sigma b_k X_{ki} + b_0)$, where F is a cumulative distribution function of e_i. $P(Y_i = 1)$ is a function of the linear term, $\Sigma b_k X_{ki}$.

Because a binary choice between exit or nonexit within the organization is analyzed by the same model as the choice between voice or nonvoice here, only the model for the voice analysis is explained. For the quantitative analysis of the members' exit (from the party) and voice (to express dissatisfaction with the party) based on the above formulation, the article employs a probit model that is suitable for the analysis of qualitative data that includes a binary choice. The probit model has the assumption that the error term, e_i, is normally distributed and can be estimated using the maximum likelihood method.

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From Abe Fortas to Zoë Baird: Why Some Presidential Nominations Fail in the Senate

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Why, given a strong presumption of success, do some presidential nominations fail? Of 1,464 important nominations from 1965 to 1994, less than 5% failed. Ninety-four percent of failures were rejected or withdrawn before reaching the floor, suggesting that opponents are most effective during prefloor stages. We propose a theoretical framework based on the notion that policy entrepreneurs pursue their goals within the context of a presumption of success. Logit analysis tends to support the theory that entrepreneurs can alter the presumption of success and defeat a nomination if they (1) identify negative information about a nominee to provide a rationale for changing the presumption and (2) expand the conflict through committee hearings and the media. Presidential resources—high public approval and efforts to signal that the nomination is a high priority—increase the chances of confirmation. Contrary to previous research, divided government has no independent effect on the fate of nominations.

The U.S. Congress was designed to make legislating difficult. Fragmented power and multiple decision points afford those opposed to a bill ample opportunities to defeat it. In the Senate more than the House, members have significant parliamentary rights that allow them to delay action on legislation. Most bills introduced in Congress are ignored. Presidential proposals fare only slightly better—about one-fourth make it into law (Peterson 1990). In sum, for legislation introduced in Congress, there is a presumption of failure.

Nominations are different. Presidential nominees, even for the highest offices in the executive branch and the judiciary, typically are approved with little opposition (Stanley and Niemi 1994, 279). Although senatorial prerogatives to block action apply to nominations as well as to legislation, when the Senate receives a presidential nomination, there is a presumption of success. For several reasons, the Senate by tradition defers to the president in the staffing of the government and judiciary (Fisher 1991). First, the Constitution affords the president greater authority over appointments than over legislation. Furthermore, if the Senate turns down a nominee, there is no assurance that the replacement will be less objectionable. Second, because these positions must be filled to have a well-functioning government, the Senate does not have the same leeway to ignore nominations as it does most legislative proposals. Moreover, while legislation can be amended to make it more palatable to opponents, nominees must either be accepted or rejected. Third, the president often consults with key senators before sending nominations to the Hill. Consequently, the Senate confirms all but a tiny number of presidential appointments.

Yet, some nominations run into trouble in the Senate and fail. Because of the presumption of success, explaining that failure poses an interesting question: Under what conditions does the Senate, predisposed to allow the president wide latitude in staffing the government, at times change this presumption and revert to a process more typical of its handling of legislation? We seek to answer this question with an analysis of presidential nominations from 1965 to 1994. We begin with a review of the literature, identifying gaps in our understanding of the confirmation process.

LITERATURE AND THEORETICAL FRAMEWORK

The literature provides insights about confirmation politics, but gaps remain. Much of the quantitative work has focused on nominations to the Supreme Court, analyzing senators' floor voting behavior (Cameron, Cover, and Segal 1990; Lanier 1995; Overby et al. 1992). Such analyses do not explain the collective outcome of greatest interest to the president: success or failure. Moreover, analyzing only floor votes on Supreme Court nominees provides an incomplete understanding of confirmation politics in general, since the justices are a small and unrepresentative sample of significant presidential nominations. More important, most unsuccessful nominations fail before reaching the floor, so cases defeated by floor votes are not representative of failures.

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We thank the Department of Political Science at Texas A&M University and the Graduate School of Arts and Sciences at Fordham University for their support. We are grateful to George C. Edwards III and the Center for Presidential Studies at Texas A&M University for supplying data on public approval of the president's job performance from 1988 to 1994, and to Michael MacKuen for sharing his presidential events data set. We also gratefully acknowledge helpful advice from Frank Baumgartner, Greg Caldeira, Kristen Campbell, Roy Flemming, Stephen Hanna, Kim Hill, Bryan Jones, Mike MacLeod, Ken Meier, Jeff Talbert, James True, Guy Whitten, Dan Wood, and participants at the Columbia University Seminar on American Politics.

The few studies that analyze nominations more generally (e.g., Fiorina 1992) cast the empirical net too wide, analyzing all nominations (tens of thousands in most Congresses). Because only a small number are salient enough for the president and Senate to take an active interest, the many routine approvals involving minor offices may distort the results.

Previous research does provide guidance for the development of a broader theoretical understanding of confirmation politics. We propose a framework based on the notion that policy entrepreneurs pursue their goals within a context of a presumption of failure for legislation and a presumption of success for nominations.

Policy Entrepreneurs and Nominations

Policy entrepreneurs are risk takers who care deeply about issues and the actions of government (Kingdon 1984; Schneider, Teske, and Mintrom 1995). Although policy entrepreneurs may come from interest groups or from the intellectual community, at some point they must enlist the active participation (Hall 1996) of members of Congress.[1] The burden of proof differs for policy and nominations. For policy, the burden is on supporters; policy failure is most likely because it is hard to overcome the presumption of failure. For nominations, the burden is on opponents; defeat of a nominee is rare because it is hard to surmount the presumption of success.

Because of the presumption of success, supporters of nominees normally do not need to push hard to get them considered. Opponents, in contrast, face a daunting task. To change the presumption of success, opponents often use a public strategy analogous to that employed by those seeking to break up policy subsystems (Baumgartner and Jones 1993; Bosso 1987; Jones, Baumgartner, and Talbert 1993; Schattschneider 1960). Although the process does not always follow a prescribed sequence, it is useful for analytical purposes to consider two conceptually distinct phases. First, entrepreneurs portray a negative image of the nominee to provide a rationale for opposition. Second, entrepreneurs seek to expand the conflict to persuade senators to see controversy. Kingdon (1981) finds that House members go along as usual in their decision making unless they perceive controversy, in which case they seek more information and scrutinize legislation. Those opposing nominations seek to get senators into such a frame of mind. Senate rules permitting unlimited debate and the prerogative to place a "hold" on nominations allow entrepreneurs to delay action while they expand the conflict.[2] Because of the presumption of success, entrepreneurs pick their battles carefully. Thus, we expect that a small proportion of nominations will be challenged and defeated.

Those opposed to a nominee must persuade additional senators to alter the normal presumption. The number of senators needed and the difficulty of persuading them vary at different points in the process. Defeating a nomination on the floor requires a majority to go on record against the nominee and the president. A defeat before that stage often takes less than a majority, and opponents do not always have to go public. Consequently, most failures never reach the floor. The visible floor defeats of Robert Bork and John Tower are unusual and unrepresentative of failed nominations.

We turn now to a discussion of rationales that entrepreneurs use as a basis for opposing a nominee and strategies to expand the conflict.

Rationale for Opposition

Opponents search for negative information in the nominee's background that can provide a rationale for opposition, including wrongdoing, a lack of qualifications, and ideological extremism.

The public expects government positions to be filled by ethical individuals who are qualified for the job. Credible evidence of wrongdoing provides a strong reason for opposing a nominee. Two examples of derailment due to such allegations are Abe Fortas, President Johnson's candidate for chief justice in 1968, and Zoë Baird, nominated for attorney general by President Clinton in 1993. Some government positions require special training and expertise. Although appropriate qualifications vary with the office, questions about a nominee's suitability can provide a basis for opposition. Examples include President Reagan's 1981 nomination of William Bell to head the Equal Employment Opportunity Commission and President Bush's 1989 nomination of William Lucas as assistant attorney general for civil rights.

More recently, ideological extremism has become another rationale for opposing nominations. From 1981 to 1994, senators challenged 20 cases on this ground, a fivefold increase over the four cases from 1965 to 1980. The increase in the use of wrongdoing or inadequate qualifications has been less dramatic: 15 cases from 1965 to 1980, compared to 29 from 1981 to 1994. The ideological extremism rationale was used to oppose President Reagan's nomination of Robert Bork to the Supreme Court in 1987 and President Clinton's nomination of Sheldon Hackney to head the National Endowment for the Humanities in 1993.[3] Previous

[1] Recent research documents the increased role of interest groups in the confirmation process, especially after the failed Fortas nomination in 1968 (Caldeira, Hojnacki, and Wright 1996; Caldeira and Wright 1995; Flemming, MacLeod, and Talbert 1998).

[2] For this study, a "hold" was considered a temporary delay to allow the senator who placed the hold to receive additional information. Party leaders normally honor the hold and do not schedule a vote until it is removed. Although this process occurs behind the scenes and cannot be incorporated as a variable in the analysis, the definition of failed nominations as those that are not confirmed includes cases defeated by inaction. The importance of this practice for our framework is that it allows entrepreneurs in the Senate to delay action in order to expand conflict.

[3] Although some may suppose that the Bork case was the first of this type, the watershed year seems to be 1981. Bork was among the last of Reagan's appointments challenged on the ground of extreme conservatism. In 1981, for example, this objection was raised against four Reagan nominees: William French Smith for attorney general,

research finds that nominees' ideology affects the chances of confirmation (Cameron, Cover, and Segal 1990; MacKenzie 1981; Massaro 1990; Overby et al. 1992).

Some characteristics may insulate a nominee from more intense scrutiny and increase the likelihood of confirmation, such as previous approval for a different office or former membership in the Senate or House. Opposition may be less likely if a nominee has weathered a process that failed to reveal negative information, and those with congressional experience bring political (as distinct from administrative) skill to an office. Since this skill is respected by the Senate (Fenno 1959, 75–6), senators may be favorably disposed toward nominees with political and personal ties to Congress.

Expanding the Conflict in Congress

Once opponents identify a potential flaw, they exploit it by expanding the scope of conflict. In politics, how a situation (or a nominee) is perceived is often more important than objective reality. Charges of inadequate qualifications and ideological extremism are based largely on perception. Standards of wrongdoing also change over time as perceptions of what is ethical evolve, and an illegal act once considered merely a technical violation can come to be viewed as more serious. Entrepreneurs must do more than make charges about a nominee's background. They must make the case that the allegation is credible and important enough to defeat the nomination.

Two avenues for expanding conflict are (1) an internal strategy through committee hearings and (2) an external strategy that encourages scrutiny by the national media. In the rough and tumble of Washington politics, these strategies become intertwined: Revelations in a committee hearing generate additional media coverage, which leads to more hearings and tougher questioning. Therefore, separating the causal sequence is problematic, but it is useful for conceptual clarity to discuss these strategies separately. The internal strategy takes place during the confirmation hearings, which are typically low-key affairs involving the nominee and a handful of witnesses who present only positive testimony. Among the important nominations in our study, the median number of witnesses was one: solely the nominee at 52% of hearings; no more than four additional witnesses in 92% of the cases. Opponents can expand conflict by enlarging the witness list to include those who present negative testimony. This expansion is a signal to senators that the appointment deserves more attention than normal and increases the likelihood of justifying a rationale.

Ernest Lefever for assistant secretary of state, James Watt for secretary of the interior, and Anne Burford Gorsuch for EPA administrator. We cannot determine whether the increased use of this rationale resulted from more extreme candidates being named or from changes in senators' behavior. Although this is an important question, it is sufficient for this analysis to observe the addition of this rationale.

The external strategy expands conflict through increased media coverage, which also signals the Senate that a nomination should be closely scrutinized. Previous research finds that more intensive news coverage is associated with more controversial nominations (MacKenzie 1981).

Thus, opponents seek to portray a nominee in a negative light and try to expand the scope of conflict, both internally through committee hearings and externally through high media exposure. The president, however, is neither helpless nor passive in this process.

Presidential Strategies and Resources

Once a nomination becomes embattled, the president's options are limited. Going public to support a challenged nominee may draw more attention to the controversy, and presidents are constrained in how many personal appeals they can make to senators on behalf of embroiled nominees. A better strategy is to preempt the opposition. "The best point at which to manage conflict," says Schattschneider (1960, 15), "is before it starts." The president may avoid trouble by consulting with senators to identify candidates and by screening nominees in light of past controversies. For political and policy reasons, presidents still nominate controversial individuals. When a controversial name is sent to the Senate, the president may go to extra lengths to highlight positive aspects of a nominee's experience and qualifications in an effort to preempt potential opposition.

The president's popularity with the public is a resource that may influence members of Congress (Neustadt 1960). Washingtonians widely accept the view that Congress is more inclined to give presidents what they want when public support is high rather than low. Previous studies provide some empirical support for this view (Brace and Hinckley 1992; Edwards 1980; Ostrom and Simon 1985; Rivers and Rose 1985). Other research identifies methodological and theoretical reasons to question this relationship (Bond and Fleisher 1984, 1990; Collier and Sullivan 1995). Although presidential popularity may have only marginal effects on legislation, there is reason to hypothesize that it may be more significant for nominations, since it reinforces the presumption of success. Given this presumption, opposing a nominee poses risks to entrepreneurs: (1) embarrassment and damage to reputation if the opposition is unsuccessful, (2) lost time that might have been used more effectively on a legislative initiative, and (3) possible future retribution from the president. Because high popularity reinforces the presumption of success and the risks associated with opposing a nomination, entrepreneurs may wait for another case when the president is perceived to be more vulnerable.

Political Circumstances

Entrepreneurs operate under political circumstances, some of which may be more favorable to opposing nominations than others. Circumstances that may influence the confirmation process include party control

of the presidency and the Senate, timing in the president's term, and type of office involved in the nomination.

Divided Government. There are theoretical reasons to expect more failed nominations when the opposition party controls the Senate. Members of the opposition party are predisposed to disagree with the president's preferences on policy and personnel (Bond and Fleisher 1990). Under divided control, there are more of these members. More important, they hold the levers of power, chairing committees and controlling the scheduling of business. Research on Supreme Court nominations finds a relationship between party control and success (King and Riddlesperger 1991; MacKenzie 1981; Ruckman 1993; Scigliano 1971; Segal 1987). But since Senate rules bestow great power on individual senators regardless of party, this relationship may not appear in a more representative sample of nominations.

Timing. Washington time runs on the electoral calendar. Entrepreneurs may have greater difficulty challenging nominations early in the president's term. Light (1983, 44, 218) advises new presidents to "move it or lose it," because they are more successful in pursuing a legislative program during the honeymoon. It is also reasonable to expect the Senate to give newly elected presidents latitude in staffing the government. Conditions later in the president's term make it less difficult to challenge nominations. After the midterm elections, presidents typically have fewer members of their party in the Senate.[4] As the presidential election approaches, opposition party senators have a greater incentive to deal the chief executive an embarrassing defeat, and a president running for reelection may become distracted with the campaign. A lame duck is in a weakened position even with senators from the same party, who know they will not have to run with this candidate at the top of the ticket.

Previous studies of judicial appointments suggest that timing influences confirmations. Ruckman (1993) and Scigliano (1971) found that Supreme Court nominations are more likely to fail late in the president's term. Richardson and Vines (1971) and Bond (1980) found that Congress is more likely to pass bills increasing the number of federal judges early in a presidential term.

Type of Office. Different offices evoke different expectations about the skills and experience needed to fill them, so this factor is likely to have a bearing on confirmations. And the Senate may be more likely to defer to the president on executive branch appointments than on appointments to the judiciary or independent regulatory agencies. Consequently, we control for type of office, but we do not have strong theoretical expectations regarding the direction of all the relationships.

[4] The pattern of the president's party losing seats in midterm elections is not as consistent in the Senate as in the House. The pattern prevailed in the Senate in six of the eight midterm elections since 1966 (Ornstein, Mann, and Malbin 1996, 55).

RESEARCH DESIGN AND MEASURES

Units of Analysis

The unit of analysis is nominations considered by the U.S. Senate from 1965 to 1994, but not all these are appropriate to test our theory. Most presidential appointments each year are military and foreign service promotions that typically pass in large blocks with little scrutiny. Including such cases may distort the results. We need a subset of important nominations in which the president, senators, and other entrepreneurs are likely take an active interest, but the sample should be broad enough to be representative. We define as important all nominations to the (1) Supreme Court, (2) U.S. Circuit Courts of Appeals, and (3) level 1, 2, and 3 positions in the Federal Executive Schedule (U.S. Congress, House Committee on Post Office and Civil Service 1988 and 1992), except "inner cabinet" positions (Defense, Justice, State, and Treasury), which also include level 4 positions.[5] This yields a sample of 1,464 important nominations from 1965 to 1994. Supreme Court and appellate court nominations comprise the most important judicial appointments. Using the Executive Schedule identifies the top positions in the executive branch and bureaucracy.

Dependent Variable

The dependent variable is the *Outcome* of a nomination (pass = 1; fail = 0). A failed nomination is one submitted to the Senate for consideration but not confirmed (U.S. Government Printing Office 1966–95a, 1966–95b, 1966–95c). Nominations may fail at several points in the process. The Senate may reject the nominee on the floor or in committee; the president may withdraw the nomination before overt rejection; the Senate may not act because a "hold" prevents it or because time runs out at the end of the session.[6]

Previous empirical studies of nominations focus on floor votes (Cameron, Cover, and Segal 1990; Lanier 1995; Overby et al. 1992). As we argued above, that provides an incomplete picture because most unsuccessful nominations fail before reaching the floor. Table 1 presents information on the point at which nominations fail. Of the 1,464 nominations in the data set, less than 5% (71) failed, which suggests that even the most important cases have a presumption of success. Yet, instances of failure are sufficiently common

[5] The Executive Schedule has five salary levels. Cabinet secretaries and their equivalents are level 1; level 2 positions are held by undersecretaries and heads of major subcabinet agencies, such as the Department of the Army and the FBI; lower level cabinet secretaries and heads of government agencies and regulatory commissions receive level 3, 4, or 5 salaries depending on the size and prestige of the agency (Meier 1993, 39).

[6] Two cases withdrawn before committee hearings were not coded as failures. One was President Carter's nomination for FBI director of Frank Johnson, who had a heart attack and asked to withdraw. The other was President Reagan's nomination of Frank Gaffney to be assistant secretary of defense, which was a promotion from a lower position he held in the Department of Defense. Gaffney asked to withdraw and resigned from the department because of policy disagreements with the administration.

TABLE 1. Point at Which Nominations Fail, 1965–94

Final Action	Percent	N
Floor rejection	5.6%	4
Stalled, stopped between committee and floor	11.3	8
Withdrawn after committee forwarded to chamber	5.6	4
Committee rejected by vote	16.9	12
Stalled, stopped between hearing and committee vote	19.7	14
Withdrawn before committee vote (after hearing)	16.9	12
Committee failed to consider a nomination[a]	12.7	9
Withdrawn before committee hearings[a]	11.3	8
Total	100.0%	71

[a]The "Committee failed to consider a nomination" category includes cases that were not withdrawn but died because the committee never held hearings or took any action to process the nomination. The "Withdrawn before committee hearings" category includes cases withdrawn by the president or the nominee before committee hearings.

to justify trying to identify the reasons. Of the failed nominations, more than 94% never reached the floor. This finding suggests that efforts to defeat a nominee are most effective during prefloor phases of the process. More than three-fourths of unsuccessful nominations (55) did not emerge from committee, so that appears to be the most critical stage; another 17 cases failed even earlier, before committee hearings.

Table 2 shows the percentage of important nominations that passed during various administrations. Except for presidents Ford and Reagan (second term), success rates are similar across administrations. Ford's record is likely the result of the aftermath of the Watergate scandal that elevated him to the presidency without election. The decline during Reagan's second term is similar to that in Nixon's second term. Although Clinton's 97% success rate during 1993–94 is above the mean of the president's in this study, it represents the record of only two years, as opposed to four for the other presidents. This relatively high success rate does suggest, however, that the highly publicized defeats early in Clinton's first term are not representative of his record on nominations in the 103d Congress.

TABLE 2. Outcome of Nominations by Administration, 1965–94

Presidential Administration (Years)	% Successful	N
Johnson (1965–68)	99.0%	99
Nixon I (1969–72)	97.7	171
Nixon II (1973–74)	94.4	90
Ford (1974–76)	92.6	95
Carter (1977–80)	96.1	207
Reagan I (1981–84)	94.2	223
Reagan II (1985–88)	92.8	209
Bush (1989–92)	94.0	215
Clinton (1993–94)	96.8	155
Total	95.2%	1,464

Success rates of recent presidents are similar to those of their predecessors, which suggests that it has not become harder to get nominees through the Senate. That is, the presumption of success on nominations prevails throughout the period of study.[7] There is a slight divided government effect: 94% pass under split party control, compared to 97% with unified control.

Independent Variables

Nominee Factors. The nominee characteristics that entrepreneurs use as rationales for opposition are measured by four dichotomous variables (1 = yes, 0 = no) to indicate the presence or absence of a negative attribute: *Alleged Wrongdoing*, *Qualifications a Concern*, *Extreme Conservative*, and *Extreme Liberal*. Opponents alleged at least one negative attribute in about 4% (62) of the cases. Allegations of wrongdoing were most common (32 cases), followed by extreme conservatism (17 cases), qualifications (12 cases), and extreme liberalism (7 cases) (Congressional Quarterly, Inc. 1966–95).[8]

We also include measures of attributes that might insulate nominees from challenge: *Previously Confirmed* for another office and current or former members of the *House* or the *Senate* (1 = yes; 0 = no). About 36% (526) of nominees had been previously confirmed (Marquis Who's Who, Inc. 1969–97). Three percent (44) were former House members, and less than 1% (13) were former senators (Congressional Quarterly, Inc. 1993).

Expansion of Conflict. The measure of the *Scope and Tone of Testimony* in committee hearings is the ratio of negative to positive witnesses (number of negative witnesses/number of positive witnesses) (Congressional Information Service, Inc. 1966–95). Low key hearings with only the nominee testifying and larger hearings with only positive witnesses score zero. Such hearings are likely to lead to confirmation because no negative information is presented to challenge the presumption of success. Hearings with more negative than positive witnesses have scores greater than 1.00. Higher scores indicate greater controversy, which reduces the likeli-

[7] Unfortunately, there is about a two-year lag in reporting complete hearing information on nominations. Since hearings are a key part of our model, we cannot include Clinton's experience with a Republican majority. There is some evidence that the confirmation process may have undergone some change with Republican control of the Senate. Long delays in acting on large blocs of judicial appointments in the 105th Congress may indicate that the traditional obligation to process presidential appointments to keep government operating is less compelling than in the past.

[8] The total number of allegations is 68 because in a few cases multiple allegations were raised.

hood of confirmation. For the 1,447 cases in our data set on which hearings were held, this variable ranges from zero to 27, with a mean of .20 and a standard deviation of 1.26. The distribution, however, is highly skewed. More than 90% of hearings score zero (only positive witnesses), but there are six extreme outliers in which negative witnesses outnumbered positive witnesses by at least ten to one. Since we believe that the effect of adding negative witnesses is likely to increase rapidly at low values and then diminish, we transformed this variable with a square root function.[9]

The indicator of *Media Coverage* is the number of stories in the *New York Times* (1966–95) about the nomination prior to the date of passage or failure. The scores range from zero to 93, with a mean of 1.9 and a standard deviation of 5.1. There are a few extreme values, but this variable is less skewed than the testimony variable. About 40% (589) of the nominees in our sample received no mention in the *New York Times,* and roughly 57% received between one and ten mentions. Since this variable is not highly skewed, we did not use any nonlinear transformation.[10]

Presidential Variables. The president may attempt to preempt the opposition by publicly touting the merits of a nominee before sending the name to Capitol Hill. Our measure of *Initial Presidential Priority* is the number of column lines in the president's speech introducing the nominee (U.S. Government Printing Office 1966–95a). This variable ranges from zero to 203 column lines, with a mean of 19.4 and a standard deviation of 23.6. *Presidential Popularity*, a resource that might reinforce the presumption of success on nominations, is measured as the percentage approving of the way the president is handling his job in the closest Gallup poll before the nomination passes or fails (Edwards 1990). This variable ranges from 24% to 87%, with a mean of 52% and a standard deviation of 11%.

Political Circumstances. We control for several political circumstances that might affect the fate of nominations. *Divided Government* is coded 1 if the president and the Senate majority are from different parties and 0 for unified control.[11] About 45% (657) of the nominations in our sample were considered under divided government.

Four dummy variables were used to measure timing in a presidential term: *Early First Term*, *Late First Term*, *Early Second Term*, and *Late Second Term* (1 = early or late; 0 otherwise). Early is defined as the first year of the term; late is the fourth year. These variables test whether the probability of success in the first and fourth years is significantly different from that in the middle years.

Finally, we included six dummy variables for type of office being filled to detect any differences across the categories: *Cabinet*, *Subcabinet*, *Executive Office of the President/White House*, *Supreme Court*, *Appellate Court*, or *Government Corporation/Regulatory Agency* (the excluded category). We do not have theoretical expectations about the direction of relationships for these variables.

Methods

Because the dependent variable is dichotomous, we used logit analysis to estimate the models. We were concerned about possible endogeneity of some right-hand side variables, especially the measures of conflict expansion. A test for simultaneous influence developed by Rivers and Vuong (1988) indicates that neither the testimony nor the media variable is endogenous (see Appendix). Therefore, we report results of standard logit models.[12]

Since alleged wrongdoing, lack of qualifications, and ideological extremism are charges raised by opponents rather than objective criteria, these might be post hoc rationalizations rather than explanations of why nominations fail. Although we cannot definitively establish causation, we identified the point in the process at which the charge was made to determine the sequence of events (*Congressional Quarterly Weekly Reports* 1965–94). If an allegation surfaces before any public action in the Senate, then the temporal sequence suggests that the rationale led to defeat, rather than the reverse. In 62 cases a negative allegation was raised, and in 89% of these it was made before any formal Senate consideration: Seven cases involved charges before the nomination, 48 cases after nomination but before Senate hearings. In the seven other cases, allegations were first raised during the hearings. In no cases did allegations emerge after hearings. Since so few charges were made after the commencement of formal Senate actions, it seems reasonable to infer that the direction of causation goes from rationale to defeat.

FINDINGS

Table 3 presents a logit analysis of the effects of the independent variables on the confirmation process. Recall from Table 1 that 17 cases were withdrawn before hearings or failed because the Senate took no action. Since there were no hearings, the testimony variable is not applicable, and there is no completely satisfactory way to deal with this missing data problem. Since we have so few failures, excluding these cases from the multivariate analysis eliminates nearly one-fourth of the failures, but efforts to impute a "tone" to hearings that never occurred seem problematical. Among the various possibilities, the process of multiple

[9] We did not use a natural log transformation for two reasons: (1) The square root transformation is a more gentle diminishing returns function, and (2) most of our cases score zero, and the natural log of zero is undefined.

[10] Using a square root transformation of this variable does not change the results.

[11] Some may object to using a dichotomous variable when the actual party split is available. Using the number of seats of the president's party in the Senate, however, produces similar results. This suggests that if divided government contributes to defeating nominations, the key reason is that the opposition controls the levers of power.

[12] We were also concerned that these two variables might be colinear. Their correlation is .39.

TABLE 3. Logit Models of the Outcome of Presidential Nominations, 1965–94

Independent Variables	Expected Direction	Model 1[a] b	t	Δp[b]	Model 2[a] b	t	Δp[b]
Nominee Factors							
Alleged Wrongdoing	−	−4.33***	−6.82	−.49	−4.21***	−7.05	−.49
Qualifications a Concern	−	−2.11**	−2.42	−.39	−2.05**	−2.48	−.39
Extreme Conservative	−	−2.16**	−2.41	−.40	−1.61*	−1.75	−.33
Extreme Liberal	−	−1.58	−1.32	−.33	−2.06*	−2.14	−.39
Previously Confirmed	+	1.00*	2.14	.23	.90**	2.36	.21
U.S. Senator	+	−.91	−1.33		−.08	−.05	
U.S. House Member	+	.28	.25		−.26	−.31	
Expansion of Conflict							
Scope and Tone of Testimony	−	−1.15***	−4.19	−.15 (−.26)	−1.06***	−4.20	−.14 (−.24)
Media Coverage	−	−.06*	−2.05	−.10 (−.18)	−.05*	−1.99	−.09 (−.16)
Presidential Resources							
Initial Presidential Priority	+	.02*	1.88	.20 (.29)	.02*	1.72	.16 (.23)
Presidential Popularity	+	.04*	1.87	.43 (.45)	.04**	2.75	.44 (.46)
Political Circumstances							
Divided Government	−	.26	.66		−.43	−1.30	
Early First Term	+	−.14	−.23		−.47	−.93	
Late First Term	−	−1.40**	−2.65	−.30	−1.67***	−3.96	−.34
Early Second Term	+	−.67	−1.06		−.48	−.81	
Late Second Term	−	−1.48**	−2.40	−.31	−1.19*	−2.20	−.27
Type of Office							
Cabinet		3.83**	2.97	.48	3.54**	2.89	.47
Subcabinet		.13	1.17		.03	.05	
EOP/White House		1.50	1.08		1.31	1.05	
Supreme Court		.64	.40		.53	.35	
Appellate Court		.65	1.01		−.30	−.55	
Constant[c]		1.93*	2.00	.37	1.88*	2.18	.37
N of cases			1,447		1,464		
Likelihood ratio test (21 d.f.)			187.181***			210.032***	
Percent correctly predicted			97.3			96.1	
Percent in modal category			96.3 (pass)			95.2 (pass)	
Proportional reduction in error			.27			.19	

Note: The dependent variable is coded 1 if the nomination passed and 0 if it failed. *$p \leq .05$, **$p \leq .01$, ***$p \leq .001$, one-tailed; two-tailed tests were used for type of office and the constant.

[a]Model 1 excludes the seventeen failed cases with no hearing. Model 2 includes those cases and imputes a hearing witness value based on similar cases in the sample (Rubin 1987). In the case of Lani Guinier, who withdrew before hearings, for example, similar assistant attorney general nominees in the sample were identified based on values of the other independent variables. A case from this list of similar cases was then randomly selected and models estimated with that hearing value inserted for the Lani Guinier case. Logit done on SPSS for Windows, Release 7.5.2.

[b]For dummy variables, we report the change in probability from .5 if the variable goes from zero to one. For interval variables, we report the change in probability from .5 if these variables change by one and two standard deviations. Since the logit curve is steepest at .5 probability, these estimates indicate the maximum potential effect of these variables.

[c]The constant reflects the effects of government corporation/regulatory agency nominations during the second and third years of presidential terms.

imputation (Rubin 1987) seems most appropriate. Although this technique is particularly appropriate for surveys, it can be used to deal with missing data in nonsurvey designs (Rubin 1987, 3). Thus, we estimate the model two ways. Model 1 reports estimates without the 17 failures that had no hearings (n = 1,447), while model 2 reports estimates with hearing values imputed for these failures (n = 1,464). The results of both models support our expectations about negative information and the expansion of conflict.[13]

All the rationales that entrepreneurs use to portray a nominee in a negative light—alleged wrongdoing, lack of qualifications, and ideological extremism—significantly reduce the chances that the nomination will be confirmed.[14] To see substantive effects, we present the change in probability of success under different values of the independent variables (Greene 1993, 638–41). Charges of not being qualified or ideological extremism reduce the probability of confirmation by at least

[13] In addition to the method described in note a of Table 3, we also imputed values using predicted values from the first-stage estimates and inserting the mean value for the testimony variable. Although the coefficient for the latter is slightly smaller, it remains significant ($p < .001$), and the other findings are unchanged.

[14] The coefficient for extreme liberal in model 1 has a p-value of .09, whereas in model 2 it passes the .05 threshold. Only seven nominees were challenged as being extreme liberals. One was Lani Guinier, whose nomination was withdrawn before hearings, so her case is not included in model 1.

one-third; alleged wrongdoing has the strongest effect, cutting the chances of success nearly in half.

Background characteristics that might insulate nominees from challenge have less influence. Those who have weathered a previous confirmation tend to be successful, but the marginal increase in the probability of success is only about 20%. Former members of Congress do not receive more favorable treatment in the Senate than nominees without congressional ties.

The models also show that both internal and external strategies to expand conflict significantly reduce the probability of confirmation. Hearings with 1.5 times more negative than positive witnesses (one standard deviation above the mean) reduce the chances by 15% (at two standard deviations above the mean, the odds drop another 10%). If media coverage increases to six stories, the probability of confirmation declines 10%.[15]

Presidential resources enhance the probability of confirmation. Efforts to preempt potential opposition by signaling that a nomination is a high priority increase the chance of success about 20%. Presidential popularity has a stronger influence. If public approval is one standard deviation above the mean (about eleven points), the positive effect on the probability of success is about 40%, a similar magnitude to the detrimental effects of negative nominee factors. The strong association with public approval suggests that entrepreneurs may be reluctant to assume the risks of trying to change the presumption of success on nominations when the president is extremely popular. This result stands in sharp contrast to previous findings indicating that public approval of the president has a marginal influence on legislative success in Congress (Bond and Fleisher 1990; Collier and Sullivan 1995; Edwards 1989).

Political circumstances have mixed effects. Contrary to studies of Supreme Court nominations (King and Riddlesperger 1991; MacKenzie 1981; Ruckman 1993; Scigliano 1971; Segal 1987), we find no significant independent effect of divided party control, which suggests that nominations of justices are not representative of nominations in general. Given Senate rules that bestow significant power on individuals, it is not surprising that once opponents provide a rationale and expand the conflict, they can challenge nominations about as effectively under unified as under divided control.[16] Consistent with previous studies (Ruckman 1993; Scigliano 1971; Segal 1987), nominations are more likely to fail late in the president's term.

Finally, among the various types of offices, only cabinet nominations are treated differently: Nominees to head these departments are more likely to be confirmed than nominees to other offices. This result may reflect a tendency to allow the president leeway in forming his government.

CONCLUSION

In a legislative system designed to block initiatives, nominations pose a puzzle. In contrast to legislation, most nominations—even the most important cases selected for this study—pass without challenge. Our model attempts to explain why, given the presumption of success, some nominations fail. Our theoretical framework is based on the notion that policy entrepreneurs pursue their goals within the context of this presumption. If no controversy arises around a nomination, senators, committees, and the chamber go along with the president and confirm the nominee. The aim of opposition entrepreneurs, therefore, is to alter the presumption. They want senators to see controversy and to scrutinize nominees closely. We suggest that entrepreneurs accomplish this through a two-part strategy: (1) Identify negative information about a nominee to provide a rationale for changing the presumption, and (2) expand the conflict internally through committee hearings and externally in the mass media.

Our analysis of important nominations from 1965 to 1994 tends to support this theory. Less than 5% of these nominations failed. Ninety-four percent of failures were rejected or withdrawn before reaching the floor, which suggests that opponents are most effective during the prefloor stages of the process. Allegations of wrongdoing, concerns about qualifications, and ideological extremism all provide a rationale to defeat a nomination. Expansion of the conflict, as indicated by a disproportionate number of negative witnesses at the hearings and considerable coverage in the national press, also reduces the likelihood of confirmation. And nominations made late in the president's term are more likely to fail. High presidential approval and extra efforts to tout the candidate's qualifications in the president's speech announcing the nomination increase the chances of success. In addition, nominees who have been previously confirmed for another office and especially the cabinet are more likely to pass than others. Contrary to previous research, divided government has no independent effect on the fate of nominations.

These findings suggest at least two implications for our understanding of politics in Congress. First, our model's presumptions of failure for legislation and success for nominations is a useful way to think about policymaking in Congress. The tasks and strategies of policy entrepreneurs vary with these presumptions. On legislation, supporters struggle to get the proposal noticed and considered. On nominations, it is opponents who must struggle to get the attention of others.

[15] Although the independent effects of expanding the conflict appear smaller than the effects of alleged negative attributes and previous confirmation, remember that these variables are measured in different units. Negative and positive nominee factors vary from minimum to maximum values (0 or 1), whereas conflict expansion variables vary from their mean by one and two standard deviations. In addition, strategies to expand conflict take place in conjunction with the use of a negative rationale by entrepreneurs, so expansion of conflict further reduces an already diminished probability.

[16] We also tested for different relationships under divided and unified government. Wrongdoing, qualifications, expansion of conflict through hearings, and cabinet appointments have the same effects in both circumstances. Media coverage and previous confirmation are not significant in either. Ideological extremism is significant only under divided government, whereas presidential efforts to preempt opposition work only under unified government.

If they succeed in portraying a nominee negatively, then the president is constrained in how to help a nomination through.

Second, previous studies of floor voting focus on Supreme Court appointments and provide an incomplete understanding of confirmation politics in general. The justices represent only a fraction of the nominees to important offices that the president must get confirmed in the Senate. And defeats on the floor are a small and unrepresentative sample of the relatively rare failures. Our analysis of a broader, more representative sample of important nominations confirms some previous findings and suggests that others should not be generalized beyond the Supreme Court context.

APPENDIX

Rivers and Vuong (1988) developed a test for endogeneity in models with a dichotomous dependent variable. It is a two-stage conditional maximum likelihood estimator with several advantages, including computational ease, asymptotic efficiency, and small sample properties. The procedure provides a direct test of endogeneity of one or more right-hand-side variables.

The first step is to estimate a reduced form equation for each right-hand-side variable suspected of endogeneity. The first-stage equations include the exogenous right-hand-side variables from the full model plus others associated with the suspected endogenous variable. The second step is to include the residuals from the first-stage equations with the suspected endogenous variables in the full probit model. If residuals from the first-stage equations have large and statistically significant coefficients, then we reject the null hypothesis that the variables are exogenous.

We estimated first-stage reduced-form ordinary-least-squares regression equations for the expansion of conflict variables: *New York Times* coverage and scope and tone of testimony. OLS is appropriate because these are interval variables. In the two first-stage models, we included two variables in addition to all exogenous variables from the full model.

The additional variables in the *NYT* model are (1) *Total Hearing Witnesses* and (2) total coverage of *Presidential Events* (excluding nominations) (MacKuen 1983; updated by MacKuen) normed by the number of days the case was under consideration. As hearings feature larger numbers of potentially more newsworthy witnesses, media coverage should increase. Coverage of other presidential events indicates media appetite for presidential news.

TABLE A-1. First-Stage OLS Models of *New York Times* Nomination Coverage and Tone of Witness Testimony

	New York Times Nomination Coverage			Scope and Tone of Witness Testimony		
Independent Variables	*b*	S.E.	*t*	*b*	S.E.	*t*
Exogenous Variables from Full Model						
Alleged Wrongdoing	2.97***	.53	5.60	.61***	.07	8.76
Qualifications a Concern	−1.08	.83	−1.30	.78***	.11	7.14
Extreme Conservative	.33	.76	.43	.36***	.10	3.59
Extreme Liberal	4.07***	1.12	3.63	.27*	.15	1.82
Previous Confirmation	.21	.16	1.36	.02	.02	1.23
U.S. Senator	3.36***	.79	4.29	−.14	.10	−1.32
U.S. House Member	−.85*	.44	−1.95	.05	.06	.94
Initial Presidential Priority	.02***	.004	5.57	−.00	.00	−.80
Presidential Popularity	−.003	.007	−.41	−.001	.001	−1.35
Divided Government	.09	.15	.60	−.03*	.02	−1.68
Early First Term	.20	.19	1.05	.04	.03	1.50
Late First Term	.40	.27	1.46	.09*	.04	2.42
Early Second Term	.23	.25	.93	.09**	.03	2.82
Late Second term	−.01	.30	−.03	.05	.04	1.21
Cabinet	2.44***	.31	7.86	.07*	.04	1.73
Subcabinet	.12	.22	.56	−.03	.03	−1.19
EOP/White House	.07	.34	.19	−.04	.05	−.99
Supreme Court	10.50***	.79	13.25	−.24**	.10	−2.33
Appellate Court	−.23	.28	−.84	−.05	.04	−1.39
Additional Variables						
Total Hearing Witnesses	.49***	.02	31.27	.03***	.002	13.37
Presidential Events	3.03*	1.33	2.28		—	
Presidential Success (Trimester)		—		−.001*	.001	−1.67
Constant	−.54	.43	−1.26	.16*	.07	2.40
N of cases		1,447			1,447	
R^2		.724			.334	
Adj. R^2		.720			.324	
F-statistic		177.727***			33.993***	

Note: The dependent variable in the media model is the number of stories about each nomination appearing in the *New York Times*. The dependent variable in the witness model is the square root of the ratio of negative to positive witnesses. OLS was done on SPSS for Windows, Release 7.5.2. *$p \leq .05$, **$p \leq .01$, ***$p \leq .001$, one-tailed; two-tailed tests are used for the constant.

The additional variables in the scope and tone of testimony model are (1) *Total Hearing Witnesses* and (2) *Presidential Success* rate (excluding nominations), which reflects the level of conflict between the president and Congress in the four months before and during the hearings. Calculating presidential success rates by trimester permits a more appropriate temporal matching than annual calculations; since some months lacked a vote on which the president expressed a position, an interval shorter than a trimester was precluded. The proportion of negative witnesses is likely to increase both when the total number of witnesses expands and when the president and Congress are feuding over policy (indicated by a low success rate).

Table A-1 presents the first-stage results. The media model explains 72% of the variance, and the hearings model explains 32% of the variance. The new variables and several exogenous variables from the full model are significant in both first-stage models.

In the second stage, we included the residuals from the first-stage equations in the full probit model. The coefficients of both reduced-form residuals in the full models are small and insignificant. In the full model, the *NYT* residual has a b of $-.29$ and t-statistic of $-.91$; the scope and tone of testimony residual has a b of 1.42 and t-statistic of .25. These results of this test indicate that neither media attention nor the tone of hearings is endogenous.

The Rivers and Vuong test was developed for probit models, and we used probit analysis for this diagnostic. In the text we report results of logit analysis because estimating substantive probabilities is more convenient. Results using probit are the same.

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Macropartisanship: A Replication and Critique

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*MacKuen, Erikson, and Stimson (1989, 1992) argue that the aggregate distribution of party identification, or macropartisanship, shifts significantly over short periods in response to changes in presidential popularity and consumer sentiment. Their results are based on a subset of Gallup surveys available from 1953 to 1988 and only those CBS/*New York Times *surveys conducted during the Reagan administration. We replicate this analysis using a more extensive inventory of Gallup and CBS/*New York Times *data and find considerably less evidence of partisan fluctuation. The amount of partisan change caused by short-term movements in consumer sentiment and presidential popularity is found to be two to three times smaller than initially reported by MacKuen et al. (1989). Our results indicate that macropartisanship adjusts to short-term shocks in a limited and gradual fashion, consistent with traditional views of partisan realignment.*

Despite the centrality of party identification to explanations of voting behavior, its nature and origins remain the subject of ongoing dispute. The traditional view of partisanship, as presented in *The American Voter* (Campbell et al. 1960), emphasizes the psychological bond that links citizens to political parties. Partisans come to see themselves as members of social groups (e.g., Democrats, Republicans) in much the same way that people incorporate religious, regional, or ethnic group memberships into their self-conception. Because it is disquieting to alter one's self-concept, particularly when it involves repudiating once-admired groups and symbols, partisan identifications that take root in young adulthood tend to persist even amid changing political circumstances. To be sure, party attachments can be disrupted in systems such as that in Canada, where new parties emerge that pit linguistic, ethnic, and regional identities against preexisting party identities (Schickler and Green 1997), or in Italy, where party labels change (Converse and Pierce 1985). But under ordinary circumstances, the distribution of partisanship is expected to adjust slowly to swings in political or economic fortunes and shift appreciably only when short-term forces favor one party for a sustained period. Party identification, as Campbell et al. (1960, 1966) argue, functions as ballast in an electoral system, stabilizing party competition amid shifting political currents.

An alternative view of partisanship, associated with scholars such as Downs (1957) and Fiorina (1981), regards party identification as a "running tally" of evaluations of party platforms and performance in office. From this vantage point, party identification more closely resembles presidential approval than religious affiliation. Continually updated in light of current information, partisanship may undergo considerable change as events alter public confidence in each party. During certain periods, the distribution of partisanship may remain in place, but this quiescence merely reflects stability in party platforms and capabilities (Franklin and Jackson 1983).

This debate has played itself out in two research literatures. The first traces individual survey respondents over time and gauges whether partisan affiliations change as opinions about the parties change. The findings have been quite mixed. Evaluations of the parties' presidential candidates (Page and Jones 1979), issues stances (Franklin 1992; Franklin and Jackson 1983), and performance in office (Brody and Rothenberg 1988; Fiorina 1981) have been said to affect party identification, but each of these studies has drawn criticism on methodological grounds from research that finds party identification to be highly resistant to change (Green and Palmquist 1990, 1994; Schickler and Green 1993–94, 1997).

The second body of literature tracks the aggregate distribution of party identification over time. The traditional view of partisanship is challenged by time-series analyses showing that the distribution of party identification shifts markedly over short periods and moves in tandem with fluctuations in presidential popularity, consumer sentiment, or objective economic indicators of unemployment and inflation (MacKuen, Erikson, and Stimson 1989, 1992; Weisberg and Kimball 1995; Weisberg and Smith 1991). "Macropartisan" volatility, that is, abrupt shifts in the proportion of Democrats and Republicans, seems antithetical to the limited and gradual change envisioned by Campbell et al. (1960, 1966).[1]

This article takes a closer look at the most important

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The authors are grateful to the National Science Foundation (SBR-9357937) and the Institution for Social and Policy Studies at Yale University, which provided research support but bear no responsibility for the content of this article. This work profited from the advice of many readers, among them Neal Beck, John Freeman, and Renée Smith.

[1] Strictly speaking, findings of aggregate volatility need not invalidate the arguments that adults maintain their partisan identities over long periods and that the principal sources of macropartisan change are generational replacement and "period effects" among the young (see Green and Palmquist 1994).

and influential demonstration that macropartisanship adjusts to fluctuations in the political environment, the work of MacKuen, Erikson, and Stimson (1989, 1992). These two works have been widely cited and justly praised for their path-breaking use of aggregate data, but their statistical analyses have not been examined closely.[2] Reproducing and refining these results is important for several reasons. First, the exercise illustrates the potential value of replication standards in social science (cf. Herrnson 1995; King 1995; Maisel 1995). Here is an instance in which a significant empirical question hangs in the balance, no complications arise due to the use of proprietary or confidential information, and data gathering and analytic procedures are, in principle, entirely reproducible.

Second, other time-series analyses of macropartisanship have produced conflicting results. Abramson and Ostrom (1991), using the biennial National Election Study series, detect little movement in party identification or responsiveness to short-term forces. Whiteley's (1988) Box-Jenkins analysis of the 1984 NES Continuous Monitoring Survey finds no evidence that party identification changes with fluctuating presidential and economic evaluations. Allsop and Weisberg (1988), in contrast, find a significant bivariate correlation between aggregate party identification and vote intention during this period; Weisberg and Smith (1991) find statistically significant links between partisanship and certain lags of unemployment, inflation, and presidential approval in monthly poll data from 1981 to 1986; and Weisberg and Kimball (1995) found that unemployment and approval predicted partisan movement during the Bush administration. Viewed against the backdrop of these other studies, the research by MacKuen and colleagues derives its persuasiveness from the fact that it traces short-term movements in partisanship using quarterly readings of party identification stretching over several decades.

A final reason to replicate the MacKuen et al. (1989) results concerns the sampling procedure used to generate their partisanship series. As MacKuen et al. explain (1989, p. 1140), this series was derived from a subset of Gallup polls: "The quarterly Gallup readings are themselves aggregated from bimonthly readings. Measures of macropartisanship were obtained from the Roper Center as a systematic sampling of party identification from the first Gallup survey of every odd-numbered month. We reaggregated to quarters because key economic indicators are measured quarterly."

After publication of their 1989 article, MacKuen et al. went on to gather data from all the Gallup surveys. Through personal communication with the authors, we learned that the original series used in MacKuen et al. (1989) no longer exists.[3] The data can be reconstructed crudely using the authors' graph of the quarterly values (p. 1138). Because only 138 of 140 data points seem to have been plotted, ruler and compass cannot always discern the quarter associated with each macropartisanship reading. We can, however, reconstruct the variance of the entire series (since this does not rely on the temporal sequencing of the data), estimated to be 20.92. This figure is almost exactly the value (20.93) implied by the summary statistics that MacKuen et al. report (1989, p. 1136).[4] The complete Gallup series, by contrast, has a variance of 16.94 over this same period. It appears that sampling error accounts for 24% of the variance in the macropartisanship series used by MacKuen et al. (1989). Since lagged partisanship functions as a predictor in their analysis, sampling error raises the question of whether published results are biased, a question that can only be assessed by replicating these analyses with the more complete Gallup series.

In the sections that follow, we use the updated Gallup macropartisanship data to replicate as closely as possible the original statistical analysis undertaken by MacKuen et al. (1989). We then cross-validate our results by replicating the analysis of CBS/*New York Times* (CBS/*NYT*) data undertaken by MacKuen et al. (1992). In the process, we come upon another rationale for replication. Only by reproducing analyses reported by others can one appreciate the crucial modeling decisions upon which the results depend. Our critique leads us to move beyond replication, and in the final section we consider alternative time-series models that offer a more parsimonious account of how approval and consumer sentiment influence macropartisanship. We then estimate these models using updated partisanship, presidential approval, and consumer sentiment data. Furthermore, we take pains to show that similar results obtain regardless of whether one employs Box-Jenkins transfer function, partial adjustment, fractional integration, error correction, or vector autoregression models. Our results indicate that economic and political conditions have much less influence on partisanship than initially reported by MacKuen et al. (1989).

THE MEASUREMENT OF MACROPARTISAN CHANGE

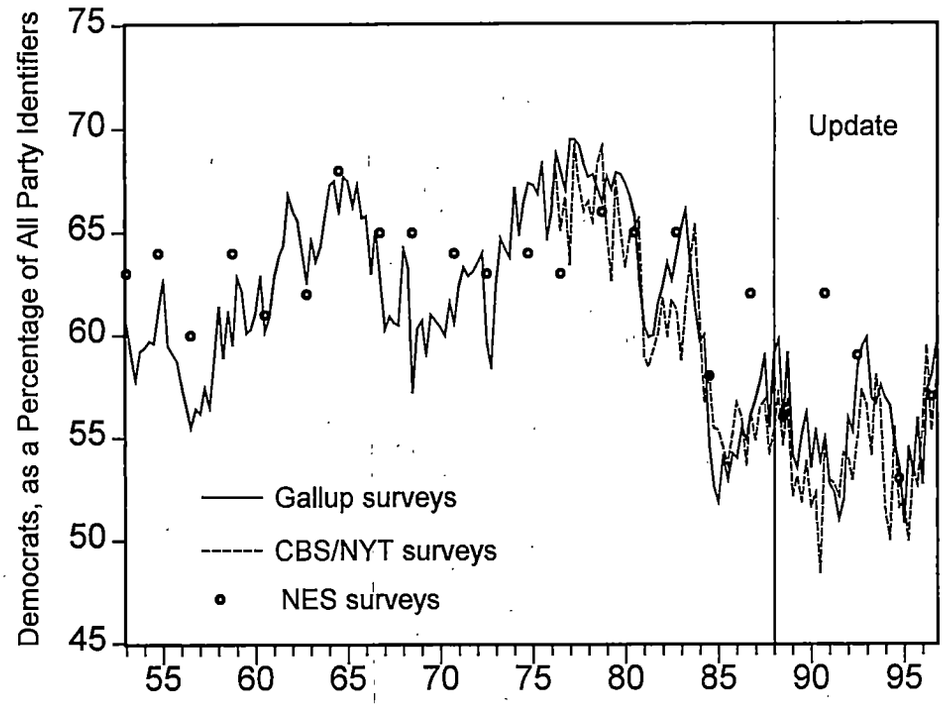
Political scientists had become accustomed to thinking that the most noteworthy trend in the distribution of party identification was dealignment, or growing ranks of self-identified independents, when MacKuen, Erik-

[2] The work that comes closest to replicating the MacKuen et al. (1989) analysis is a methodology text by Cromwell et al. (1994a), which takes as its central example the causal interplay between consumer confidence and macropartisanship. But in reconstructing the MacKuen et al. analysis, the authors fail to take into account the fact that improving economic conditions have a *positive* effect on the macropartisanship series during Democratic administrations and a *negative* effect during Republican administrations.

[3] Using Roper Center documents, we attempted to re-create the series using the monthly Gallup polls and the sampling procedure of MacKuen et al. (1989), but we were unable to reproduce the series they graphed on p. 1138. Because we cannot analyze the precise data used by MacKuen et al., our reanalysis is properly characterized as a replication rather than verification (Herrnson 1995, 452).

[4] Taking the R^2 that MacKuen et al. (1989) report in their Table 3 to be an adjusted R^2, we use the formula $\sigma_e^2/(1 - R^2)$ to obtain 20.93. The statistics that MacKuen et al. (1989) report in their Table N-1 suggest an even higher variance.

FIGURE 1. Macropartisanship: Gallup, CBS/*NYT*, and NES Surveys



son, and Stimson reshaped the terms of debate. Unlike previous students of American party affiliation, these authors relied not on the placid series of biennial NES surveys but instead on a quarterly compilation of polls conducted by the Gallup organization. Besides charting party identification outside the confines of the biennial election season, MacKuen and colleagues sidestepped the dealignment issue by dropping independents from the analysis and focusing on the relative proportions of Democrats and Republicans.

Figure 1 traces the proportion of party identifiers who called themselves Democrats in more than a thousand Gallup surveys conducted between 1953 and 1996. Corresponding figures are also presented from CBS/*NYT* polls (1976–96) and NES surveys (1952–96). Following MacKuen et al. (1989, 1992), we have aggregated individual surveys into quarterly data (see the Appendix for a description of how the updated series was constructed). The partisan balance, as gauged by the Gallup surveys, tips increasingly in the Democratic direction from the early 1950s until the mid-1960s. Some retrenchment occurs until the start of the Nixon administration, when Democrats begin to recapture their former dominance. With the election of Ronald Reagan, Democratic fortunes again wane. The Republican gains, halted momentarily by the recession of 1982–83, continue through the rest of the Reagan administration, so that the series ends at very nearly the same level at which it began.[5]

MacKuen et al. (1989, 1128) regard these fluctuations as strong prima facie evidence against the prevailing view that the partisan balance holds intact until suddenly and permanently altered by an electoral realignment.

> These movements in partisanship are often of a magnitude large enough to suggest electoral realignment. Note that these shifts are not temporary but persist from quarter to quarter. Yet, they have nothing like the permanence envisioned in realignment notions. The partisan balance is not nearly so stable as *The American Voter* or critical realignment theory would lead us to expect.

As Abramson and Ostrom (1991) point out, however, swings in macropartisanship are much less evident in the NES series. Since the mid-1960s, the NES surveys have charted a gradual decline in Democratic identification. The NES trendline slices through the more volatile Gallup series; there is no sign, for example, of one of the most striking shifts in the Gallup series, the surge in Democratic affiliation between 1968 and 1978. When we look at the 22 quarters between 1954 and 1996 for which both Gallup and NES data are available, we find the NES reading of macropartisanship to have a variance of 13.2, as opposed to 19.9 for Gallup.

Why does there seem to be more movement in the Gallup series than the NES series? The list of possibilities is lengthy. In one category are contrasts in sam-

[5] The volatility of the Gallup macropartisanship series in Figure 1 is partly cosmetic. Macropartisanship, as operationalized by MacKuen et al. (1989), is the proportion of all party identifiers who affiliate with the Democrats. Respondents calling themselves independents are discarded from the computation, which contributes to the apparent volatility of the series. Suppose, for example, respondents in Poll A are 34% Democrats, 33% Republicans, and 33% independents. In Poll B, 33% call themselves Democrats, 34% Republicans, and 33% independents. This shift of one percentage point in the proportion of Democrats translates into a 50.75% − 49.25% = 1.5% shift in macropartisanship.

pling design and procedure; few commercial polls have ever approximated the care with which the NES designs and implements its surveys. In another group are hypotheses about the effects of interview context and question wording. The Gallup surveys occur more frequently and therefore may pick up monthly changes in partisanship that would elude a biennial election survey. This feature of the Gallup series puts the partisan stability thesis to an especially demanding test, particularly as the Gallup polls often coincide with the emergence of a salient political issue or concern. One suspects that a respondent's partisan coloration may be swayed after discussing the Iran-Contra or Whitewater scandal during a half-hour interview. Moreover, the Gallup polls have used a measure of party identification that differs from the traditional NES question, which reads: "Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?" The Gallup measure asks: "In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?"

A great deal of controversy surrounds the issue of question wording. Abramson and Ostrom (1991) argue that the short-term focus of "politics as of today" accounts for the greater volatility in the Gallup series. In reply, MacKuen et al. (1992) criticize the NES series as too sparse for meaningful time-series analysis, and they offer instead an analysis based on quarterly aggregations of CBS/*NYT* polls, which date back to 1976 and use the NES wording. As there is no simple way to resolve this debate, any replication effort must inevitably come to terms with both Gallup and CBS/*NYT* series, attending to potential differences between them.

A PRELIMINARY LOOK AT THE DYNAMICS OF MACROPARTISANSHIP

From a statistical standpoint, the issue of macropartisan stability turns on the way in which the current partisan balance is shaped by its past. On the one hand, if partisanship is stable, then a sudden surge in the proportion of Republicans (due, for example, to favorable political events) should leave a long-lasting imprint on the macropartisan balance. Newly minted Republicans will by and large remain Republicans, and the ratio of Democrats to Republicans will return slowly to its long-term mean as this cohort is gradually overwhelmed by others more typical of the two-party balance. On the other hand, if partisan attachments are transitory, then ceteris paribus the macropartisanship series should return quickly to its long-term mean in the wake of a momentary perturbation.

A common technique used to describe the dynamic characteristics of a time series is to graph the correlations and partial correlations between macropartisanship at time t and at $t-k$. Box and Jenkins (1976) point out that series which follow a first-order autoregressive process will tend to show geometrically declining autocorrelations as k becomes large. First-order moving average processes, in contrast, will be suggested by significant partial autocorrelations at lag 2 that decline thereafter. As Figure 2 indicates, Gallup and CBS/*NYT* data produce similar correlograms. The autocorrelations are initially strong (.92 and .87) and decline gradually; both series produce significant partial autocorrelations at lag 2.

The implication is that the two macropartisanship series may be characterized as ARMA(1,1) processes, which is what we would expect to observe if macropartisanship were autoregressive but measured with error.[6] Formally, an ARMA(1,1) model may be written

$$M_t = \mu + \frac{1 - \phi L}{1 - \delta L} u_t, \qquad (1)$$

where u_t is a disturbance term, and L is the lag operator, such that $Ly_t = y_{t-1}$. If we let $\alpha = \mu(1 - \delta)$, we may write this model in a form that is more readily interpretable:

$$M_t = \alpha + \delta M_{t-1} + u_t - \phi u_{t-1}. \qquad (2)$$

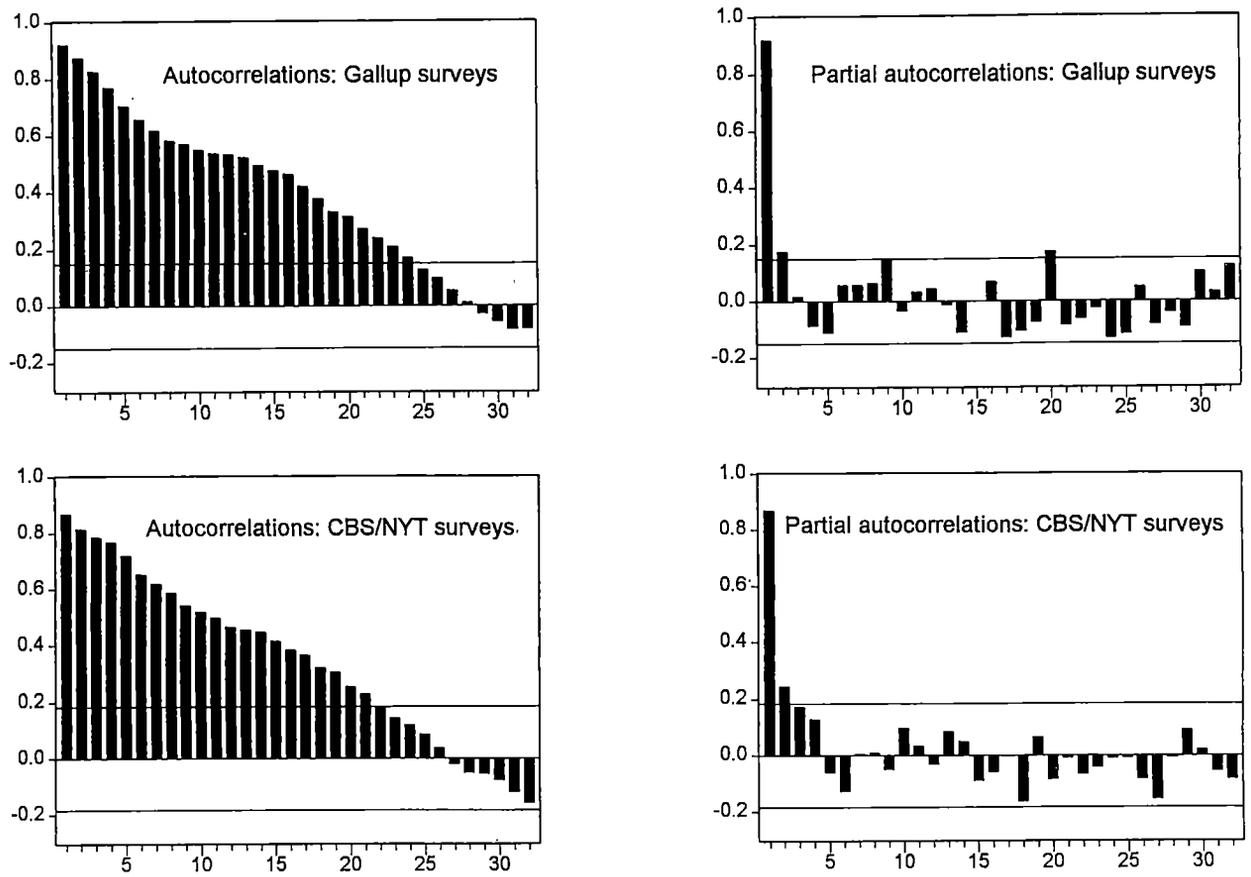
When we estimate this ARMA model, we find $\hat{\delta}$ to be .949 (SE = .026) for Gallup 1953:1–1996:4 and .956 (SE = .027) for CBS/*NYT* 1976:1–1996:4. Moreover, the disturbances are drawn from narrow distributions, the standard deviations of which are 1.82 and 2.36, as shown in Table 1.

What do these results tell us about the character of macropartisan change? The fact that δ is close to one means that partisanship reverts to its mean very gradually over time. Knowing, for example, that macropartisanship stands one unit above its long-term mean today implies that one may expect it to be $.95^{16} = .44$ units above its mean four years from now. This feature of macropartisanship distinguishes it from other sorts of public opinion time series, which return to their means within a matter of months or even days. Because any change in M_t will influence M_{t+1}, a string of consecutive positive or negative disturbances can produce a gradual but substantial movement in macropartisanship. For example, if the u_t were to remain at $-.05$ indefinitely, then the result for $\delta = .95$ would be a shift of $-.05/(1 - \delta) = -.05/.05 = -1$ unit in the expected value of M_t.

If δ were exactly 1, then disturbances to the macropartisanship series would *never* decay; partisanship would be a random walk, or "integrated" series. Because integrated series present a range of statistical complications, scholars have expressed concern that macropartisanship might be a random walk (Box-Steffensmeier and Smith 1996; Cromwell, Labys, and Terrazza 1994b), but a number of empirical and theoretical considerations suggest otherwise. Not surprisingly, given their low power, direct tests using aggre-

[6] As Beck (1991) points out, sampling variability in an autoregressive series will tend to introduce a moving average component. Because CBS/*NYT* interviews fewer people in a typical quarter than Gallup, its moving average component turns out to be somewhat stronger (see Table 1). Note that the consequences of sampling error tend to be overlooked in studies that have charted monthly movements in macropartisanship, often based on just a single poll reading at each point (Weisberg and Kimball 1995; Weisberg and Smith 1991).

FIGURE 2. Correlograms of Gallup and CBS/*NYT* Macropartisanship



Note: The Gallup data cover the period 1953:1 to 1996:4 (N = 176). The CBS/*NYT* data cover 1976:1 to 1996:4 (N = 84).

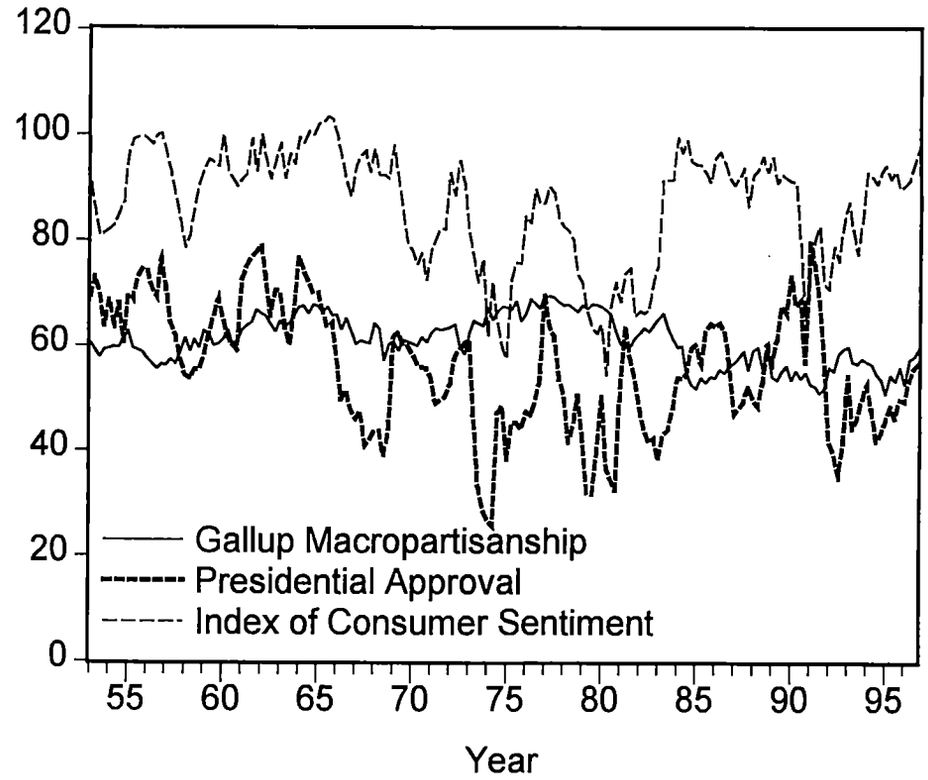
gate data speak equivocally to the null hypothesis that $\delta = 1$. The augmented Dickey-Fuller test for the Gallup series from 1953 to 1996 with four lags is −2.46, which does not reach the $p < .10$ threshold; the corresponding Phillips-Perron test is −2.80, which does achieve $p < .10$. Simulations, however, show that when u is distributed normally with standard deviation 1.82 (as in Table 1), an integrated series that starts at 60% Democratic has less than a 5% chance of remaining within the bounds of 49–70% Democratic, based on quarterly observations since 1945. The limited range of the macropartisanship series comports with theoretical intuitions about the constraints of two-party competition. To say that the series has a unit root (i.e., $\delta = 1$) would imply that its variance is infinite, which in turn implies the implausible notion that the Democratic or Republican party could win over the entire partisan electorate (Alvarez and Katz 1996). Again, simulations show that if macropartisanship had been an integrated series with a value of 60% in 1945, then the odds would be one in three that at some point one party would have won more than 80% of the partisan identifiers.

TABLE 1. Univariate ARMA Models for Gallup and CBS/*NYT* Macropartisanship

	Gallup (1953:2–1996:4)	CBS/*NYT* (1976:2–1996:4)
Constant	60.800	55.313
standard error	(2.181)	(3.407)
AR(1)	.949	.956
standard error	(.026)	(.027)
MA(1)	−.189	−.515
standard error	(.081)	(.105)
Adjusted R^2	.850	.806
Standard error of estimate	1.821	2.363
Number of cases	175	83

Univariate analysis helps illuminate some of the basic dynamic properties of the macropartisanship series, in particular, the gradual pace at which it changes and returns to equilibrium. But many questions remain unanswered. What sorts of variables reside in the disturbance term? Can we identify systematic causes of quarter-to-quarter change in party identification? How much of the movement in macropartisanship over time can be attributed to these short-term forces?

FIGURE 3. Macropartisanship, Consumer Sentiment, and Presidential Approval, 1953:1–1996:4



THE EFFECT OF SHORT-TERM FORCES ON MACROPARTISANSHIP

MacKuen et al. (1989, 1992) argue that the aggregate distribution of party identification shifts considerably in response to fluctuations in consumer confidence and presidential approval. The more popular a sitting president, the more attractive is his party as an object of identification. This hypothesis can be tested by tracking inter- or intraadministration changes in presidential approval ratings (e.g., the transition from a disgraced Nixon in 1974 to a more popular Gerald Ford, or from the triumphant Reagan of 1984 to the scandal-beset Reagan of 1987). In addition, MacKuen et al. maintain that parties are rewarded or punished for their economic stewardship. The more buoyant the public's view of the economy, the more attractive the incumbent president's party.

MacKuen et al. (1989) examine macropartisanship, presidential approval, and consumer sentiment on a quarterly basis for the period 1953–88. Approval ratings are compiled from Gallup polls, aggregated quarterly. Economic perceptions are measured using the Michigan Index of Consumer Sentiment.[7]

Figure 3, which charts consumer sentiment, presidential approval, and macropartisanship back to 1953, may be viewed as a brief synopsis of the economic and political vicissitudes of the postwar era. As extensive scholarship on the links between economic performance and presidential approval would lead us to expect, incumbent popularity suffered during the recessions of the late 1950s, mid-1970s, and early 1980s. The relationship between macropartisanship and the other two variables is a bit more subtle. The variability in the macropartisanship series is relatively small, and only some of the dramatic shifts in party fortunes led to macropartisan adjustment. The decline of the Democrats during the Johnson and Reagan administrations did coincide with shifts in presidential approval, but the precipitous drop in Nixon's popularity following his landslide election victory in 1972 produced only a modest increase in Democratic identification, and Carter's fall from public favor in 1977–78 led to small gains for the Republicans. This cursory examination cannot, of course, substitute for a more rigorous quantitative assessment, but it does structure our intuitions about what a statistical analysis is likely to conclude. It would be somewhat surprising to encounter statistical evidence implying that partisanship moves abruptly in response to swings in economic and political circumstances. Since that is what MacKuen et al. conclude, it

[7] In contrast to Weisberg and Smith (1991) and Weisberg and Kimball (1995), MacKuen et al. use consumer perceptions rather than objective economic indicators, such as inflation or unemployment rates. Although we do not report the results here, we find very little systematic relationship between objective economic indicators and Gallup macropartisanship. For a description of the survey questions used to build the Index of Consumer Sentiment and its relationship to objective economic indicators, see Matsusaka and Sbordone (1995).

is important to retrace their steps to see how their methods may tend to produce this result.

REPLICATION OF THE MACKUEN ET AL. (1989) STUDY

MacKuen et al. (1989) rely on two time-series analyses to establish that partisanship changes in the wake of economic conditions and presidential popularity. First, they develop and estimate an elaborate Box-Jenkins time-series model (tables 2 and 3, pp. 1135–6) in which approval and economic perceptions produce immediate as well as delayed effects on macropartisanship. That analysis is then repeated using OLS regression (pp. 1140–1) in an effort to assure readers that these findings are not reducible to "mere technical wizardry" (p. 1141). Again, partisanship is found to change in response to swings in approval and consumer confidence.

The Box-Jenkins and OLS analyses, despite some notable differences, share certain key specification assumptions. Macropartisanship is modeled as a function of consumer sentiment, presidential approval, and an extensive set of variables representing historical events and administration effects. Each administration, for example, is represented by both a dummy variable and a "template that begins with a score of one at the president's (initial) inauguration and then declines exponentially throughout his tenure in office" by a prespecified rate (p. 1133). In addition, dummy variables mark the beginning of each new administration. This brings the total number of administration control variables to 19, plus 5 variables used to mark the Iran hostage crisis, Vietnam, and other noteworthy events.

The authors say very little about why these 24 control variables belong in a model of macropartisanship, but this choice of model specification turns out to be crucial. From both a substantive and econometric standpoint, administration variables may be necessary when modeling presidential approval, because one is splicing together what are in effect distinct series for different presidents, but no such logic applies to macropartisanship. Indeed, one observes neither spikes in the series when presidents assume office nor patterns of Democratic surges in identification when a Democrat controls the White House. The cost of including an ad hoc set of dummy variables marking different periods, without theoretical justification, is that one risks overfitting the model and undercutting the apparent autoregressive character of macropartisanship (McCleary and Hay 1980). In cross-sectional analysis, irrelevant regressors are benign; in time-series analysis, irrelevant period variables introduce bias.

This point is driven home by means of a simple simulation experiment. Suppose one generates a fictitious AR(1) series for the period 1953:1–1988:1 using the formula

$$y_t = 3 + .95y_{t-1} + u_t,$$

where y_0 equals 60, and u_t is distributed i.i.d. $N(0,1)$. When one regresses the fictitious y_t on y_{t-1} and the 19 administration variables used by MacKuen et al. (all of which, of course, have a true coefficient of zero), the average estimated slope for y_{t-1} is .71 over 1000 replications. This estimate is a far cry from the true parameter of .95, and when the five event variables are added, this average slope drops to .68. Overfitting is apparent as well from the fact that the average error variance is .88, well below the true variance of 1. In effect, the inclusion of irrelevant predictors derails the estimation of this autoregressive model.

When this experiment is repeated using actual macropartisanship data (1953:1–1988:1), the same pattern turns up. Regressing M_t on M_{t-1}, one obtains an autoregressive coefficient of .91 (SE = .036), which rises to .93 (SE = .034) when an MA(1) term is included to account for sampling error in the data. When the latter model is reestimated with the 24 control variables (Z) in the equation

$$y_t = \gamma_0 + \gamma_1 y_{t-1} + Z\beta + u_t(1 - \phi L), \qquad (3)$$

the autoregressive coefficient drops to .73 (SE = .084).

Notwithstanding these simulation results, one may object that good substantive grounds exist for expecting macropartisanship to move in response to international events, elections, and different presidential administrations. *Note, however, that the 24 control variables as a group do not have a statistically significant total effect on macropartisanship.* The sum of squared residuals from the estimation of equation 3 is 312.29, and the exclusion of the 24 regressors raises this sum to 406.15 ($\chi^2_{(24)} = 29.2, p > .10$). By contrast, each of the transition, honeymoon, event, and administration variables contributes significantly to the prediction of presidential approval. For example, the 18 honeymoon, event, and administration regressors reduce the sum of squared residuals from 2937.3 to 1328.2 ($\chi^2_{(18)} = 127.3, p < .05$). The contrast between party identification and presidential approval is precisely what *The American Voter* would lead us to expect. Events and changes in personae that profoundly shape presidential popularity have no total effect on macropartisanship.

Interpreting the MacKuen et al. (1989) results requires also that one attend to the manner in which they operationalize presidential approval. In a preliminary analysis, it is modeled as a function of consumer sentiment, historical events, and administration effects; these estimates are then used to purge approval of the variance attributable to consumer sentiment. Thus, when MacKuen et al. refer to "political approval" they have in mind presidential approval "minus that part of approval forecast from the economic component alone, with other parts of the model zeroed out" (p. 1140, note 10). The purged approval variable credits consumer confidence for the influence it exerts indirectly through presidential approval. The intent, in other words, is to estimate the *total effect* of consumer sentiment on macropartisanship.

We begin our replication by taking up the simplest of the MacKuen et al. analyses, the OLS regression presented in their Appendix. Let M_t represent macropartisanship, let A_{t-1} denote lagged "political" ap-

proval, and C_{t-1} lagged consumer sentiment. Call Z a matrix of administration and event variables and u_t a white noise disturbance. The OLS model estimated by MacKuen et al. may be written:

$$M_t = \delta M_{t-1} + \gamma_1 A_{t-1} + \gamma_2 C_{t-1} + Z\beta + u_t;$$

$$M_t - \delta M_{t-1} = \gamma_1 A_{t-1} + \gamma_2 C_{t-1} + Z\beta + u_t;$$

$$(1 - \delta L)M_t = \gamma_1 A_{t-1} + \gamma_2 C_{t-1} + Z\beta + u_t;$$

$$M_t = \frac{\gamma_1}{1 - \delta L} A_{t-1} + \frac{\gamma_2}{1 - \delta L} C_{t-1} + \frac{1}{1 - \delta L}(Z\beta) + \frac{u_t}{1 - \delta L}. \quad (4)$$

MacKuen et al. present statistically significant, although not substantively large, effects for both purged approval (γ_1 = .13) and consumer sentiment (γ_2 = .10). Surprisingly, given our earlier univariate analysis, the decay parameter δ (the coefficient for lagged partisanship) is estimated to be just .26. Taken together, these estimates suggest that partisan stability is something of an illusion: Net of the effects of the economy and approval, party identification is only weakly propelled by its past values.[8]

Our efforts to replicate this regression analysis proved successful for the model in which presidential approval is the dependent variable. As shown in Table 2, the lagged effects of consumer sentiment and approval are .37 (versus .29) and .38 (versus .31), respectively. Our R^2 is .93; theirs is .94. When the dependent variable becomes the comprehensive Gallup macropartisanship series, however, each of the specifications we tried produced results that were weaker than those reported by MacKuen et al. The effect of political approval (i.e., purged of consumer sentiment) is small and statistically insignificant. Replacing political approval with presidential approval increases the estimate, but it remains half the size of that obtained by MacKuen et al. The effect of consumer sentiment seems more robust, particularly when approval is omitted from the regression equation. The most striking contrast between the original results and our replication concerns the effects of lagged partisanship. Our estimate (.53) is twice what was reported originally, perhaps due to the lower reliability of the original macropartisanship series.

When the slew of control variables is pared down to include just a dummy for Democratic administrations and transitions between administrations,[9] lagged presidential approval has no discernible effect on macropartisanship. The effects of lagged consumer sentiment are statistically significant but small (β = .03, SE = .01). Meanwhile, the influence of lagged macropartisanship grows to .84. These estimates suggest that only large shifts in consumer sentiment sustained over long stretches of time would lead to appreciable movement in macropartisanship.

The Box-Jenkins model resembles the OLS model with a few technical embellishments. Whereas the OLS analysis stipulates just one dynamic parameter (δ), MacKuen et al.'s Box-Jenkins model uses three dynamic parameters to describe the rate at which changes in the political environment decay over time. Macropartisanship (M_t) is now expressed as a decaying function of *contemporaneous* approval (A_t) and consumer sentiment (C_t), a matrix of administration and event variables (Z), and an AR(1) disturbance (u_t):

$$M_t = \frac{\gamma_1}{1 - \delta_1 L} A_t + \frac{\gamma_2}{1 - \delta_2 L} C_t + Z\beta + \frac{u_t}{1 - \delta_3 L}, \quad (5)$$

where L again represents the backshift operator, such that $LA_t = A_{t-1}$.

A dynamic parameter value of zero implies that an independent variable's influence persists for just one quarter. Conversely, a value of 1 means that an independent variable's influence is felt each and every subsequent period, without decaying over time. In between these extremes, a parameter of .5 implies that an effect is full for the first period, halved for the next, halved again, and so on. When an independent variable undergoes a permanent one-unit shift, the cumulative long-term effect—given an immediate influence of γ_k and a dynamic parameter δ_k—would be $\gamma_k/(1-\delta_k)$.

Traditional conceptions of partisanship lead one to expect each dynamic parameter to be near one (perturbations to the partisan balance die out slowly), but MacKuen et al. (1989) discover that the three estimated dynamic parameters vary markedly. MacKuen et al. obtain a large dynamic parameter estimate for consumer sentiment (.84), which suggests that changes in economic perceptions continue to influence macropartisanship long after they occur (p. 1137). Approval seems to have less staying power (.35), but there is again clear evidence of gradually dissipating influence over time. Finally, the AR(1) coefficient (δ_3) takes on the remarkable value of −.04. This estimate suggests that consumer sentiment, political approval, and the assorted administration variables account for the persistence in the macropartisanship series seen earlier in Table 1; other influences on partisanship evaporate quickly. Again, the apparent persistence of partisanship over time merely seems to reflect the persistent effects of its causal antecedents, consumer sentiment and presidential approval.

Our replication begins with the Box-Jenkins model of presidential approval. With the exception of certain administration effects, most coefficients are similar to those reported by MacKuen et al. Particularly close are the estimated coefficients for the immediate effect and dynamic parameter associated with consumer sentiment. A shift of one percentage point in consumer

[8] MacKuen et al. regard the OLS results as confirming their thesis about partisan changeability, but in fact these estimates suggest the limited influence of economic and political perceptions. While their Box-Jenkins analysis (see below) suggests that a permanent one-point shift in consumer sentiment eventually produces a .63 adjustment in macropartisanship, their OLS results imply an adjustment of .14.

[9] Administration transition dummies are included because consumer sentiment and approval are lagged one quarter.

TABLE 2. Replication of MacKuen et al. (1989): OLS Models of Macropartisanship and Presidential Approval (1953:2–1988:1)

	Results Reported by MacKuen et al.		Replication				
	Presidential Approval	Gallup Macropartisanship	Presidential Approval	Gallup Macropartisanship (t)			
Independent Variables	(t)	(t)	(t)	(i)	(ii)	(iii)	(iv)
Presidential Approval ($t-1$)	.31		.38		.07		
standard error	(.05)		(.05)		(.03)		
Consumer Sentiment ($t-1$)	.29	.10	.37	.08	.06	.09	.03
standard error	(.04)	(.03)	(.05)	(.03)	(.03)	(.02)	(.01)
Macropartisanship ($t-1$)		.26		.53	.48	.58	.84
standard error		(.09)		(.08)	(.09)	(.07)	(.05)
"Political" Approval ($t-1$)		.13		.04			.01
standard error		(.04)		(.04)			(.02)
R^2	.94	.81	.93	.88	.88	.88	.85
Number of cases	139	139	139	139	140	140	139

Note: "Political" approval is presidential approval minus .29 times lagged consumer sentiment (see MacKuen et al. 1989, 1140–1). Regression equations include administration dummies, dynamic administration effects, and event series shown in tables 1–2 of MacKuen et al. In addition, the model of macropartisanship includes dummy variables scored 1 during a new president's first quarter. Without these transition dummies, the estimated effects of political approval and consumer sentiment are negligible: .02 (SE = .03) and −.01 (SE = .01), respectively. Replication (iv) presents estimates from a model that omits all dummies for events and administrations; this specification includes a dummy for Democratic control of the presidency and dummies marking the start of each administration.

sentiment results in an immediate .33 change in presidential approval, followed by effects that decline by a factor of .64 thereafter. Comparable estimates obtained by MacKuen et al. are .32 and .61, respectively.

After constructing the "political approval" variable (using the parameter estimates obtained by MacKuen et al.), we attempted to reproduce the results reported in Table 3 of MacKuen et al. (1989). We included all the regressors in their Table 2, not just the statistically significant predictors reported in their Table 3. Replicating the analysis with just the subset of regressors listed by MacKuen et al. in their Table 3 drives the estimated effects of consumer sentiment and approval to nil. What we present here are the *maximal* estimates of short-term effects that we uncovered during our efforts to replicate the MacKuen et al. model.

Table 3 compares our results to those obtained by MacKuen et al. In replication 1, we come fairly close. The dynamic coefficients are approximately the same, suggesting that the effects of a perturbation in the consumer sentiment series stretch over years. The decay in the effects of approval occurs more rapidly, as MacKuen et al. suggest. The principal difference between our results and theirs lies in the magnitude of the contemporaneous effects. Our coefficient for approval is half of what they estimated (.117 versus .22); for consumer sentiment, two-thirds (.066 versus .10). Another difference is that we find a stronger AR(1) coefficient, although this estimate is still much lower than that observed in our Table 1, because the MacKuen et al. model includes dummy variables for each administration. Finally, and not without important implications for the argument that partisanship wavers amid shifting party fortunes, we find no significant direct effects of historical events on macropartisanship.

How do the results change when the model is respecified so as to eliminate the 24 administration, transition, template, and event variables? To answer this question, we reestimated the Box-Jenkins transfer function model using a mean-centered version of consumer sentiment or presidential approval, described in the Appendix. These analyses also introduce an MA(1) parameter to account for sampling error in the macropartisanship series. Although this nuance has little influence on the key parameters of interest, it remedies indications of autocorrelation among the residuals. The respecified model linking consumer sentiment and macropartisanship, for example, may be written:

$$M_t = \frac{\gamma}{1-\delta_1 L} C_t + \frac{(1-\phi L)u_t}{1-\delta_2 L}. \qquad (6)$$

The results of these analyses diverge markedly from the original estimates. First, the immediate influence of a change in the economic or political environment is found to be very small. The total effect of a one-unit shift in the Index of Consumer Sentiment is .029, while the direct effect of a one-point shift in approval is .059. Given these parameter estimates, only dramatic and sustained swings in political or economic fortunes would produce sizable short-run changes in macropartisanship. Second, our results paint a very different picture of long-term dynamics. MacKuen et al. find that for consumer sentiment δ_1 is large, while δ_2 is close to zero. In replication 6, the dynamic parameters associated with the AR(1) term and the effects of consumer sentiment are nearly identical (.91, .94). Far from explaining away the persistence of partisanship, the dynamic effects of consumer sentiment are what one would expect from a partial adjustment model in which $\delta_1 = \delta_2$:

TABLE 3. Replication of MacKuen et al. (1989): Macropartisanship as a Function of Contemporaneous Consumer Sentiment, Presidential Approval, Events, and Administration Dummies (1953:2–1988:1)

	Results Reported by MacKuen et al.	#1	#2	#3	#4	Lagged Regressors #5	Omits Events, Transitions, Administration Variables #6	#7
Consumer Sentiment								
Immediate Effect	.10	.066	.065	−.031		.051	.029	
standard error	(.01)	(.011)	(.012)	(.031)		(.035)	(.011)	
Dynamic Parameter	.84	.849	.849	.501		.039	.943	
standard error	(.02)	(.008)	(.010)	(.335)		(.137)	(.038)	
"Political" Pres. Approval								
Immediate Effect	.22	.117				.061		.059
standard error	(.04)	(.043)				(.040)		(.018)
Dynamic Parameter	.35	.338				.259		.684
standard error	(.09)	(.224)				(.489)		(.134)
Presidential Approval								
Immediate Effect				.172	.155			
standard error				(.041)	(.036)			
Dynamic Parameter				.309	.242			
standard error				(.135)	(.108)			
AR(1)	−.04	.345	.464	.441	.459	.607	.911	.937
standard error	(.10)	(.098)	(.089)	(.096)	(.089)	(.082)	(.040)	(.034)
MA(1)							−.172	−.247
standard error							(.097)	(.095)
Standard error of estimate	1.83	1.517	1.586	1.595	1.592	1.714	1.680	1.649
Q statistic, lags 1–6		3.15	4.53	4.22	5.56	6.89	6.10	4.89
degrees of freedom		5	5	5	5	5	4	4
p value		(.68)	(.48)	(.52)	(.35)	(.23)	(.19)	(.30)

Note: Omitted are parameters for dummy variables associated with each president, transitions between presidents, and templates that specify the declining effect of these transitions over time. Also omitted are coefficients associated with five event series used by MacKuen et al. (Vietnam troop levels, the attempted assassination of Reagan, Watergate, the Iran hostage crisis, and omnibus event series). Replication 5 substitutes first-order lags of consumer sentiment and political approval. Replications 6 and 7 omit event variables, administration dummies, templates, and dummies marking the start of new administrations. Consumer sentiment and political approval in these last two equations have been generated by taking the residuals from a regression in which each series (multiplied by −1 for quarters during which Republicans occupy the White House) is regressed on a dummy variable scored 1 for quarters during a Democratic administration. See Appendix for discussion of this transformation.

$$M_t = \frac{\gamma}{1-\delta L} C_t + \frac{(1-\phi L)u_t}{1-\delta L} = \delta M_{t-1} + \gamma C_t + (1-\phi L)u_t. \quad (7)$$

In effect, the influence of consumer sentiment extends over a long period due to the autoregressive, or self-replicating, nature of partisanship. Replication 7 in Table 3 shows the dynamics of approval to be somewhat *weaker* than that of the error process (.68, .94), though a Wald test of the equality of these two parameters cannot reject the null at .05 ($F(1, 138) = 3.28$, $p > .05$).

In sum, the Box-Jenkins analysis reported by MacKuen et al. exaggerates the degree to which partisanship responds to short-term influences. In part, their findings reflect the idiosyncracies of their original macropartisanship series; when their analysis is replicated using a complete inventory of Gallup surveys, consumer sentiment and presidential approval are found to have less influence, and lagged partisanship more. Perhaps more important than the construction of the macropartisanship series is the way MacKuen et al. specify their ARMA model. Their analysis of 139 observations requires the estimation of 29 parameters, 23 of which are associated with variables that single out particular observations or groups of successive observations. Overfitting the data in this manner distorts the dynamics of the partisanship series. Throw out the assortment of statistically insignificant and theoretically questionable control variables, and the results change dramatically: Partisanship is strongly determined by its past and responds to political or economic fortunes in a limited and gradual fashion.

CROSS-VALIDATION USING CBS/*NYT* POLLS

In an effort to disprove Abramson and Ostrom's (1991) claim that the Gallup series moves with short-term changes in the political environment due to the im-

TABLE 4. Replication of MacKuen et al.'s (1992) Analysis of Macropartisan Change Using CBS/*NYT* and Gallup Data (OLS Estimates)

Independent Variables	1981:2–1988:4 Results Reported by MacKuen et al.	Replication 1981:2–1988:4[a]	Replication 1976:2–1996:4[b]	Replication 1976:2–1996:4[c]
		Dependent Variable: CBS/*NYT* Macropartisanship		
CBS/*NYT* Partisanship, $t-1$	.62	.55	.390	.865
standard error	(.14)	(.14)	(.107)	(.061)
Presidential Approval $t-1$	.09	.09	.047	.026
standard error	(.06)	(.06)	(.031)	(.034)
Consumer Sentiment, $t-1$	.07	.05	.057	−.001
standard error	(.04)	(.04)	(.029)	(.030)
Adjusted R^2	.65	.64	.837	.784
Number of cases	26	29	83	83
Standard error of estimate	1.84	1.84	2.177	2.490
		Dependent Variable: Gallup Macropartisanship		
Gallup Partisanship, $t-1$	.53	.53	.654	.924
standard error	(.14)	(.16)	(.088)	(.046)
Presidential Approval, $t-1$	.10	.09	.043	.010
standard error	(.07)	(.07)	(.028)	(.027)
Consumer Sentiment, $t-1$	.12	.12	.054	.023
standard error	(.04)	(.04)	(.024)	(.022)
Adjusted R^2	.78	.80	.900	.887
Number of cases	26	29	83	83
Standard error of estimate	1.84	1.77	1.754	1.865

Note: The second column in the lower panel uses Gallup macropartisanship data supplied by MacKuen et al. The third and fourth columns use the MacKuen et al. series through 1988:1; for 1988:2–1996:4, Gallup macropartisanship scores are derived from quarterly aggregations of Gallup telephone surveys, adjusted for pro-Republican bias (see text).
[a]1981:4 and 1982:1 are missing.
[b]Regressors include dummy variables for administration and first quarter in office.
[c]Regressors include first quarter and Democratic presidency dummies.

mediate focus of its party identification measure, MacKuen et al. (1992) replicated their results using CBS/*NYT* polls, which use the NES question wording. These polls date to 1976, but MacKuen et al. (1992) analyze only the 32 quarters of the Reagan administration. Apparently due to the paucity of observations, the authors rely exclusively on an OLS regression analysis and restrict the model of macropartisanship to three lagged regressors: macropartisanship, consumer sentiment, and presidential approval (in its original unpurged form).

Table 4 compares our results and those obtained by MacKuen et al. (1992).[10] Using the CBS/*NYT* polls, our analysis produces the same effect for approval and a slightly weaker effect for consumer sentiment. The Gallup estimates are nearly identical to those reported by MacKuen et al. (1992).

[10] Exact replication of the results given in MacKuen et al. (1992) is hampered by the fact that they report an analysis using just 26 of the 32 cases, namely, those in which "both Gallup and CBS-*Times* measures of macropartisanship are measured for times t and $t-1$" (p. 479). The first-quarter observation case is lost when the data are lagged one period. Another two cases are lost because no CBS/*New York Times* polls were conducted during the fourth quarter of 1981. This leaves 29 cases. The authors neglect to mention which of the remaining quarters were omitted. We tried every possible combination of 26 cases but could not replicate exactly the published results. We came closest by dropping 1982:4 and 1988:4, but the Gallup results remained more or less as we report them.

These results corroborate the OLS analysis undertaken in MacKuen et al. (1989) in that the estimated effects of short-term forces are quite modest. Indeed, the estimates become more modest still when the analysis is replicated over a wider horizon, 1976:1–1996:4. The longer time span not only speaks to the generalizability of the earlier results but also addresses a nagging statistical issue, the small-sample bias of autoregressive models. As Table 4 makes clear, the addition of about three dozen new cases reduces the apparent effects of lagged presidential approval and consumer sentiment, even when one includes the scheme of administration variables used by MacKuen et al. (1989). Of the two short-term forces, only economic perceptions have a statistically reliable, though small, instantaneous effect (.057, .054) on macropartisanship. Absent the four administration dummy variables, even these small effects disappear.

We do not read these results to mean that partisanship is altogether unswayed by economic or political circumstances. The steady stream of small coefficients suggests that presidential popularity and consumer confidence bear some systematic relationship to the party balance. At the same time, it is important to keep in mind, particularly when discussing the Reagan era, how limited that relationship tends to be.

Time and again, statistical case studies have charted

the memorable surge in GOP identification at the end of the first Reagan term. In spring 1983, Reagan's approval rating was a mere 38%; consumer confidence was a dismal 75; and 65% of party identifiers were Democrats. By spring 1985, with Reagan's popularity at 60% and consumer confidence at 95, macropartisanship dropped to 52%—the largest short-run change in party identification ever recorded in American surveys. This sequence of events paints a vivid image, but viewed against the backdrop of the partisan movement since 1953, a shift of this magnitude is much larger than expected. The estimates in the last columns of tables 2–4, for example, imply that no more than half the observed movement in macropartisanship can be attributed to economic recovery and rebounding popularity. The remainder is due to a succession of negative residuals that perhaps reflects the transformation of southern party organization and identification during this period (Green and Schickler 1996; Hadley and Bowman 1995; Miller 1991). To attribute the abrupt shift in the partisan balance under Reagan to economic perceptions or presidential popularity (cf. Weisberg and Smith 1991) makes problematic the many instances when similar changes in political climate failed to produce partisan change (e.g., macropartisanship was 55% in spring 1990 and remained at 56% two years later, despite a decline in approval from 68% to 42% and a drop in consumer sentiment from 91 to 71). One may argue that macropartisanship was unusually responsive to short-term influences during the Reagan years, but statistical tests of structural change fall well short of significance ($p > .25$) across a variety of specifications. By focusing solely on the Reagan era, the MacKuen et al. (1992) analysis not only encounters problems of small sample size but also loses the broad historical scope that makes their 1989 study so attractive.

REVISED MODELS AND UPDATED DATA

To this point, we have worked within the modeling framework laid out by MacKuen, Erikson, and Stimson in an effort to gauge the robustness of their results. We have found that their conclusions draw strong support only from their Box-Jenkins analysis, which hinges on certain questionable modeling assumptions. Since there are almost as many time-series modeling approaches as there are data points in the macropartisanship series, we now consider how our results might change if analyzed in a different manner. Using data from 1953 through the fourth quarter of 1996, we briefly summarize results obtained from partial adjustment, error correction, fractional integration, and vector autoregression models. In each case, we mean-center the independent variables, contemporaneous approval and consumer sentiment, using the procedures described in the Appendix. This transformation obviates the need for a dummy variable marking Democratic administrations, although it should be emphasized that the results do not change if such a dummy variable is used. We also include an MA(1) term to accommodate sampling error in the macropartisanship series.

The partial adjustment model is a special case of the transfer function model in equation 6, with δ_1 constrained to equal δ_2. This model in effect characterizes partisanship as a self-perpetuating attitude that adjusts to accommodate new information about political and economic performance. Taking consumer sentiment as a surrogate for short-term forces, the model may be written as

$$M_t = \delta M_{t-1} + \gamma C_t + (1 - \phi L)u_t. \qquad \textbf{(8)}$$

The error correction model is an extension of the partial adjustment specification and allows for the possibility that macropartisanship and predictors such as consumer sentiment are cointegrated or bear an equilibrium relationship to one another. One may argue, for example, that when economic confidence drops below a level that supports a certain level of partisanship, the disequilibrium is gradually corrected. If this long-term relationship is expressed as

$$M_t = \gamma C_t + \varepsilon_t,$$

then the expected change in partisanship at the current period (ΔM_t) is a function of both last period's disequilibrium ($M_t - \gamma C_t$) and recent changes in the explanatory variable (ΔC_t). This implies an error correction model of

$$\Delta M_t = \nu \Delta C_t - \alpha(M_{t-1} - \gamma C_{t-1}) + u_t.$$

Rearranging terms shows that this model augments the partial adjustment specification by introducing C_{t-1} as a predictor:

$$M_t = (1 - \alpha)M_{t-1} + \nu C_t + (\gamma - \nu)C_{t-1} + u_t. \qquad \textbf{(9)}$$

The fractional integration model has been suggested as a way of coping with problems of aggregation. Box-Steffensmeier and Smith (1996) point out that if each individual's partisanship follows an autoregressive process with varying β_i parameters, so that

$$M_{ti} = \beta_{ti} M_{t-1,i} + u_{ti}, \qquad \textbf{(10)}$$

Then the resulting macropartisanship series may be fractionally integrated. A fractionally integrated series of order d may be written as

$$(1 - B)^d M_t = \varepsilon_t, \qquad \textbf{(11)}$$

which for $.5 < d \leq 1$ may be expressed as an AR (∞) series:

$$\Delta M_t = d\Delta M_{t-1} + \frac{1}{2!} d(1 - d)\Delta M_{t-2} + \frac{1}{3!} d(1 - d)(2 - d)\Delta M_{t-3} + \ldots \varepsilon_t \qquad \textbf{(12)}$$

(Hamilton 1994, p. 449).

Fractionally integrated processes have "long memory" in that each disturbance decays very slowly. To get some sense of how slowly, consider the estimate of $d = .83$ obtained by Box-Steffensmeier and Smith (1996) in

TABLE 5. Rates of Partisan Change Implied by Alternative Time-Series Models

	Average Absolute Quarterly Change in Gallup Macropartisanship, 1953–96, Caused by Observed Fluctuations in . . .		Expected Number of Quarters before Gallup Macropartisanship Would Change 10 Points in Response to a Permanent Change of . . .	
	Consumer Sentiment	Presidential Approval	−31 Points[a] in Consumer Sentiment	−30 Points[a] in Presidential Approval
Box-Jenkins Model of MacKuen, et al. (1989)	.68	1.09	5	5
Partial adjustment	.25	.36	16	13
Fractional integration	.16	.18	25	25
Vector autoregression (four lags)	.25	.36	21	Inf.
Error correction	.25	.46	16	13

[a]Figures correspond to shifts from these series' historical means to their lowest observed levels.

their analysis of Gallup macropartisanship from 1953 to 1992. This estimate implies that a one-point disturbance in the first quarter of 1953 continues to boost the macropartisanship series by .36 in 2000. By comparison, a coefficient of .95 in a simple AR(1) model implies that a shock leaves virtually no trace after two decades. In this sense, Box-Steffensmeier and Smith's results go farther than ours in emphasizing the lasting consequences of partisan change.

The attraction of ARMA and ARFIMA models is that they characterize the dynamics of a time series in terms of just a few parameters. The trade-off, however, is that these models impose strong assumptions about the lag structure and rate at which causal influences die out over time. Vector autoregression models, by contrast, impose fewer constraints. Many more parameters are estimated, as each endogenous variable in a system is treated as a function of its own lags and the lags of the other variables. Just how many lags to use is an open question; somewhat arbitrarily, we have chosen to use lags of up to four quarters.

Although the four models make quite different assumptions about the process by which partisanship evolves, they offer similar assessments of the rate and extent of partisan change. Table 5 compares the average rate of quarterly change in Gallup macropartisanship across the four models. In the interests of providing the most generous assessments of change due to short-term forces, the first column is based upon the estimated *total effects* of economic change, as gauged by analyses in which consumer sentiment is the sole predictor; the basis for the second column is the estimated effects when approval is the sole predictor. The MacKuen et al. (1989) results suggest that fluctuations in consumer sentiment caused macropartisanship to change at a rate of .68 per quarter from 1953 to 1996. The corresponding figures from the four alternative models range from .16 to .25. Similarly, the estimates of MacKuen et al. imply that presidential approval has produced, on average, macropartisan change of 1.09 points per quarter, while the four revised models put this figure between .18 and .46. By this criterion, the estimates originally reported by MacKuen et al. seem to overstate the endogeneity of partisanship by a factor of two or three.

Another way to gauge the magnitude of short-term forces is to consider the sorts of political or economic reversals that would be necessary to produce a ten-point shift in macropartisanship. The MacKuen et al. (1989) estimates ($\beta = .10$, $\delta = .84$) suggest that in order to produce a realignment of this magnitude, the Index of Consumer Sentiment must plummet from its mean (86) to its lowest recorded level (55) and remain there for five consecutive quarters (during one party's watch). By contrast, our partial adjustment estimates ($\beta = .03$, $\delta = .95$) imply that the downturn in consumer sentiment must last four years, precisely the sort of sustained economic catastrophe to which traditional realignment theories point when explaining upheavals in the party system.

As noted earlier, the list of time-series models with which to analyze macropartisanship is potentially limitless, and we could well have included others. Our experience, however, is that all of the analyses that work within the basic framework laid out by MacKuen et al. (1989)[11] point toward a similar conclusion: Depending on the criterion used, partisanship tends to be at least two or three times more stable than originally suggested by MacKuen et al. Unless one analyzes a short time series (or cuts a long series into short segments by introducing a host of intercepts), the recurrent finding is that the partisan balance undulates gradually as economic optimism and presidential popularity wax and wane. In this respect, partisanship is quite different from other facets of public opinion, such

[11] In their reply, Erikson, MacKuen, and Stimson introduce a fundamentally different macropartisanship model. In it the equilibrium level of macropartisanship is determined by innovations (rather than levels) of consumer sentiment and presidential approval. These innovations are assumed to accumulate over time without any decay whatsoever. Time will tell whether the assumptions and predictions of this new model prove to be empirically sustainable.

as the perceived competence of the parties, which shift markedly in the wake of changing economic and political circumstances (Gerber and Green 1998).

CONCLUSION

Findings purporting to show that partisanship travels with short-term forces have had a demonstrable effect on the way students of electoral politics view party identification. Tracing successive editions of leading texts, such as the *Change and Continuity* series (Abramson, Aldrich, and Rohde 1982, 161; 1995, 247) or *Controversies in American Voting Behavior* (Neimi and Weisberg 1976, 310; 1993, 268), one finds increasing skepticism toward the traditional view of party identification as an unmoved mover. Like MacKuen, Erikson, and Stimson (1989, 1139), many students of public opinion now see in decades of survey data clear evidence that "partisanship's twisting course has been shaped by the winds of political and economic fortune."

Although we take a different view of macropartisan change, this metaphor nonetheless seems apt. Wind, unlike water, alters a landscape slowly, almost imperceptibly. A political party is doubtless more successful at winning adherents when its leaders are popular and its record spotless. Skeptical though we may be of the notion that short-term changes in the political environment alter the distribution of party identifications appreciably, it would be foolish to take the position that party attachments are altogether unwavering. Identification with a party wanes somewhat when, year after year, it presides over hard times or lacks effective leadership. The question is whether shifts in partisanship are sufficiently large and abrupt to call into question traditional views of realignment and the stabilizing role that party identification plays in a party system.

MacKuen, Erikson, and Stimson argue that the relationship between short-term fluctuations and partisanship is so strong that one must speak of realignment as an ongoing process, one in which the party balance at any given time reflects recent trends in political popularity and economic optimism. Our results, which show partisan adjustment occurring at less than half the rate initially suggested by MacKuen et al. (1989), call this characterization into question. The sluggish pace at which partisanship adapts to changing circumstances means that only dramatic and *sustained* shifts in the political fortunes of a party precipitate sizable changes in the macropartisan balance. Given the ebb and flow of partisan warfare and economic performance in the United States, it is understandable that substantial, let alone cataclysmic, partisan realignments should occur so infrequently.

APPENDIX

Except where noted, replication programs and data sets may be found at the website http://pantheon.yale.edu/~gogreen/macrepl.html.

Construction of the Macropartisanship Series

The quarterly Gallup series for 1953:1–1988:1 is based on a complete inventory of in-person surveys. These data were provided by MacKuen, Erikson, and Stimson. (Using data from each in-person poll conducted during this period, we were able to reproduce this series with a high degree of accuracy.) In 1985, Gallup had begun to phase out personal surveys, and in 1988 gaps begin to appear in the quarterly series of personal interviews. At this point, MacKuen et al. use a weighted average of personal and phone surveys, with a correction for the pro-Republican bias of Gallup telephone surveys. We use their series through 1988:1 when seeking to replicate their results.

For extensions of their analyses, we substitute telephone surveys (again, aggregated quarterly) for 1988:2–1996:1. To correct for the pro-Republican bias of Gallup telephone polls, we add 3.537 to macropartisanship scores for 1988:2–1996:4, which is the predicted difference between personal and phone assessments of macropartisanship for 1988:2, based on a regression of phone polls on personal surveys (using quarters in which both types of Gallup surveys were conducted). It should be stressed, however, that the pattern of results we report is not sensitive to the particular manner in which the macropartisanship series is updated and adjusted. We have also analyzed the Gallup personal interview data on a poll-by-poll basis, using a Kalman filter to fill in the daily gaps between polls and to account for sampling error in any given poll. The results are very nearly identical to those presented in Table 1, suggesting that temporal aggregation of the partisanship data is not a serious problem. (See http://pantheon.yale.edu/~gogreen/kalman.html).

NES data were drawn from the Cumulative Data File for 1952–94 and the Early Release for 1996. Data for each survey were weighted. For surveys with a panel design, only preelection surveys were used to avoid biases due to panel attrition. CBS/*NYT* data were obtained from MacKuen et al. for 1976–92 and updated using RPOLL and CBS/*NYT* reports thereafter.

Construction of the "Political Approval" Measure

Following MacKuen et al. (1989), we estimated the regression models presented in Table 2 in a two-step manner. First, approval is regressed on the same administration and events variables, with approval lagged one quarter and consumer sentiment lagged one quarter. The lagged effect of consumer sentiment (.29, see their Table N-1) is then subtracted from approval to create a purged approval series. Second, the macropartisanship model includes the lag-1 purged approval series, lag-1 consumer sentiment, lag-1 macropartisanship, and the rest of the event and administration variables (MacKuen et al. 1989, 1140, n 13).

Notes on Alternative Box-Jenkins Specifications

In order to gauge the robustness of our results, we experimented with alternative model specifications, many of which are reported in Table 3. First, we excluded approval from the model but obtained nearly identical estimated effects for consumer sentiment, confirming that the original MacKuen et al. model estimates consumer sentiment's total effect on macropartisanship. Next, we substituted presidential approval for "political" approval and found that consumer

TABLE A-1. Partial Adjustment Models Relating Consumer Sentiment and Presidential Approval to Macropartisanship

	Gallup Macropartisanship (1953:2–1996:4)			CBS/*NYT* Macropartisanship (1976:2–1996:4)		
Consumer Sentiment$_t$	.031		.008	.021		.012
	(.010)		(.012)	(.011)		(.013)
Presidential Approval$_t$		.043	.038		.030	.023
		(.010)	(.012)		(.015)	(.016)
Macropartisanship$_{t-1}$	.947	.915	.918	.975	.950	.962
	(.024)	(.025)	(.025)	(.022)	(.024)	(.024)
MA(1)	−.228	−.237	−.242	−.614	−.570	−.612
	(.081)	(.080)	(.080)	(.097)	(.101)	(.097)
Constant	3.230	5.178	4.992	1.327	2.773	2.074
	(1.493)	(1.528)	(1.535)	(1.304)	(1.420)	(1.401)
N	175	175	175	83	83	83
Standard error of estimate	1.78	1.73	1.73	2.34	2.32	2.33
Adjusted R^2	.86	.87	.86	.81	.81	.81
Q statistic, lags 1–12	8.91	8.68	8.53	9.56	7.73	8.03
p value for *Q*	.63	.65	.67	.57	.74	.71

Note: Standard errors are in parentheses. Presidential approval and consumer sentiment have been adjusted to reflect changes in party control of the presidency.

sentiment exerts no direct effect on macropartisanship, net of presidential evaluations. Conversely, we discarded consumer sentiment and focused on presidential approval (which no longer had to be purged, since consumer sentiment was absent from the model). We found statistically significant coefficients for the immediate effect of approval and its rate of decay, but both were notably smaller than what MacKuen et al. report. Finally, we approximated the OLS specification by lagging both consumer sentiment and political approval one period. No significant immediate or dynamic parameter estimates were obtained for either variable ($p > .05$).

Procedures for Mean-Centering Approval and Consumer Sentiment

Higher values of consumer sentiment and presidential approval should produce rising macropartisanship scores during Democratic administrations and decreasing scores during Republican administrations. The Index of Consumer Sentiment may be said to comprise two parallel series, each with opposite effects, one for Democratic administrations and one for Republican. In an earlier version of this essay we verified empirically that predictors such as consumer sentiment have equal and opposite effects during periods of Republican and Democratic party control. For efficiency, we generate a single mean-centered series by taking residuals from a regression in which consumer sentiment (multiplied by −1 for Republican quarters) is regressed on a dummy variable scored 1 for Democratic quarters. A similar procedure is used for presidential approval. This mean-centering approach produces results that are very similar to those obtained simply by including a dummy for Democratic administrations in equations in which macropartisanship is the dependent variable.

Partial Adjustment Model

Table A-1 presents the results of a series of partial adjustment models in which macropartisanship is regressed on consumer sentiment, approval, or both. The results are similar to the last columns in tables 3 and 4. Unlike the Box-Jenkins models in Table 3, the partisan adjustment model assumes that the effects of disturbances and short-term forces decay at the same rate. Unlike the models in Table 4, those presented in Table A-1 include an MA(1) term and contemporaneous regressors.

Fractional Integration

Although the long-term implications of fractional integration and ARMA(1,1) models differ, the two models are hard to distinguish with 176 observations. Indeed, the macropartisanship scores predicted by the ARMA(1,1) and ARFIMA(0,d,0) models correlate at .999. Not surprisingly, then, an ARFIMA model that includes consumer sentiment and presidential approval as predictors produces results similar to the partial adjustment analysis reported above. When consumer sentiment is the independent variable, the effect is .025 (SE = .009) with $\hat{d} = .772$, implying an average quarterly movement of .16 in the Gallup macropartisanship series. When approval replaces consumer sentiment, its effect is .025 (SE = .008), and $\hat{d} = .785$. These estimates translate into a rate of change of just .18 per quarter.

Whether fractional integration is an appropriate model for these data is unclear. A fractional integration parameter 7.5 implies that the macropartisanship series is mean-reverting but not covariance stationary. Time will tell whether the autocorrelation function for these data changes as new observations are added. It should be noted, however, that for 1953–96 the autocorrelations for the Gallup series become negative at the 29th lag. When fractionally integrated series with 176 observations are simulated with $d = .83$, only 25% display negative autocorrelations before lag 30, and the median lag is 40. For AR(1) processes with an autoregressive coefficient of .95, the comparable figures are 60% and 25.

Vector Autoregression

The estimates used to generate the statistics reported in Table 5 were drawn from a VAR in which macropartisanship was regressed on its first four lags and the first four lags of *either* approval or consumer sentiment. Impulse functions from models in which both approval and consumer sentiment

TABLE A-2. Error Correction Models of Macropartisan Change

	Change in Gallup Macropartisanship (1953:2–1996:4)		Change in CBS/*NYT* Macropartisanship (1976:2–1996:4)	
	Coefficient	Standard Error	Coefficient	Standard Error
Constant	3.354	1.512	1.465	1.113
Change in consumer sentiment	.019	.024	−.061	.033
Lagged consumer sentiment	.032	.010	.025	.010
Lagged macropartisanship	−.055	.025	−.027	.019
MA(1)	−.230	.081	−.673	.091
Standard error of estimate	1.78		2.26	
Constant	3.540	1.450	2.598	1.431
Change in presidential approval	.095	.020	.040	.035
Lagged presidential approval	.034	.009	.029	.015
Lagged macropartisanship	−.058	.024	−.047	.025
MA(1)	−.308	.080	−.587	.102
Standard error of estimate	1.70		2.33	
N	175		83	

are included as predictors (using the ordering consumer sentiment, approval, macropartisanship) produce similar results.

Estimation of Error Correction Model

Engle and Granger (1987) present a two-stage method of estimation, but this is most efficient when the correlation between the variables is close to one, which is manifestly not the case for partisanship and either approval or consumer confidence. We follow Beck (1991) and others in using a simpler one-equation method. We have also included the MA(1) term as in the partial adjustment model.

The results are shown in Table A-2 and coincide with what we have obtained using other techniques. The Gallup macropartisanship series is more responsive to short-term forces than the corresponding CBS/*NYT* series. The immediate effects of approval on macropartisanship, which come through strongly in the Gallup analysis (.095, SE = .020), prove much weaker (.040, SE = .035) when using CBS/*NYT* data, perhaps due to differences in question wording or the fact that presidential approval is measured using Gallup polls. Evidence of economic adjustment also is weaker in the case of CBS/*NYT*, as the immediate adjustment parameter is incorrectly signed. These results collectively suggest that macropartisanship changes with fluctuating political and economic conditions, but to a lesser extent than suggested by MacKuen et al. (1989, 1992).

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What Moves Macropartisanship? A Response to Green, Palmquist, and Schickler

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Contrary to the claim by Green, Palmquist, and Schickler (1998), macropartisanship is largely shaped by presidential approval and consumer sentiment. It is not the case, however, that macropartisanship mirrors the ever-changing levels of current *presidential popularity and prosperity. Rather, macropartisanship reflects the cumulation of political and economic news that shapes approval and consumer sentiment. Using ECM technology, we show that, far from being the weak force that Green et al. suggest, the cumulation of innovations in presidential approval and consumer sentiment largely account for the long-term trends in macropartisanship. For forecasting macropartisanship in the near future, it is better to predict from the fundamentals represented by the history of approval and consumer sentiment up to a given moment than from current values of macropartisanship itself.*

When "Macropartisanship" was written almost a decade ago (MacKuen, Erikson, and Stimson 1989), we saw our contribution as the following basic points. First, we showed that macrolevel party identification undergoes considerable movement over time. Second, we showed that much of this movement tracks changes in presidential approval and the economy as monitored by the Michigan Index of Consumer Sentiment. In retrospect, "Macropartisanship" is remarkable for what it did *not* say. Apart from chiding the then conventional view that national partisan sentiment seldom moves except when shaken by rare "realignments," the article offered few specifics about the nature of the macropartisanship time series. And it offered no specific viewpoint about microlevel party identification, other than dutifully citing competing "schools" ("psychological attachment" versus "running tally"). The motivating theoretical argument of that article, evidently original for its time, was that the relative stability of microlevel party identification did not logically require that macrolevel partisanship be a constant. These empirical results merely put partisanship back in the realm of politics.

Green, Palmquist, and Schickler (1998) charge that our earlier work seriously overestimated the effects of consumer sentiment and presidential approval on macropartisanship. More may be at stake than the size of a few coefficients. As they tell it, their revisionary interpretation helps to vindicate both the microlevel theory of party identification as a stable psychological attachment and the macrolevel theory of a partisan division largely immune from major political shocks. In their preferred model, macropartisanship is a stationary series oscillating in long cycles around its long-term mean, propelled largely by shocks of unknown origin. Curiously, they offer no discussion of the possible implications of this model, which suggests that party fortune is little affected by party performance.

Green, Palmquist, and Schickler are able researchers, but in this instance they manage to overlook effects that are hiding in plain sight. We welcome this opportunity to set the record straight. In our response, we emphasize two major points.

First, *macropartisanship is driven largely by the same political and economic forces that drive presidential approval and consumer sentiment.* When political and economic events cause approval and sentiment to change, macropartisanship also changes. The changes are small but permanent and cumulating, so that macropartisanship at any moment is largely the sum of the preceding economic and political shocks. Changes in macropartisanship from other sources that do not register in the approval and sentiment series (such as election campaigns and their aftermath) are mostly transient and of little long-term consequence.

Second, *the perceived inconsistency between macrolevel movement and microlevel stability is nothing more than a statistical illusion.* Our macrolevel evidence and interpretation are in harmony with the evidence and interpretation (e.g., Campbell et al. 1960; Green and Palmquist 1994) that microlevel party identification behaves as a stable psychological attachment.

Putting the parts together, we offer the following model for reconciliation of macrolevel and microlevel findings. (1) The time series of individual-level party identification represents an initial partisan disposition that is slowly modified by events, including the national political and economic forces that shape party fortune. (2) The macropartisanship time series is dominated by

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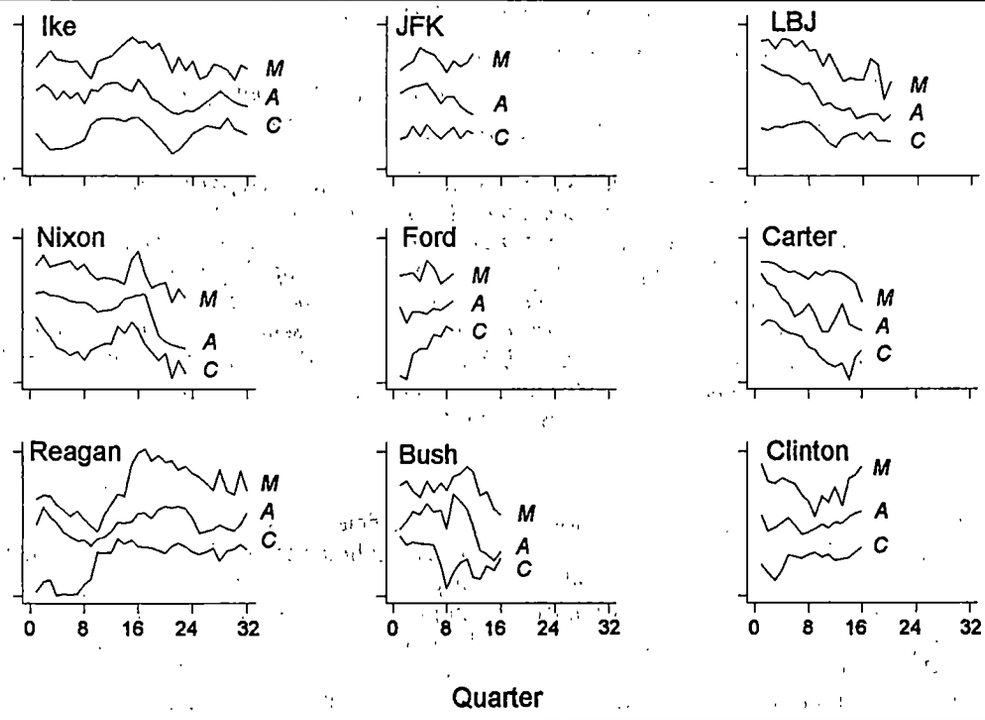
Some of the ideas expressed here had their origin in "Party Identification and Macropartisanship: Resolving the Paradox of Micro-Level Stability and Macro-Level Dynamics," presented at the 1996 meetings of the Political Methodology Society and the American Political Science Association. We thank Christopher Achen, Nathaniel Beck, John Freeman, Renée Smith, Christopher Wlezien, and anonymous *APSR* referees for their helpful comments along the way. We also thank Donald Green, Bradley Palmquist, and Eric Schickler for their generosity with their data and for pushing us to think harder about the dynamics of macropartisanship.

FIGURE 1. Consumer Sentiment, Presidential Approval, and Macropartisanship over Nine Administrations



M = Macropartisanship; *A* = Approval; *C* = Consumer Sentiment

the cumulation of these national political and economic forces.

THE STATISTICAL ARGUMENT OF GREEN AND COLLEAGUES

Figure 1 presents the time series of *Macropartisanship* (here, percentage of partisans for the presidential party), Gallup's *Presidential Approval*, and the *Michigan Index of Consumer Sentiment* for nine administrations. The graphed lines represent the raw data, unadulterated except for the standardization of scales to enhance the visual display.[1] As we view these data, party identification tracks approval, which tracks consumer sentiment. Not all approval trends stem from consumer sentiment. But virtually all detectable shifts in macropartisanship reflect the movement of approval and (perhaps indirectly) consumer sentiment.

Does this informal assessment stand up to statistical scrutiny? Green et al. say "no." While accepting our assessment of the effect of consumer sentiment on approval (MacKuen et al. 1989), they claim that we exaggerate these variables' influence on macropartisanship. They believe we "overfit" the data, particularly by our use of dummy variables for presidential administrations as controls. They prefer to report results without these controls, due to the presidential dummies' alleged lack of theoretical relevance. Their imposition of this statistical rule appreciably diminishes the coefficients for consumer sentiment and presidential approval and by itself accounts for much of the present disagreement.[2] Virtually all the ambiguity that they generate regarding our earlier claims is due to their omission of controls for presidential administration.

Table 1 presents two versions of a partial adjustment model predicting macropartisanship (percentage of Democrats among Democratic and Republican partisans) from current (Gallup) approval, one with and one without the presidential dummies.[3] (Consumer

[1] In Figure 1, the Michigan Index of Consumer Sentiment (MICS) and presidential approval are scaled in standard deviation units, while macropartisanship is scaled in 1.5 standard deviation units. This calibration allows approximately identical variances to the change scores in the three variables. Each series is depicted as centered around the within-administration mean, with the approval series centered two units higher than MICS and macropartisanship centered two units higher than approval.

[2] Not a matter of contention is use by Green et al. of *all* available Gallup surveys rather than the sample we used in our 1989 work. Their better measure of macropartisanship allows them to revise downward the coefficients for consumer sentiment and presidential (or "political") approval, while revising upward the coefficients for lagged macropartisanship. As they explain, the revised coefficients follow from the improved measurement of the control variable of lagged partisanship. We accept the revised numbers from their replications over our original coefficients when applied to our original models.

[3] Table 1 includes an MA(1) term to replicate the statistical practice of our replicators. The statistical analysis is virtually unchanged when the MA(1) term is omitted. The justification for the MA(1) by Green et al. is that it serves as an adjustment for sampling error, following Beck (1989). Yet, this adjustment is valid only when the underlying model is an AR(1) process, which we see as invalid for the macropartisanship data (discussed later). If the model *were* AR(1), then the

sentiment is omitted here because its effect is largely indirect.) As the table shows, the approval coefficient is virtually cut in half when the presidential dummies are eliminated.[4] How should one interpret this result?

The larger approval coefficient with the dummies represents the averaging over nine separate *within*-administration time series (Eisenhower through Clinton I), controlling away between-administration differences. For each administration, support for the presidential party is highest when approval is highest: about one point of macropartisanship gained for every ten points of approval. Green et al. would have us mingle different presidents of the same party, leaving between-administration differences uncontrolled and thus leaving open the door to unwanted noise and potential biases. Does this matter?

The presidential dummies are collectively significant at the .00006 level, a magnitude that may surprise readers of Green et al. (1998).[5] These dummies represent different equation intercepts for different presidents, reflecting the variation in party history that each president inherits. Statistically, these controls can only reduce bias rather than add bias, and at the trivial cost of a few degrees of freedom. The claim of theoretical irrelevance by Green et al. not withstanding, on strictly statistical grounds the higher estimate of an approval effect with the administration controls is decisively better.

Still, Green et al. have a point. Why should the presidential administration matter? Rather than deny the existence of this effect, as Green et al. prefer, let us try to understand it. The dummy coefficients in Table 1 do not represent presidential characteristics but the partisan nature of the times. The base categories are zero for Eisenhower (for Republican administrations) and Kennedy (for Democratic administrations). The coefficients are near zero for Johnson and Nixon but then turn sharply Democratic for Ford and Carter, as a post-Watergate legacy, and sharply Republican for Reagan, Bush, and Clinton, after partisan fortune turned again in the 1980s and 1990s. The dummy variable coefficients remind us that partisanship is a function not only of political and economic conditions of the moment but also of the political and economic history leading up to that moment.

The contrast between the Carter and Clinton I administrations provides a useful illustration. Within each administration, higher approval (along with consumer sentiment) is associated with higher Democratic partisanship, just as one might expect. The story changes when we look *between* administrations. On average, Clinton enjoyed better approval numbers (and

TABLE 1. Partial Adjustment Models Predicting Macropartisanship with and without Presidential Dummies

Variables	With Dummies	Without Dummies
Approval$_t$ × Party	0.092	0.044
	(0.013)	(0.010)
Party (D = 1, R = −1)	−5.156	−2.347
	(0.859)	(0.528)
Macropartisanship$_{t-1}$	0.637	0.904
	(0.061)	(0.028)
Intercept	22.358	5.844
	(3.770)	(1.720)
Presidential Dummies (Ike, JFK = 0)		
LBJ	0.734	
	(0.521)	
Nixon	−0.132	
	(0.394)	
Ford	1.119	
	(0.632)	
Carter	2.863	
	(0.692)	
Reagan	−1.531	
	(0.375)	
Bush	−2.150	
	(0.525)	
Clinton	−1.451	
	(0.652)	
MA(1)	−0.179	−0.225
	(0.099)	(0.081)
Significance of Presidential Dummies	0.000063	
Adjusted R^2	.889	.865
SEE	1.569	1.729
(*N*)	(175)	(175)

Note: Standard errors are in parentheses.

implied proportion of the variance in the dependent variable due to measurement error would be (using Green et al. symbols) $-(1 - \phi L)/\gamma_0$, or minus the ratio of the MA(1) coefficient to the autoregressive coefficient. This ratio is persistently .2 or higher in the Green et al. analysis, suggesting an implausible degree of sampling error in political surveys. Based on average quarterly *N*s exceeding 4,000 (Gallup) and 2,500 (CBS/*NYT*) and sampling theory, we estimate the reliability of macropartisanship to be .98 in the Gallup series and .96 in the CBS/*NYT* series. This comfortable level of accuracy does not call for debatable attempts at correction. To avoid making the analysis any more complicated than necessary, we do not include an MA(1) term in our original analysis which follows. Fortunately, all substantive findings are virtually unaffected by the presence or absence of an MA(1) term in the equation.

[4] The Table 1 equation without dummy variables is virtually identical to the first equation in Table A-1 of Green et al. In this and following data analyses, we use the Green et al. data set, except that we do not mingle approval ratings for different presidents for the same quarter. We use Kennedy's approval numbers for 1963:4; and Nixon's approval numbers for 1974:3.

[5] Green et al. appear to argue that administration dummies are not statistically significant. They report that our "slew" of 24 control variables, including the administration dummies, is not collectively significant for the period of our 1989 study. But that is without MICS or approval in the equation! Indeed, administration dummies achieve no more than borderline significance when the dummies and lagged macropartisanship are alone on the right-hand side of the equation. The reason the dummies gain dramatically in significance when when approval is entered into the equation will become clear as the discussion progresses. Presidential dummies are proxies for the equilibrium level of macropartisanship derived from cumulative political and economic approval over time.

better economic numbers) than Carter, yet Americans were more Democratic in the late 1970s than in the mid-1990s. This should surprise no one who knows the political history preceding Carter (the Watergate debacle) and Clinton (the Reagan-Bush successes). Any analysis that fails to incorporate that recent history would mistakenly conclude that good economic management and political approval produce partisan failure.

It is often said that categorical dummy variables of the sort represented by presidential administrations represent theoretical ignorance. If they matter statistically, then they control for something important, even if we do not know precisely what it is. Our next task is to identify that something. Calling it history is only the beginning. We refine our model to demonstrate the economic and political forces affecting macropartisanship, while rendering unnecessary a resort to presidential dummy variables, and we learn something important about the nature of partisan change.

A REFORMULATION

Consider again the three time series depicted in Figure 1. Statistically, consumer sentiment and presidential approval can be modeled as AR(1) *stationary* series, where current values are a function of values lagged one quarter:[6]

$$Approval_t = 3.792 + 0.917\, Approval_{t-1} + a_t, \quad (1)$$

and

$$MICS_t = 8.998 + 0.897\, MICS_{t-1} + e_t. \quad (2)$$

The terms e_t and a_t represent the innovations in the two series. Given the autoregressive coefficients of about .90, the innovations dissipate at a fairly rapid rate, about 10% per quarter, which limits the ability to forecast several quarters ahead. In effect, the electorate "forgets." It weighs recent inputs most heavily, discounting the past about .10 more each passing quarter. All this is reasonable. It makes sense for consumer sentiment to mirror the current economic circumstances more than those of the past. It makes sense for the president's approval rating to mirror current successes and failures more than those of the past.[7]

Macropartisanship, we argue, is different. Let us consider the implications of a partisanship that is not only the enduring trait emphasized by the traditionalists (e.g., Campbell et al. 1960; Miller and Shanks, 1996) but also changeable in response to events as emphasized by revisionists (e.g., Fiorina 1981; Franklin and Jackson 1983; Weisberg and Smith 1991). According to the original portrayal of partisanship as a stable attachment, people's fundamental feelings about the parties rarely change. If, in the extreme, virtually all partisanship is learned by early adulthood, then aggregate partisanship could change only glacially, as a function of the shifting partisanship of successive birth cohorts that enter and exit the system. Call this model I. Macropartisanship would be essentially a constant. Consider the other extreme: routine shifts in partisanship according to current evaluations of the president and the economy, along with perhaps other variables. Aggregate partisanship would reflect aggregate values of these predictor variables, with a memory no longer than the predictor variables themselves. Call this model II. Macropartisanship would move as a stationary series, reflecting the recent political and economic environment. We could aggregate the two models, making the near-constant birth-cohort component of model I the long-term equilibrium, while political and economic events generate a stationary series around it. Call this hybrid model III. While it anchors the equilibrium component of the stationary series by attributing it to aggregated early partisan socialization, its empirical prediction is indistinguishable from that of model II.

Our challenge is that none of these models fit empirically. Model I cannot be true by the simple test that macropartisanship moves too much. Models II and III do not fit the data well (at least without adding time-dependent dummy variables), which we know from Green et al. (1998). This verdict presents the following puzzle. How may partisanship (1) be largely shaped by early political learning and remain essentially stable during adulthood, (2) show real change in the aggregate, and (3) be effectively modeled as a function of its political and economic environment?[8]

Our solution is a revised blending of the two models of partisanship as permanence and change. To the extent the partisanship of mature adults is constrained by early socialization, in effect there must be no "forgetting" of partisan information learned during early adulthood, adolescence, and before. Of course, we also expect partisan learning to continue during adulthood. We suggest that the retention rate for partisan learning should not depend on the location in the partisan life cycle. *If early socialization leaves a permanent imprint, then any later changes in partisan disposition must also be permanent.* It makes no sense to argue that people discount their recent past but cannot shake off their distant past.[9] Thus, we expect the equilibrium value of partisanship (macro and micro) to

[6] For the estimated innovations in approval, only the lagged innovations for the current president are used. While this rule allows us to estimate innovations for the first quarter of the Johnson and Ford administrations, it means we must omit each presidential transition in the inaugural quarter following a presidential election.

[7] The approval and MICS series can be validated as stationary instead of unit-root by the conservative Dickey-Fuller test. The hypothesis that MICS is unit-root can be rejected at the .05 level ($|t| = 3.03$). Perhaps surprisingly, the verdict for approval is more ambivalent. The hypothesis that approval is unit-root can be rejected at the .10 level but just misses the .05 level ($|t| = 2.79$) using the quarterly series. When approval is measured monthly, however, the unit-root hypothesis is rejected at the more stringent .05 level. For earlier discussions, see Ostrom and Smith 1992 and Beck 1992. The approval and MICS series each show the autocorrelation pattern consistent with an AR(1) model.

[8] One resolution is that macropartisanship moves a lot, but for reasons that have little to do with the observable political and economic environment.

[9] By the same logic, if recent effects are transient, then those from the distant past are even more so. For most people, the effects of childhood socialization would be long forgotten.

represent the cumulation of its various causes, with no "forgetting."

This reasoning implies that while partisanship changes only slowly, its changes are permanent rather than transitory. When political and economic events affect presidential approval and/or consumer sentiment, the corresponding effect on macropartisanship will be small. But while these influences on approval or consumer sentiment wear off quickly, the small effects on macropartisanship stay, so that macropartisanship represents the accumulation of political and economic news.

Crucial to our understanding is that the stable, integrated series represents partisanship at equilibrium rather than its observed (macro or micro) realization. At the individual level, this formulation is consistent with Green and Palmquist's (1990, 1994) statistical portrayal of individual-level stable party attachments that rarely change. Most adults work out a social and psychological niche in which they encounter information, or interpret information, in ways that rarely threaten their understanding of the party system. While people can and do see reasons for changing their party loyalty, other factors usually "bring them home." These include psychological mechanisms, such as selective perception, internal counterarguing, and a need for psychological order; they also include such social elements as the issue considerations that frame political argument, the weight and direction of the frequently consulted mass media, and, of course, the political views of relevant others. For most people most of the time, these conditions and mechanisms will remain in balance. Thus, partisanship itself is not merely a verbal self-description but is the product of a self-regulating social and psychological system. Crucially, partisanship is a marker for the equilibrium conditions generated by a constellation of social and psychological forces. The equilibrating nature of partisanship (macro and micro) is important for politics because the resulting stability anchors the future to provide predictability.[10]

Equilibrium partisanship implies a distinctive dynamic: When party identification is at equilibrium, it will not change without outside surprises. Simply speaking, a Democrat with Democratic friends and liberal policy views who lives during good times presided over by a Democratic government will remain a Democrat unless something remarkable happens. Of course, unusual events may change the mix—the individual may move into new friendship circles, the national issue debate may induce conservatism, or the performance of the parties may fluctuate. To the extent to which these factors constitute the equilibrating mechanism, equilibrium partisanship will change in response to these shocks. Thus, we have $\Delta PID_{it}^* = u_{it}$, where PID_{it}^* is an individual's equilibrium partisanship, and u_{it} is the shock or innovation at any time. This implies that equilibrium partisanship is a cumulation of all previous innovations in the individual's lifetime (plus an initial partisanship)[11]:

$$PID_t^* = (u_{it} + u_{i,t-1} + u_{i,t-2} + \ldots) + PID_0^*.$$

In addition, we expect some people to stray temporarily from their equilibrium partisanship—perhaps attracted by particular personalities or moved by specific events. For instance, the Democratic partisan of our example may be seduced by a charismatic candidate to support the Republicans but will sooner than later "return home" to equilibrium partisanship. This produces:

$$\Delta PID_{it} = -\gamma(PID_{i,t-1} - PID_{i,t-1}^*) + v_{it},$$

where $(PID_{i,t} - PID_{i,t}^*)$ is the temporary deviation from equilibrium partisanship, and v_{it} represents short-term forces that deflect voters from their partisan equilibrium. These temporary deviations are based entirely in the present and are eventually "forgotten." Thus, equilibrium partisanship integrates history, whereas the temporary deviations disappear. These two parts imply that, to the extent to which equilibrium partisanship itself is unaffected by the personal environment and national forces, all movement will represent temporary deviation, and individual partisanship itself should be a stationary series.

At the macrolevel, the individual-level u_{it} and v_{it} shocks cancel out, except for their mean values, which we label α_t and ε_t, respectively. The macrolevel extension is straightforward. We have an observed macropartisanship, M_t, and its equilibrium value, M_t^*, where:

$$\Delta M_t^* = \alpha_t,$$

$$M_t^* = (\alpha_t + \alpha_{t-1} + \alpha_{t-2} + \ldots),$$

and

$$\Delta M_t = -\gamma(M_{t-1} - M_{t-1}^*) + \varepsilon_t.$$

The α_t inputs for macropartisanship represent the cumulated political and economic shocks to macropartisanship.[12] But we need not represent these shocks as mere white noise. We model the cumulated α_t as a function of the cumulated shocks to presidential approval and consumer sentiment. Our argument is that to know the cumulated political and economic shocks to approval and consumer sentiment is largely to know the equilibrium value of partisanship. Next, we put this argument to the test.

[10] This equilibrium partisanship is consistent with partisanship as self-identification as well as partisanship as a running tally, where the citizen accumulates evidence regarding the parties' relative advantages. These two equilibrating mechanisms may be less distinct in reality than in abstract theory.

[11] Note that $\Delta PID_{it}^* = u_{it}$ can be written $PID_{it}^* = PID_{i,t-1}^* + u_{it}$. We may take this last expression, substitute $PID_{i,t-1}^* = PID_{i,t-2}^* + u_{i,t-2}$ for $PID_{i,t-1}^*$, and repeat substituting recursively to generate a model of PID_{it}^* as an accumulation of innovations.

[12] A potential complication is that the γ term should be subscripted for individuals, as if different people have different autoregressive terms for the short-term component (see Box-Steffensmeier and Smith 1996, 1998). The possibility that the short-term component of partisanship may be fractionally integrated is beyond the scope of our attention here.

FROM THEORY TO ANALYSIS

In practice, macropartisanship decidedly does not behave as a stationary series. As Box-Steffensmeier and Smith (1996, 1998) observe, macropartisanship is long-memoried.[13] Box-Steffensmeier and Smith diagnose the series as "fractionally integrated," meaning as a composite representing a density of different AR(1) series. We find it satisfactory to model it more simply, as a cumulative series (integrated of order 1) plus a short-term stationary component. The cumulative series represents the aggregated partisan dispositions of citizens—in effect the collective equilibrium that itself changes in response to economic and political shocks. The short-term series represents the short-term variation around equilibrium.

We are now ready to reexamine the responsiveness of macropartisanship to economic and political forces. We want to model M_t^*, the equilibrium condition of macropartisanship (M_t). For M_t^* to be statistically integrated (no "forgetting"), current innovations must be uncorrelated with lagged values. This condition is readily accomplished by modeling macropartisanship not as a function of consumer sentiment and approval per se, but as a function of the *innovations* in these variables, e_t and a_t from equations 1 and 2. In this formulation, changes in consumer sentiment and presidential approval do not directly cause macropartisanship; rather, the economic and political events responsible for these changes also cause macropartisanship to change. The innovations in consumer sentiment and approval represent surprises, that is, changes in the economic or political climate unanticipated by the previous levels of consumer sentiment and approval.

Before we proceed, there are a few additional preliminaries. To gauge how the dynamics evolve, it is helpful to separate the flows of political and economic influences on M_t^*. Consumer sentiment reflects the workings of economic forces that operate largely but not entirely via approval. Complicating matters is the greater delay in the effects of economic news compared to political news. Consequently, we purge the innovations in approval (a_t) of the portion due to economic news (e_t). We do this by regressing a_t on e_t and e_{t-1}, taking residuals. These new residuals represent the political innovations (p_t) in approval, purged of the economic innovations. We also must adjust both sets of innovations to take into account the party controlling the presidency. We do so by multiplying each by a party variable scored +1 when the president is a Democrat and −1 when the president is a Republican, while scaling macropartisanship as the percentage of Democrats. In this way, good news adds to Democratic identification, given a Democratic president, but subtracts from it in the case of a Republican president.[14] Finally, we measure the cumulated versions of the p_t and e_t series. We start each cumulative series at 0 in 1953:1 and add scores.[15]

MACROPARTISANSHIP, POLITICS, AND THE ECONOMY

Armed with theory and data, we test the plausibility of our dynamic equilibrium formulation. In the Green et al. model, macropartisanship responds minimally and gradually to political and economic innovations, eventually trending toward a constant long-term equilibrium. In our proposed model, macropartisanship cumulates the effects of political and economic forces, thus undergoing permanent changes over time. We construct an error correction model (ECM) of change in macropartisanship as a function of the innovations in political and economic news plus the reequilibration of previous "errors" toward the equilibrium level of macropartisanship. We explicitly specify the equilibrium level to be a cumulative function of political and economic conditions. In abstract form, the model is:

$$\Delta M_t = b_E E_t + b_P P_t - \gamma(M_{t-1} - M_{t-1}^*) + u_t, \qquad (3)$$

and the equilibrium is:

$$M_{t-1}^* = c_0 + c_E CumE_{t-1} + c_P CumP_{t-1}, \qquad (3a)$$

where E_t and P_t are current economic and political innovations (capitalized to signify their status as independent variables), and $CumE_{t-1}$ and $CumP_{t-1}$ are the lagged *cumulative* economic and political innovations. (For an expanded discussion of the error correction model and its application to the problem at hand, see the Appendix.)

This model incorporates both of the dynamic formulations we wish to test. If the equilibrium M_t^* were a constant or driven by forces other than political and economic news, then we should estimate zero coefficients for c_E and c_P. Such a condition would reduce equation 3 to a model in which macropartisanship is a function of disturbances in the political economy that dissipate over time. Thus, the key test lies in the coefficients c_E and c_P. If they are zero, then we can adequately model the political part of macropartisanship as a gradual transitory process. If they are not zero, then the effects of the political economy persist in the form of equilibrium macropartisanship. (See also the discussion in the Appendix.) Using the Gallup macropartisanship series, the estimates are:[16]

$$\Delta M^G{}_t = 0.055E_t + 0.111P_t - 0.198(M^G{}_{t-1} - M^G{}_{t-1}^*),$$
$$\text{St. Err.} = (0.027) \quad (0.029) \quad (0.044) \qquad (4)$$

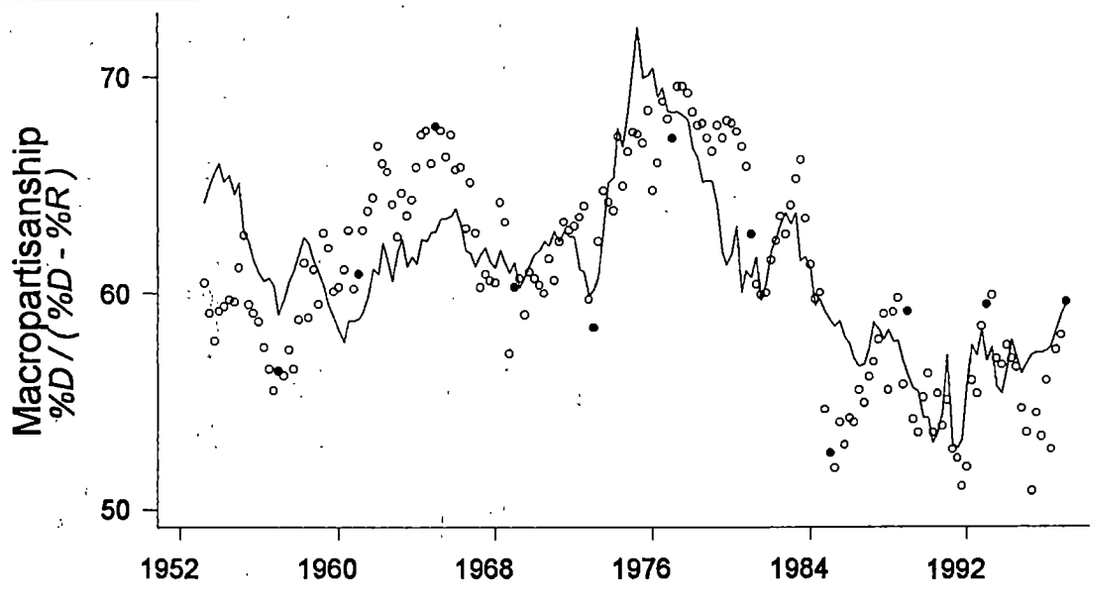
[13] One sign is that the over-time correlations are far stronger than they should be, given the autocorrelations at short lags. For instance, considering that the Gallup autocorrelation is .92 over one quarter and assuming an AR(1) model, the autocorrelation should be $.92^{12}$ after 12 quarters, or a mere .37. The observed autocorrelation after 12 quarters is .58, indicating a long-memoried process. By contrast, after 12 quarters the autocorrelations for MICS and approval (same president) drop to .18 and .08.

[14] An alternative to cumulating simple residuals would be to cumulate "recursive" residuals. See the Appendix for a discussion.

[15] For inaugural quarters we have no measure of political innovations because we have no lagged approval scores for the new president. For these quarters we substitute the value of zero for generating cumulative political innovations. Given the lags involved in the creation of the independent variables, the estimation equation is based on a set of observations that do not start until 1953:3.

[16] These estimates are from the "one-step" procedure.

FIGURE 2. Gallup Macropartisanship, 1952–96



Dots = M; Line = M^*. Solid Dots = Presidential Elections

and

$$M^G{}_t{}^* = 64.17 + 0.169CumE_t + 0.126CumP_t,$$
$$\text{St. Err.} = (1.41)\ (0.036)\ \ (0.029)$$
$$\text{Adj. } R^2 = 0.168;\ SEE = 1.712;\ N = 175.$$

We see that c_E and c_P are decisively nonzero (a joint test produces $p = 0.00000$). The equilibrium $M_t{}^*$ is a dynamic function of the political and economic environment. Furthermore, if we add administration dummy variables, they neither are statistically significant nor affect the other coefficients in an appreciable way. In short, by explicitly modeling the enduring component of macropartisanship in terms of its underlying cumulative economic and political causes, we account for the historical "effects" otherwise represented by administration dummies and resolve the empirical dispute raised by Green et al. More important, we affirm that the crucial part of macropartisanship—its equilibrium condition—is driven by political and economic information.

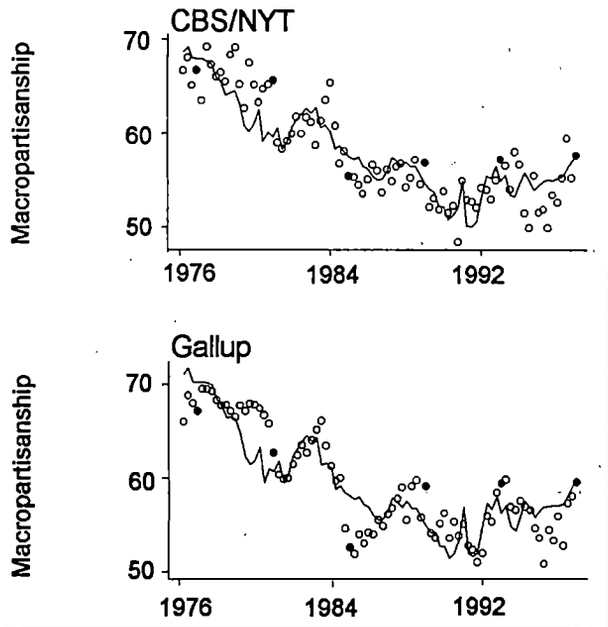
More is revealed in equation 4 than the likely existence of a moving equilibrium. We observe from the $(M_{t-1} - M_{t-1}{}^*)$ coefficient that in the absence of $M_t{}^*$ change, macropartisanship moves toward its equilibrium value to fill about 20% of the gap per quarter. We also observe a distinctive dynamic response of macropartisanship to politics compared to economics. Political shocks (P_t) produce an immediate reaction in macropartisanship of 0.111 points per unit shift. Thus, when the equilibrium shift is in the form of a changing political input, we obtain nearly the full movement toward the new equilibrium (0.111 of 0.126 points). For economics, in contrast, a one-point innovation in E_t yields only an 0.055 immediate response, which is only one-third of the way toward the new equilibrium (0.169 points higher than before). In subsequent periods, both deviations will reequilibrate at a rate of 0.198, but the major movement in the political part will have already occurred. Thus, we see that the political part of equilibrium partisanship reacts almost immediately to exogenous shocks, while the economic part reacts only partially, that is, gradually. This makes sense in that people understand quickly the partisan meaning of political news, but they take longer to appreciate the partisan implications of economic change.[17]

Figure 2 graphs observed (Gallup) M_t along with $M_t{}^*$ equilibrium values. With M_t and $M_t{}^*$ correlated at 0.77, more than half the variance in M_t is accounted for by $M_t{}^*$, the moving long-term equilibrium. It is important to note that the largest part of macropartisanship's range, from its post-Watergate peak to its Gulf War trough, is captured by the fundamentals.[18] Meanwhile, the $(M_t - M_t{}^*)$ "residuals" reveal a pattern of systematic short-term variation. Most of these short-term surges center around landslide national elections, such as the short-term Democratic

[17] Consistent with macropartisanship's gradual response to economic change, macropartisanship moves only when economic realizations are felt in the pocketbook. This is unlike presidential approval, which responds as economic change is anticipated in the form of expectations (MacKuen et al. 1992). For details, see the Appendix.

[18] We also can observe the $M_t{}^*$ economic and political components separately over time. The economic component (with a slightly larger variance) has generally favored the Democrats. The political component has more consistently favored the Republicans. The two largest reversals of this Republican trend occurred during Watergate and late in the Bush administration.

FIGURE 3. Two Macropartisanship Series, 1976–96



Dots = M; Line = M^*. Solid Dots = Presidential Elections

surge in 1964 and a short-term Republican surge in 1984.[19]

We can repeat the ECM exercise using the CBS/*NYT* measure available for 1976–96:

$$\Delta M^C_t = 0.019E_t + 0.096P_t - 0.536(M^C_{t-1} - M^C_{t-1^*}),$$
$$\text{St. Err.} = (0.053) \quad (0.048) \quad (0.098) \tag{5}$$

and

$$M^{C*}_t = 64.52 + 0.174CumE_t + 0.156CumP_t,$$
$$\text{St. Err.} = (1.83) \quad (0.031) \quad (0.026)$$
$$\text{Adj. } R^2 = 0.254;\ SEE = 2.281;\ N = 83.$$

For comparison, the Gallup version of the equation for the same time period is:

$$\Delta M^G_t = 0.029E_t + 0.127P_t - 0.294(M^G_{t-1} - M^G_{t-1}{}^*),$$
$$\text{St. Err.} = (0.039) \quad (0.035) \quad (0.070) \tag{6}$$

and

$$M^G_t{}^* = 64.29 + 0.221CumE_t + 0.143CumP_t,$$
$$\text{St. Err.} = (2.50) \quad (0.044) \quad (0.035)$$
$$\text{Adj. } R^2 = 0.265;\ SEE = 1.168;\ N = 83.$$

Figure 3 compares the tracking of the Gallup and CBS/*NYT* series as a function of their $M_t{}^*$ predictions. Even though the latter series looks noisier (due to its inherently greater measurement error), on average it is just as adequately predicted by $M_t{}^*$. The apparent differences in the two series are largely a matter of statistical illusion.[20] The two measures of $M_t{}^*$ correlate at .99 with each other and at .87 with the two observed M_t series, while the two M_t measures correlate at .91 with each other.

TABLE 2. Predicting Future Macropartisanship from Current *Observed* Values (M_t) plus Current *Equilibrium* Values (M^*_t), 1976–96

Dependent Variable	Gallup		CBS/*NYT*	
	M_t	M^*_t	M_t	M^*_t
M_{t+1}	0.70	0.35	0.49	0.50
(N = 83)	(9.43)	(3.57)	(5.08)	(4.49)
M_{t+2}	0.55	0.50	0.31	0.67
(N = 82)	(6.20)	(4.22)	(3.05)	(5.75)
M_{t+3}	0.38	0.65	0.23	0.74
(N = 81)	(3.49)	(4.47)	(2.15)	(6.12)
M_{t+4}	0.24	0.78	0.26	0.67
(N = 80)	(1.95)	(4.84)	(2.41)	(5.41)
M_{t+5}	0.04	0.96	0.19	0.71
(N = 79)	(0.30)	(5.62)	(1.61)	(5.12)
M_{t+6}	−0.12	1.01	0.09	0.77
(N = 78)	(0.60)	(5.34)	(0.69)	(5.45)

Note: Entries are regression coefficients, with t-values in parentheses.

There can be little doubt that the forces driving long-term change in macropartisanship are largely the same forces driving consumer sentiment and approval. The proof is in the predictive accuracy. Table 2 compares the ability to predict future values of macropartisanship from current realizations (M_t) and the current equilibrium ($M_t{}^*$) of macropartisanship, for 1976–96. The coefficients represent the OLS coefficients for M_t and $M_t{}^*$ predicting future macropartisanship at various leads. To predict future Gallup realizations as immediate as three quarters ahead, the current equilibrium value provides more information than current realizations. For predicting five or more quarters ahead, current realizations are not even statistically significant when the current equilibrium value is controlled. For predicting six quarters ahead, current macropartisanship has the wrong sign. For the CBS/*NYT* series, which Green et al. claim is so unresponsive to economic and political inputs, the predictive dominance of $M_t{}^*$ is even stronger. For predicting one quarter ahead, M^C_t and $M^C_t{}^*$ perform about equally. For longer leads (even a mere two quarters), $M^C_t{}^*$ predicts better than M^C_t. *It is more important to know the fundamentals represented by cumulative innovations in consumer sentiment and approval than to know the current value of macropartisanship.* The equilibrium value represents the fundamentals; any current deviation from this value will be temporary. Moreover, the

[19] This transient component is exactly that envisioned by Converse (1976). Much (but not all) of the evident partisan fluctuation before elections that seems to presage the election outcome is merely temporary.

[20] Since the CBS/*NYT* series reequilibrates faster than the Gallup series, a case can be made that the Gallup series (with its "in politics today" wording) is more affected by short-term forces unrelated to $M_t{}^*$. The crucial point is that the two series respond identically to the long-term component of macropartisanship represented by $M_t{}^*$.

fundamentals provide stability, correlating at .97 with its lagged values.[21]

The key to understanding the macropartisanship series is its long memory. The response of macropartisanship to new economic and political inputs may be imperceptibly small at the time of occurrence, but it will be long-lasting. For instance, the gain in Democratic macropartisanship during Watergate was far less noticeable than the decline in Nixon's popularity. Yet, as Nixon left the scene, the Democratic gain from Watergate persisted to provide an otherwise inexplicable level of Democratic identification during the Ford and Carter presidencies. Similarly, the more visible long-term Republican gain of the 1980s provided a new base of Republican strength that limited the Democratic recovery starting in the late Bush administration. (See the Appendix for further discussion of M^* as an integrated series.)

In time-series terminology, the long-term or fundamental component of macropartisanship is cointegrated with the cumulative economic and political innovations that influence consumer sentiment and presidential approval. At the same time, a short-term component of macropartisanship (most visible in the Gallup version) represents the transitory response to momentary events. This is very different from the Green et al. model of macropartisanship as a stationary series propelled largely by its own lagged values, with only small transitory effects for consumer sentiment and presidential approval.[22]

THE MICRO-MACRO CONNECTION

To say that the fundamental part of macropartisanship derives from the permanent cumulation of economic and political shocks implies that partisan changes for *individuals who comprise the series* are permanent.[23] Here, we address the plausibility of our model, given what we know about individual-level partisanship. Are the equilibrium values of individuals' party identifications integrated? Is there enough microlevel change in party identification to account for the macrolevel movement?

Actually, plentiful side-evidence supports an integrated individual-level partisanship. For instance, it is established lore that individuals become more partisan with time (Converse 1976). The growth of the variance of individual partisanship with age implies that individual partisanship is an integrated rather than stationary series. We can also cite the distinctively Republican partisanship of the pre-Depression generation, famous for its long memory, even as this older generation approaches extinction.[24]

Much of what we know about the stability of party identification at the individual level is due to the sophisticated statistical analysis by Green and Palmquist (1990, 1994) themselves. Using Wiley-Wiley methodology (Wiley and Wiley 1970), they estimate that most of the observed change in the survey responses to the party identification question is actually error, while "true" party identification is remarkably stable. Corrected for "error," Green and Palmquist's microlevel equation is:

$$PID_{it}^* = \beta PID_{i,t-1}^* + u_{it}, \qquad (7)$$

where PID_{it}^* is the "true" value of party identification, and u_t is its normally distributed disturbance. The variables are mean-centered to omit the intercept. Using the two NES four-year panel studies, Green and Palmquist's (1994) mean estimate of β is 0.972 for eight quarters, which prorates to 0.997 for one quarter.[25]

For all practical purposes, Green and Palmquist's (1994) "true" partisanship is unit-root. In our terminology, their "true" partisanship is *equilibrium* partisanship at the microlevel, while their measurement error is a composite of actual survey error plus short-term movement around the respondent's equilibrium partisanship. Just as the long-term component of M_t is integrated, so is the long-term component of individual party identification.

Now we restore the intercept to equation 7:

$$PID^*_{it} = \beta PID_{i,t-1}^* + \alpha_t + u_{it}, \qquad (8)$$

where, as we now see, $\beta = 1$. The intercept α_t represents a uniform shock shared by all citizens at time t due to a common reaction to shared events, while u_{it} is the individual variation around α_t due to variation in individual experience and evaluation. At the macrolevel, the u_{it} terms cancel out, so that only national forces matter. The result is that $\alpha_t = \Delta\overline{PID}^*_t = k\Delta M_t^*$, so that microlevel change and macrolevel

[21] When the Gallup series is extended to include observations as early as 1957 (the start of Eisenhower's second term), we get nearly as strong a verdict in favor of the forecasting power of $M^G_t{}^*$. This result falters, however, when the first Eisenhower term is included. As Figure 2 shows, the M^G_t versus $M^G_t{}^*$ gap is unusually pronounced for 1953–56. The explanation is uncertain and could lie in weaker data (with more missing values) during this period or perhaps omitted variables.

[22] Green et al. object to the possibility that macropartisanship could be an integrated variable. For our response, see the Appendix.

[23] Given individual mortality, effects are permanent only for the life of the person experiencing them. How can effects be "permanent" if voters are mortal? The time series can be regarded as an amalgam of parallel time series for different birth cohorts that enter and exit. The "slopes" of these series are parallel random walks with cohort-specific intercepts. Our M_t^* captures the slopes but not the mean intercepts that reflect the electorate's shifting cohort composition. Compared to the response of macropartisanship to political and economic events, cohort replacement makes only a small contribution. We estimate that the loss of the pre-Depression generation may have cost the Republicans a few percentage points from 1952 to 1980, with early departures from the post-Depression generation causing a mild reversal of the arrow in more recent years. Including estimates of the contribution of cohort replacement adds to our prediction of M_t^* without altering our ECM model in any appreciable way.

[24] Voters from the pre-Depression generation respond to contemporary stimuli in modern polls, but they remain less Democratic than the post-Depression era generation.

[25] To prorate estimates of β for the quarterly time frame, we take β reported over c quarters to the $(1/c)$ power. In addition to the two NES panels with waves two years apart, Green and Palmquist (1994) also present estimates for four additional adult panels involving shorter time intervals. If we estimate β from pooling six separate estimates from six panels, we obtain a sample mean of 1.00, with a range (from panel to panel) of 0.99 to 1.01.

change are now equivalent, except for a constant k to correct for different scales.

This directs us to the crucial puzzle. How can we reconcile the "large" macrolevel movement in macropartisanship (ΔM_t^*) with the relatively "small" amount of microlevel change represented by the variance in u_{it}? This discrepancy exemplifies the typical illusion of a large change at the macrolevel appearing small at the microlevel. We demonstrate by rescaling ΔM_t^* in party identification units and comparing its variance with the variance in quarterly individual-level movement in equilibrium partisanship, u_{it}. For a plausible result, there should be less variance in the common movement represented by $\alpha_t = k\Delta M_t^*$ than in the individual deviation around this movement represented by u_{it}, since most movement of microlevel partisanship probably depends on idiosyncratic reasons rather than national forces.

Green and Palmquist (1994) provide four estimates of the variance in u_{it} from the four-year NES panels, which we prorate for quarterly time intervals to obtain the estimate of 0.032.[26] To compare this variance with that for ΔM_t^* we need the conversion rate k to transform ΔM_t^* into microlevel seven-point-scale units. We can approximate k using the 23 NES surveys, 1952–96, comparing the standard deviation of aggregate party identification on the 1–7 scale with the standard deviation of macropartisanship [$(\%D/\%(D + R)$] from the same NES surveys. (The two measures correlate at 0.95, reassuring that they measure the same thing.) The ratio of the standard deviations is 24.1:1.[27] Using this conversion table to translate ΔM_t^* variance into party identification units, we obtain relative variances of u_{it} and α_t as 0.032 to 0.0017, or a ratio of 19:1.[28] Thus, by our estimates, there is far less partisan change due to national conditions than to idiosyncratic factors. The amount of macrolevel movement in partisanship is quite compatible with Green and Palmquist's understanding of microlevel partisanship's stability.

What of Green and Palmquists' (1994) measurement error in observed individual partisanship? Measured at two-year intervals, all short-term variation in individual partisanship around equilibrium values would be statistically indistinguishable from classical measurement error, with its instantaneous decay rate. Aggregated, this short-term change is the observed variation in $M_t - M_t^*$. We compare the observed variances of short-term macropartisanship ($M_t - M^*_t$) with Green and Palmquist's (1994) error ($PID_{it} - PID^*_{it}$). Using our conversion rule, and comparing the variance of $M_t - M_t^*$ (8.86) with the mean Green-Palmquist (1994) estimate of the error variance (0.599), we obtain a 1:40 ratio of variances. Thus, if only about .025 of Green and Palmquist's "error variance" were induced by national short-term forces, we can readily account for the observed short-term deviations of M_t around M_t^*.

CONCLUSION

Macropartisanship responds to the political and economic forces represented by presidential approval and consumer sentiment. To see this, one must know where to look. An obvious starting point is to investigate the short-term connection between presidential approval and consumer sentiment, on the one hand, and macropartisanship, on the other. But as Green, Palmquist, and Schickler (1998) demonstrate, such a search is only partially fruitful. As we have shown here, macropartisanship incorporates not only the political and economic news of the present but also the cumulation of news from the past. By adopting an "equilibrium" model of *micro*partisanship, we see that the major turns of partisan fortune found in the macropartisanship series can be explained by changes in the economic and political environment. This result, we believe, should be reassuring rather than controversial: Politics does matter. And that confirmation does not require upending current understanding about the stability of microlevel party identification.

APPENDIX

The Error Correction Model and Macropartisanship

The error correction model (ECM) is a standard formulation in which current change has two components: one due to current inputs, and one due to a reequilibration or a return to a sustainable value. In our application, the first part is straightforward: People react to political and economic information. The idea of the second part is that if M_{t-1} were out of equilibrium—for example, if people overreact to good news—then people would move back toward the partisanship that is consistent with their psychological and social environment, hence "error correction."

The classic formulation of this model is by Davidson et al. (1978), and its use is now commonplace (e.g., Bannerjee et al. 1993; Hendry 1995). Beck (1991) provides an instructive discussion of ECMs in political science. The following equations, repeating equations 3 and 3a from the text, illustrate how the dynamics work.

$$\Delta M_t = b_E E_t + b_P P_t - \gamma(M_{t-1} - M_{t-1}^*) + u_t \quad \textbf{(A-1)}$$

and the equilibrium is:

$$M_{t-1}^* = c_0 + c_E CumE_{t-1} + c_P CumP_{t-1} \quad \textbf{(A-1a)}$$

If $b_P = c_P$, for example, the immediate adjustment in M_t due to P_t would exactly equal the new long-run equilibrium M_t^* due to P_t, so that the path of M_t and M_t^* would correspond.

[26] Given $\beta = 1$, the reported disturbance variance of u_{it} can be converted to quarterly intervals by dividing the reported variance of u_{it} by the number of quarters between waves.

[27] That is, a change of 24 percentage points in macropartisanship is required for the party identification mean to move one full unit on the seven-point scale.

[28] This calculation assumes the Gallup version, M^G_t.* Another interpretation is as follows. Suppose we could follow typical citizens over 44 years, measuring their true (equilibrium) partisanship over this span and at the same time represent the long-term component of macropartisanship in 7-point-scale units over the same span. The individual citizen's equilibrium partisanship would display 20 (19 + 1) times the variance of M_t^*. In 1–7 scale units, movement in macropartisanship has moved only in the range between "pure-Independent" and "Independent, leaning Democrat" (or less than one point) since 1953.

If the immediate effects were substantially higher than the long-run implications, $b_P > c_P$, public overreaction to political news, the public would then readjust to the more modest change in equilibrium partisanship. Finally, if the immediate influence is only a fraction of the long-run implication, $b_P < c_P$, then we have evidence for a partial adjustment to the new dynamic equilibrium. The short-run dynamics are a function both of the ratio b_P/c_P (which is the proportion of the equilibrium effect felt immediately) and of the "return to equilibrium" parameter γ, which shows the proportion of the remaining disequilibrium ($M_t - M_t^*$) is adjusted away each quarter.

If c_E and c_P are zero in equations 3a and A-1a, then the equilibrium M_t^* becomes the constant c_0, and equations 3 and A-1 reduce to $\Delta M_t = b_E E_t + b_P P_t - d(M_{t-1} - c_0) + u_t$. Were this the case, the change in M_t would be a function of a shock in the political economy and a return to a constant equilibrium, making the political or economic component a transient. If c_E and c_P are nonzero, then the effects of political or economic shocks carry forward in the form of an updating M_t^*.

The estimates for equations 3 and 3a (A-1 and A-1a) are obtained by algebraically substituting the latter into the former and using a nonlinear least squares algorithm. Alternatively, one could use a two-step procedure (Engle and Granger 1987): estimate equation 3a and then substitute the estimated M_{t-1}^* into equation 3, all in OLS. Although there is a lively literature on whether the two-step or one-step procedure is preferrable, the matter is moot here. The two-step estimate correlates with our one-step estimate at .998. (We have also estimated the ECMs with MA and AR terms modeling the disturbances; the reequilibration rate estimates diminish slightly, but the rest of the model remains the same.)

Macropartisanship as an Integrated Series

By construction, our cumulative series ($CumE_t \doteq \Sigma E_s$; $CumP_t = \Sigma P_s$; $s = 0, 1 \ldots t$) represent the strict addition of estimated political and economic shocks. Because the shocks are statistically independent over time, the cumulated series are statistically integrated. $CumE_t$ and $CumP_t$ (and therefore $M^G{}_t^*$ and $M^c{}_t^*$) all easily pass the statistical challenge of the Dickey-Fuller unit root tests; with nonsignificant $|t|$ values of 1.65, 1.57, 1.80, and 2.31, respectively, the null hypothesis of a random walk cannot be rejected for any of these variables. Dickey-Fuller tests on the residuals ($M^G{}_t - M^G{}_t^*$) and ($M^C{}_t - M^C{}_t^*$) show $|t|$ values of 4.44 and 4.36, respectively, which are significant beyond the .01 level. Thus, standard diagnostic tests reassure that M^* is unit-root with stationary residuals.

The Alternative of Stationarity. Of course, to construct integrated series from political and economic shocks is not by itself sufficient evidence of the correct specification for the explanation of macropartisanship. An alternative construction would be stationary series where effects decay geometrically ($CumE_t = \Sigma E_s\lambda^{t-s+1}$; $CumP_t = \Sigma P_s\lambda^{t-s+1}$) according to a λ parameter between 0 and 1. In effect, we chose $\lambda = 1.00$. Other possibilities, even in the $\lambda = .99$ range, produce lesser correlations between predicted and actual M_t. For this test, we search for the value of λ that maximizes the R^2 predicting M_t from the λ-specific $CumE$ and $CumP$ measures. We start the cumulations in 1953 and use them to predict M_t for 1976–96. Actually, the λ that maximizes the R^2 is 1.002 for Gallup and 1.006 for CBS/*NYT*.

Thus, we model macropartisanship's equilibrium as a random walk and observe that empirically this model appears to defeat all alternatives. Still, our analysis does not depend on the knife-edge assumption that the autoregressive parameter is precisely 1.0000. If in truth M^* is merely "near integrated" (see Banerjee et al. 1993; DeBoef and Granato 1997) then the implications would be no different for practical politics.

The Plausibility of Integration. Green et al. reject as fundamentally implausible the possibility that a bounded series such as macropartisanship could be integrated or unit-root. Their objection is that a random walk implies an explosive variance and a run to the boundary conditions (0% or 100%). In theory, this objection carries weight; in practice the concern is misplaced.

First, macropartisanship is an unbounded variable that is indexed as a bounded percentage only due to measurement limitations. The concept itself (the electorate's mean relative attraction for the Republican or Democratic Party is unbounded. For comparison, consider the plausible argument that the stock market is unit-root. If we were forced to measure its performance as the percentage of stocks above some arbitrary value-per-unit threshold, then the truth of the statistical properties of the underlying variable would remain unchanged. In any case, we could make measured macropartisanship unbounded by the simple expedient of recalibrating it by the logit transformation. Such a maneuver is unnecessary for the observed data that range from about 50% to 70% Democratic. M_t and its logit transformation [$\log(M_t/(1 - M_t))$] correlate at 0.9998 for the actual M_t observations.

Second, M_t^* avoids the calamity of a run to the 0% and 100% boundaries because of its slow speed of movement. In theory, the net change in a unit-root variable will have zero mean with a variance equal to $m\mathrm{Var}(u_t)$, where m is the number of periods and u_t is the change over interval t. The variance in ΔM_t^* is a shade under 1.0, so the expected variance over m quarters is about equal to the number of quarters. For instance, following a century of change, the expected variance is about 400, and the expected standard deviation is about 20. With a standard deviation of 20, half the cases are within 16 points of the mean. Thus, the median absolute value of ΔM_t^* over several runs of 100 years is a mere 16 percentage points.

Because the bounded variable works well enough and because we want to keep the measure easily interpretable ("percentage Democratic" rather than "log of the odds that a partisan is a Democrat"), we chose to follow convention and use percentages, even though we expect an integrated behavioral process.

Recursive Residuals

One interpretation of the innovations in approval and economic sentiment is that these represent the degree of positive/negative surprise conditional upon the lagged levels of the variables. To estimate these innovations, we merely regress the current value on the previous value and take the residuals, with the intent that these represent actual surprises. A plausible alternative is to generate the "recursive" residuals. In that case, the "surprise" is estimated for every time point using only the historical data available up to that time, as if the public could perform these regressions using history when they form their judgments.

The obvious attraction of recursive residuals is that people know history but not the future; for instance, people living in 1956 could not have known the course of events for the next 40 years and thus could not have used those data to estimate persistence functions. It is awkward to assume, however, that everyone's historical monitoring began precisely at the time

when our statistical monitoring began, in 1953. In defense of our use of regular residuals we assume that citizens early in our time span used pre-1953 information to estimate the persistence functions and that these estimates are similar to those using later information as well.

If the persistence function changes over time, then there may be an advantage to using the recursive residuals. If the persistence function is relatively stable, however, then this advantage disappears. Furthermore, the recursive residuals approach produces poor estimates at the beginning of the series because the data are too slim for the task.

We compared estimates of our model using both methods. For the recursive residuals model we follow the exact same procedures as we did for the "simple residuals" estimates of the innovations in economic sentiment and political approval, this time substituting the recursive residuals for the simple residuals. In the end, the two procedures produce statistically indistinguishable estimates.

Decomposing Economic Shocks

Our main analysis shows equilibrium macropartisanship as a function of cumulated economic and political shocks, where the economic shocks are drawn from the Michigan Index of Consumer Sentiment (MICS). Although the central question about economics has been framed as whether consumer sentiment matters, we can probe further to ask which ingredients of the index actually stir mass partisanship. If the question is which ingredient affects presidential approval, the answer is business expectations (MacKuen et al. 1992). The effect on macropartisanship is different. Consistent with its long-term character, macropartisanship's economic response is to collective pocketbook realizations.

To see this, from MICS we extract the separate components of pocketbook realizations (whether family fortunes improved over the past year) and long-term business expectations (the five-year forecast of business conditions). Then, we repeat our ECM exercise with *two* economic components plus the political component (approval unexplained by economics). With two economic components (R_t = shocks to personal retrospections; F_t = shocks to business expectations; and P_t redefined as presidential approval not accounted for by the pair of economic series), we obtain:

$$\Delta M^G_t = 0.031R_t + 0.018F_t + 0.115P_t - 0.271(M_{t-1} - M_{t-1}{}^*)$$
$$\text{St. Err.} = (0.024) \quad (0.019) \quad (0.028) \quad (0.053) \qquad \textbf{(A-2)}$$

and

$$M^G_t{}^* = 61.89 + 0.119CumR_t - 0.017CumF_t + 0.113CumP_t$$
$$\text{St. Err.} = (1.50) \quad (0.024) \quad (0.025) \quad (0.020) \qquad \textbf{(A-2a)}$$
$$\text{Adj. } R^2 = 0.222;\ SEE = 1.693;\ N = 174.$$

The result is decisive. M^* is driven by aggregate pocketbook considerations and the political component of approval but not by business expectations. Partisan self-identification, quite unlike approval of a president, is a matter of personal experience rather than informed speculation. Thus, while economic expectations can affect a president's popularity, any long-term effect on macropartisanship depends on whether the expectations are followed by their predicted realizations. For the presidential party's economic policies to alter the electorate's collective partisan memory, anticipation is not enough. The effects must be felt by the voters.

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The Puzzling Case of Christianity and Republicanism: A Comment on Black

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Antony Black argues that Christian republicanism was one of the discourses at work in framing the history of Western republican thought. But he neglects to confront the theoretically unique character of the Christian approach to republican institutions. First, Christian republicanism derived from more general beliefs about the divinely ordained organic structure of the universe. Second, it evinced no necessary hostility toward monarchic rule; indeed, quite to the contrary, its cosmological premise of organic hierarchy supported the office of the king (whether papal or secular). Once these elements of Christian republicanism are supplied, the medieval contribution to the history of republican ideas takes on a complexion very different from that described by Black.

Antony Black (1997) has drawn attention to a major lacuna in the present literature on the history of republican thought: its failure to take account of the Christian contribution to the development of Western republicanism. By viewing republicanism as a purely secular phenomenon, Pocock (1975), Skinner (1978), and others (for example, Rahe 1992) assume the equation of the Christian tradition of politics with monarchical absolutism, systematically ignoring the fact that republican practices as well as theories were central to the development of Christianity. Black thus adds a crucial dimension to the study of the development of republicanism.

Black has not, however, achieved this considerable accomplishment without introducing some new and significant confusions into the discussion. Specifically, I wish to suggest two difficulties in Black's presentation of the Christian tradition of republicanism. First, he continues to rely upon a primarily secular reading of medieval political thought, one which filters out precisely those elements of Christian cosmology and theology that distinguish the republicanism of the Latin Middle Ages from either its classical or modern instantiations. There is more at stake than meets the eye in Black's apparently wry remark that he "will not attempt to define *Christianity*" (1997, 647), since reference to the distinctly *Christian* character of "Christian republicanism" is precisely what his account lacks. Second, by positing an ideal typology of republicanism, he imposes a single vision of a republican tradition that was (and is) diverse and multiplicitous. This leads, in particular, to an anachronistic opposition between republic and monarchy to which many "republicans" of medieval Europe would not subscribe. As Janet Coleman has observed, Latin Christian thought was characterized by a discernible "lack of a neat and oppositional distinction between *regnum* and *respublica*" (1997, 217; see 215–7).

In support of these claims, I sketch the outlines of a form of Christian republicanism typical of the Latin Middle Ages: one imbued with the belief-system of Christianity and by no means hostile to the principle of monarchy. To defend this view of Christian republicanism fully would, of course, require a magisterial examination of the full range of medieval political writings. In the present context, I can only illustrate my understanding of Christian republicanism anecdotally, by brief examination of the ideas of two major contributors to late medieval political theory: Jean Gerson and Nicholas of Cusa. My choice of these two authors is by no means arbitrary, however. Both figured centrally in the conciliar movement within ecclesiology that Black (1997, 651) considers to be "the strongest evidence for convergence between republicanism and Christianity." A survey of these thinkers sustains the validity of Black's general thesis that the history of republicanism cannot be told without reference to Christianity, while also demonstrating that this claim requires the narration of a more complicated and less Whiggish process than he permits.

CHRISTIAN REPUBLICANISM

The key to medieval republicanism, I maintain, is a theologically informed conception of social and political institutions as organic unities: head and members are interdependent and reciprocally interrelated segments of a living body, characterized by hierarchy but also by inclusiveness and mutual respect. The *constitutio* of the medieval political organism was, metaphorically, a matter of physiological health. Of course, organic conceptions of political community enjoyed a history prior to Christian times and were also attractive to political thinkers working within non-Christian religions, such as Islam. The medieval European reliance upon organic discourse and imagery is distinctively characterized by its embeddedness in a broader Christian cosmology, a conception of the universe as an ordered and living whole composed of microcosm and macrocosm—not so much a "chain of being" as an animated but differentiated totality. According to a line of Christian thought that runs back to the church Fathers, God created the universe as a unity, composed of many different parts, each of which is itself an organic unit that both partakes of the greater whole and has a distinct purpose within the larger scheme.

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The author thanks Dr. Simone Zurbuchen and Professor Donnalee Dox for helpful comments on this essay.

In a 595 letter, Pope Gregory the Great stated:

> The provision of divine dispensation established that the ranks and diverse orders are distinct, so that when the lesser ones show reverence to the greater ones and the greater ones use love towards lesser ones, one combination of love is made out of diversity and the administration of each office is properly carried out. For the whole could not exist in any way if the great order of this difference did not serve it. But since creatures cannot be governed or live in one and the same equality, . . . it is clear that they are not equal but differ one from the other, as you know, in power and in order. If this distinction is known to exist among those who are without sin, what man will resist submitting willingly to this same dispensation (cited in Constable 1995, 271)?

Within each organic unit, as within the universe as a whole, there is higher and lower, superior and inferior. Yet, this hierarchical ordering does not exclude the lesser members of the body from a proper and worthwhile role within the whole.

The principal idea here is reciprocity, that is, mutual support and respect among the various parts of the whole necessary for the common good. This implies, in turn, the need for the active inclusion of all members for the sake of the welfare of the whole. Each segment makes its own unique contribution; the organism cannot survive without the coordination and cooperation of every function. The twelfth-century author John of Salisbury captured this implication of Christian cosmology in a letter to his friend, Peter of Celle:

> Everything consists in mutual aid, and heavy things are reciprocally (*vicissim*) intermingled with light things, and indeed in this way things always proceed, because the same spirit of unanimity within things nurtures the concordance of things dissident and the dissidence of things concordant, and disposes the parts of the cosmos (*mundani*) as though the members of a body, so that they may be in accord reciprocally for the ministering of each to the other. Thus it is in the human body that the members faithfully serve each other reciprocally and individuals' duties are assigned for the public advantage. Some have more or less of these than others on account of the mass of the body, but all are united by the goal of its health; they have various functions, but if you consider the advantage of health, they contribute to the same totality (*universa*). Yet they are not all coequal, but the inferiors submit to the superiors. For the foot which is planted in the mud by no means aspires to the dignity of the head; but also the head, which is raised up towards the heavens, does not scorn the foot which moves around in the mud (John of Salisbury [1159] 1955, 181–2).

When transferred by analogy to the political realm, these themes coalesce into a coherent republican position: The ruler is restricted in what he can do by the reciprocity built into the body politic; the participation of the other members of the organism in the business of the common welfare both sets limits on the actions of the head—he cannot properly interfere with their unique tasks—and establishes a sort of "check" on the head in cases where it oversteps its appropriate scope of activity (see Nederman 1992, 980–1, 982–3).

Note that organic political thought was not a single, fixed idea.[1] Rather, it is perhaps more accurate to characterize the image of the body politic as a site of intellectual and political struggle among many contending interpretations and interests. Some of its features were taken as given by all parties to the discourse. For instance, it was taken for granted that some ruling part (generally, but not always, the head) was necessary for the body's health; a headless body was doomed to misery and destruction. Likewise, it was incontestable that law was the medium through which the members of the organism cooperated and to which all parts must submit; law formed the muscles and sinews, or perhaps the backbone, of the animate polity. And no one seriously challenged the existence of some good or welfare common to all parts of the body and greater than each of them taken individually.

Yet, even while creating a framework for discussion, the organic model raised a large number of questions that stimulated wide-ranging debates among medieval political theorists. Could the head be corrected or removed if it engaged in conduct detrimental to the health of the body? By whom? In what way(s)? What duties and powers did members retain under ordinary circumstances? How did they coordinate their specialized roles and tasks with one another? These questions were by no means closed or settled in the medieval mind. The body politic provided a matrix within which central issues about the nature of rulership, governance, and community could be addressed in a manner consonant with deeply held cosmological and theological beliefs.

Medieval ecclesiology, no less than secular theory, commonly embraced this organic model of communal life and its organization. The church styled itself the *corpus Christi mysticum*, "the mystical body of Christ"; but although that phrase had conventionally denoted the sacramental powers with which the church was endowed, a change in usage is noticeable around the middle of the twelfth century. As Francis Oakley (following the work of Henry de Lubac) points out, the sacral functions associated with the church came to be denoted by the words *corpus Christi verum*, while "the expression *corpus Christi mysticum* . . . [began] to acquire political and juristic connotations so that by the fourteenth century 'mystical body' has become almost a synonym for 'moral and political body' " (Oakley 1969, 380). One notes significant overlap between the discourses employed by authors discussing ecclesiological and secular affairs: The language associated with the body politic raised essentially the same questions in the context of the governance and organization of the church as it posed in the case of the earthly community (see Kantorowicz 1957, 193–232).

GERSON'S ECCLESIOLOGY

The Parisian theologian Jean Gerson, whose importance Black (1988, 575, 580; 1992, 171–2) has noted,

[1] The best general treatment of medieval organic political thought is Struve 1978.

affords a clear case of the distinctive form of republicanism I have identified. Gerson is best known as an advocate of conciliarism, that is, the doctrine of the supremacy of the General Council in the governance of the church. Gerson's conciliar credentials are well established. He was personally active in promoting a conciliar solution to the Great Schism in the early fifteenth century, and his writings were widely disseminated throughout the period of the councils of Pisa and Constance (see Morrall 1960; Pascoe 1973). Yet, Gerson's conciliarism conforms less to the typology of antimonarchic institutional and civic republicanism offered by Black than to the organic Christian republicanism I describe. As a consequence, Gerson simultaneously subscribes to a conciliar ecclesiology *and* to a strong defense of papal monarchy.

To understand how Gerson was able to endorse what may seem to us antithetical principles, it is first necessary to grasp his very conception of the nature of government. All legitimate exercise of power, he contends, must be limited according to law. He states flatly that "spiritual [jurisdiction] should be exercised according to canonical laws toward the ultimate end of realizing beatitude," just as "temporal [jurisdiction] should be exercised according to the civil laws" (Gerson [1417] 1965a, 216). This pertains directly to the governance of the church. Spiritual jurisdiction in Gerson's view is placed principally in the hands of the pope, who serves as a judge whose determinations must occur on the basis of law. Specifically, papal jurisdiction is constituted by "the juridical power of excommunication or interdict by denial of ecclesiastical sacraments and the communion of faith to those who rebel against and disobey the church" (p. 218). The pope, in other words, is charged with primary authority for the maintenance of faith and morals among the body of Christian believers to be accomplished by the enforcement of church law.

Gerson's requirement that the papacy must govern in accordance with law does not, however, entail a denial of papal plenitude of power, that is, the independence of the authority of the pope's office from any superior except God. Gerson remarks simply that "the papal plenitude of power was granted by Christ in those things which are supernatural... [and] stands supreme" (Gerson [1415b] 1963, 479). This principle is accepted throughout Gerson's writings. In turn, the theoretical basis for the pope's indispensable *plenitudo potestatis* may be located in Gerson's analysis of the nature of ecclesiastical power in general. Gerson ([1417] 1965a, 226) argues that all offices within the church have, formally speaking, been ordained by God. In this sense, the powers pertaining to the pope cannot be curtailed or controlled by any human authority without violating the divine will. Rather,

> ecclesiastical power in its plenitude is formally and subjectively in the Roman pontiff alone.... The plenitude of ecclesiastical power is the power of order and jurisdiction which was supernaturally given by Christ to Peter, as His direct and first King, for himself and for his successors to the end of time, for the edification of the church militant toward the achievement of eternal felicity (Gerson [1417] 1965a, 277–8).

It is thereby erroneous to attribute to Gerson the position that "the pope's apparent plenitude of power is in effect conceded to him as a matter of administrative convenience" (Skinner 1978, II, 116). On the contrary, Gerson believes that the papal *plenitudo potestatis* is of divine origin and is absolutely essential for the conduct of the church's main business, namely, the salvation of souls. In no way ought the pope to be construed as ancillary or derivative to the full spiritual life of the church.

Thus far, Gerson may sound to us like an extreme papalist, but he also recognized that the doctrine of *plenitudo potestatis* has perpetuated an exaggerated vision of the pope's office. Consequently, he allows that ecclesiastical power may also be understood to convey two other meanings: a "material" or "respective" sense, related to its assignment "to particular persons by legitimate right"; and a sense connected to the "exercise or execution" of power (Gerson [1417] 1965a, 220). Materially speaking, the pope (or any ecclesiastical officer) must be chosen and consecrated, activities which appropriately inhere in the whole church. Thus, the church as the mystical body of Christian believers retains ultimate authority over the specific occupants of the papal office (p. 223). Analogously, the power of regulating the use to which particular persons put the rights attached to ecclesiastical government is to be found in the corporate church (p. 232). Papal jurisdiction exists not for its own sake, but instead in order to "edify" the church, to serve the common good of salvation (Gerson [1415a] 1962, 300). And should a given pope not aim at this end, the authority pertaining to the papal office is abused rather than rightly used.

In sum, Gerson ([1417] 1965a, 232) believes that "the regulation of the use" of papal power is confined to those circumstances in which "it is liable to be turned into abuse." The church does not control the office; it instead supervises the conduct of the individual incumbent of the papacy. The regulatory role with which the church is endowed arises because of the uncertainties surrounding human moral will: "Since the supreme pontiff... is open to sin and may wish to turn his power to the destruction of the church," it is necessary for "abuse of this sort to be repressed, limited and moderated" by the whole church (p. 233).

Yet, the pope whose behavior is appropriate to the office is beyond any scrutiny; he is protected by his *plenitudo potestatis*. The pope can do whatever is demanded for the edification and welfare of the church without any limit or supervision. When the incumbent acts according to the precepts of faith and charity, the papacy must be totally free to exercise and impose its jurisdiction: "We are able to do what we are able to do rightfully" (Gerson [1417] 1965a, 221). Abuses of power are defined only in terms of actions arising from an evil will that places its private interests above the needs of the public. The "limitation" of papal authority can only be applied to the use of the powers by individual occupants, and never to the direct control of

the office itself, which is a matter for divine determination. Gerson's attitude is summarized by his declaration: *"Papa fluit, papatus stabilis est"* ("The pope changes, but the papacy is stable") (Gerson [1409a] 1965b, 132).

Such considerations also inform the relationship between the pope and the General Council. Gerson ([1415b] 1963, 309) argues that just as a pope is only elevated to office when he has been formally elected and consecrated, so he can only be ousted from the papal throne by a public proceeding and final judgment within the context of a General Council. But Gerson also makes it clear that the reason popes are deposed by councils stems from human frailty and imperfection. Insofar as an individual pope may fall into sin, he is susceptible to the judgment of church and council, "to which he is subject in the same manner as one who is capable of erring to what can never err" (Gerson [1409b] 1965c, 140). In other words, because the papal will is exercised by fallible humans, it is *"accidentalis atque mutabilis"* ("contingent and mutable") (p. 137), whereas the will of the General Council is without moral defect and therefore may always be expected to reach the correct decision in matters of papal turpitude. The determinations of the council are effectively the actions of a morally superior and incorruptible body.

The "perfection" of the General Council reflects its status as the sole and incontrovertible voice of the corporate mystical body of the church. When the council speaks, its proclamations do not reflect the private concerns of its membership but the true needs and purposes of the total ecclesiastical corporation. The General Council does not have a will of its own; rather, it expresses the genuine and singular will of the church community. At this juncture we encounter the corporatism that was an ingrained feature of medieval canonistic and conciliar writing. (For a definition and analysis of this corporatism, see Tierney 1955, 106–53; 1982, 19–28.) As a mystical body, the church has only one will and only one set of interests; its common utility is to be found in the defense and dissemination of the orthodox faith and correct morals, a universal and self-consistent doctrine. The church, in sum, is the bearer of "indeviable wisdom" (Gerson [1417] 1965a, 233) with regard to its own goals and ends. Within the ecclesiastical setting, the communal church is the final arbiter of all matters touching upon the beliefs and conduct of the faithful.

In a sense, then, the pope does rule with the consent of the church manifested through its unique representative, the General Council (Gerson [1415b] 1963, 479). Yet, the council hardly functions "politically" at all in the sense a secular republican might understand, since it is under the direct guidance of the Holy Spirit. The church itself has only one genuine interest to be served: the perpetuation of a unified orthodoxy, about which there could be no legitimate difference of opinion. Therefore, the General Council concerns itself solely with matters arising from the identification and enforcement of catholic doctrine, that is, with the truer and higher purpose necessarily shared by all Christians (Gerson [1415b] 1963, 478–9). In this, the faithful at large were effectively treated as mere subjects rather than full citizens; their actual participation as individual members of the church was not essential or important. The mystical body of the church is not to be confused with an aggregate of its particular members.

NICHOLAS OF CUSA AND THE CHURCH

This vision of Christian republicanism was not unique to Gerson, but rather seems to have been shared generally among conciliarists of the fifteenth century. In the *De Concordantia Catholica* of Nicholas of Cusa,[2] too, we find doctrines that echo republicanism admixed with Christian theology and acceptance of monarchic rule. Nicholas constructs the edifice of the church upon expressly organic foundations. Citing Jerome and other Fathers to the effect that the church is a concordant body of disparate members, headed by Christ (Nicholas of Cusa 1991, 5–6), Nicholas ties this to a general cosmological observation: "It is most evident that everything existing and living has been created in concord.... But every concordance is made up of differences. And the less opposition among the differences, the greater the concordance and the longer the life" (p. 6, translation altered). In turn, the organization of the natural and supernatural worlds offers the proper model for the church:

> When the concordance of differences in the whole universe is examined, the wise recognize that there is a wonderful combination in nature, the entire world shares in a mutual spherical interaction, and everything is ordered to a single end. It suffices for the present to know for our investigation that just as all created beings... are preserved in union and concordance with their natural originating source, so our church, which is our topic of discussion, is made up of rational spirits and human beings who are united with Christ, although not in the same way, but in gradations (Nicholas of Cusa 1991, 9–10, translation altered).

The church partakes, as a mystical body, of the same ordering principles as the rest of the universe, according to Nicholas. Therefore, to understand these principles is to grasp the appropriate arrangement and interrelation of the parts of the church.

Specifically, Nicholas concludes from his organic model that no single member of the church may represent fully the powers inhering in the body as a totality. Hence, neither the pope nor any other individual or group within the church may claim supremacy over the whole. At best, Christ alone, as the true Head of the church, may command with finality; and Christ's presence occurs "virtually" when a body representative of the whole church—namely, the General Council—meets and is imbued with the inspiration of the Holy Spirit (Nicholas of Cusa 191, 118–9, 193–4). Yet, this does not lead Nicholas to deny the primacy of the See of St. Peter or the plenitude of papal power in matters

[2] While in the following I usually rely upon Sigmund's translation of this work (Nicholas of Cusa 1991), I have occasionally retranslated passages on the basis of the Kallen edition of the Latin text (Nicholas of Cusa 1959–68).

of ecclesiastical leadership. Indeed, he attacks Marsiglio of Padua by name for upholding just such a position (pp. 197, 204). Nicholas acknowledges the presence of superior and inferior, ruling and ruled, in all organic structures. Like other conciliarists, he regards the papacy to be of divine origin and thus to be a necessary and permanent feature of the mystical body. Indeed, Nicholas insists on organic grounds that the pope must possess a measure of "coercive" power for the sake of maintaining the church:

> Unless an ecclesiastical ruler has some coercive power—although not that lordship by which [secular] rulers hold sway—unity cannot be preserved. For it is necessary that the rotten member and foot should be cut off and the eye that scandalizes torn out if the health of the body of the church is to be preserved (Nicholas of Cusa 1991, 201, translation altered).

Ordinarily, responsibility for securing such health pertains to the pope; acting to achieve the unity of the faith is one of the central duties assigned to the Roman bishop by divine ordination.

Consequently, no council of the church has the authority to eliminate the papal office or to control the pope when he is engaged in the proper execution of his functions. Indeed, no council would wish to do so, given the papacy's divine ordination and established role in the process of salvation. The real problem arises, instead, when a ruler disorders the body by performing duties other than those attached to his office. As Nicholas (1991, 164, translation altered) expresses the matter, "all the members of the one church and the mystical body of Christ have their specific duties, the exercise of which cannot be impeded without the disturbance of order." The General Council, as the agent of universal ecclesiastical reform, is charged with overseeing these functions and with scrutinizing parts that either abdicate or exceed their proper tasks: "If it were the case that every power governed in accordance with the regulations established by the universal councils, the best possible reform would necessarily follow" (p. 164, translation altered).

This is especially true for the papacy, which exercises superior powers within the order of the church. Nicholas fears that the infection of the leading member of the political organism will readily spread to the rest of the body:

> If we know what is proper for each member according to the rules of the ancients, then we may see how the body of the church becomes deformed. For it would not be deformed had not an excess of abuse occurred. And since when the head is ill, the other members suffer, it follows that the salvation of the subjects depends upon the health of the rulers.... No greater deformity can, therefore, arise than when one believes that, because of one's great power, anything may be permitted and that person violates the rights of subjects (Nicholas of Cusa 1991, 164, translation altered).

By framing the issue with reference to the organic community, and by conceptualizing the General Council as the only truly "representative" body within the church, Nicholas proceeds to demonstrate that it falls to the church uniquely to reform the limbs and organs, and especially the papal part, whenever the good order of the whole is threatened. The council is the sole valid embodiment of the welfare of the church, and it alone may determine what and where reform must occur. Nicholas posits the council as infallibly capable of discovering and mandating the true and objective interests of the church as a united entity. Nicholas's republicanism thus depends upon prior acceptance of distinctively Christian principles of theology, and it does not conflict with adherence to the preferability (indeed, necessity) of monarchy. Indeed, this helps explain why Nicholas could uphold conciliarism and yet within a few years defect to the position of extreme papalism.[3]

CHRISTIAN REPUBLICANISM: PAST AND FUTURE

Black is surely warranted in his conclusion that elements of medieval Christian republicanism did not simply disappear with the Reformation, the scientific revolution, and the other harbingers of modernity. In particular, he seems correct to locate resemblances between "the ecclesiological concepts of inerrancy and unanimity," on the one hand, and "the concept of the general will," on the other (Black 1997, 654). But insofar as the premises of Rousseau's community were manifestly "non-Christian," as Black admits, such connections are perhaps at best superficial. The organic communalism of Christian republicanism required acceptance of theological assumptions that Rousseau certainly rejected, not least conceptions of natural hierarchy and the necessity of monarchic rule. Nor should it be forgotten that Rousseau reviled the system of parliamentary representation that he associated with the "degraded" and "dishonorable" practices of medieval life (Rousseau 1987, 198–9)—an explicit rejection of the line of republicanism that Black traces out. Rousseau's passion for equality, not to mention his secularist orientation, places him at a distance from republicanism as it was conceived and practiced by the conciliarists and other thinkers grounded in medieval Christian doctrine.

Black (1997, 654) merits praise, then, for his reminder that Christianity was one of the discourses at work in framing the history of Western republican thought. Christianity is not inherently more sympathetic to a strictly monarchist than to a republican position. But Black's account seems marred by a desire to construct direct lineages between medieval Christian approaches to republican institutions and modern doctrines of republicanism. This leads to the neglect of some features that are most distinctive and constitutive of Christian republicanism: that it derived from more

[3] On the wider implications of Nicholas's apparent change of heart, see Fasolt 1991, 318–9. Fasolt stresses that the very distinction between "papalism" and "conciliarism," like the supposed opposition between clusters of other "isms" (e.g., "constitutionalism" and "absolutism," "republicanism" and "monarchism"), results from an anachronistic application of these categories to the Middle Ages.

general beliefs about the divinely ordained organic structure of the universe; and that this cosmological premise of organic hierarchy was by no means antithetical to the office of the king (whether papal or secular). Once these elements are supplied, the medieval contribution to the history of republican ideas takes on a complexion very different from that described by Black.

Specifically, it becomes possible to propose an alternative "ideal type" of republicanism more consonant with the main (although by no means exclusive) intellectual currents of the Christian Middle Ages, founded on a set of principles relatively detached from modern debate (à la Rousseau). Such republicanism, exemplified by the writings of Jean Gerson and Nicholas of Cusa, incorporates (1) moral-theological rather than formal-institutional limitations on the *person* of the ruler who exercises political power and (2) accountability of both government and citizens to a corporate or spiritual authority rather than to an actual community composed of free and equal individuals. Both of these medieval doctrines were gradually eroded and replaced in modern Europe by principles owing more to secularized patterns of thought. And, as a consequence, quintessentially medieval Christian republicanism gave way to its modern counterpart: a republican vision that indeed accords more directly with the typology constructed by Black.

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Christianity and Republicanism: A Response to Nederman

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In reply to Nederman I argue that in the Middle Ages monarchy and republicanism were not mutually exclusive, and that hierarchy and organicism were not specifically Christian ideas, and should not, therefore, be seen as the Christian element in republican thought. One should not assume that Christianity or any other historical ideology has an essential core of unchanging characteristics.

In my article on Christianity and republicanism (Black 1997), I argued that there were a significant number of de facto historical conjunctions between Christianity and republicanism throughout history, not only in the Middle Ages. I concluded that Christianity was not, as has been widely implied or assumed, inherently monarchical. Cary Nederman's argument is confined to the Middle Ages. He argues that there was "Christian republicanism" then, but that I failed correctly to identify its character, namely, its basis in the organic simile and its continued adherence to the principle of monarchy.

The evidence Nederman cites is drawn from conciliarist thought and in particular Gerson and Cusa. One cannot generalize on the basis of one late school of thought and two very unusual thinkers. To make his case, he would have to look at a much wider range of thinkers and milieux, including the civic and communal milieux. Such further evidence might have falsified or supported him, but without it his thesis is unsustainable.

Nederman's article does, however, raise important questions. He argues that medieval Christian thought was preponderantly monarchical. I would respond that in the period from about 300 to 1500 republican ideas in Europe did indeed develop within and as part of a monarchical and hierarchical culture. For example, Florence was devoutly propapal; Bartolus accepted the general overlordship of the emperor (Canning 1987, 71–90).

But we have to identify correctly what people meant by *monarchia*. To medieval thinkers it did not mean absolute monarchy but rule by one person: It was necessary to have one person in charge of the government. That conception did not rule out a great deal of what we today call democracy; it could mean the equivalent of presidential or prime-ministerial government (France, Britain). Thus, Thomas Aquinas wrote that the best constitution is one in which one person is put in charge on account of his virtue, others rule under him, and nevertheless "the government belongs to all, because these can be chosen (*eliguntur*, elected) from all" (D'Entrèves 1954, 148–9).

The monarch could be elected to serve for life, for a fixed period, or along with dynastic successors. Though the hereditary principle was widely practiced, it was seldom defended (Black 1992, 147–8). Kings had originally, it was said, been chosen by the people (John of Paris 1971, chapter 10, 19). The two most extensive monarchs, pope and emperor, were elected for life. It was generally agreed that monarchy was the best form of government because it mirrored God's regime in the universe and because experience showed that it promotes order. But nearly all theorists held that the best form of monarchy was not absolute but mixed, that is, with an aristocratic element (counsel and consent from feudal peers) and an element of *politeia* or, in today's language, democracy (the Commons, towns, third estate) (Blythe 1992). Everyone insisted that a monarch must abide by the laws and rule in accordance with them, except when extraordinary circumstances justified extralegal "equity" (*epieikeia*). Some argued s/he should be punished for not doing so. Monarchs were quite liable to be threatened by resistance and to be deposed, a practice defended by many political theorists (Burns 1998, 407–8, 493–511). Monarchical government was nothing like as stable as the popular image, from which Nederman does not dissent, suggests.

This means that some regimes described by medieval and Renaissance people as monarchical might be described as republican today; examples are the Serenissima Venice, with its lifelong doge, and the Florentine Republic, under which Machiavelli served and which he subsequently upheld as a good example, with Soderini as lifelong gonfalonier. The historical difference between constitutional monarchy and a republic is often semantic. (Later, even the American republic opted for one president rather than two consuls.) This point actually supports Nederman's argument, because the main witnesses he calls as Christian contributors to republicanism are the two most eminent theorists of constitutional monarchy, Gerson and Cusa.

Another important issue is raised by Nederman. While not disagreeing that there was a Christian contribution to republican thought, he believes this principally to have consisted in the concepts of hierarchy, harmony, and the organic simile. On hierarchy he is mistaken in two ways. First, he imagines it to have been a specifically Christian concept. It was taken over by Christianity, most famously in the sixth century by the neoplatonic theologian Pseudo-Denis, and subsequently applied promiscuously to political and ecclesiastical regimes. But its roots, especially as a political idea, lay deep in antiquity, for example with Iranian thinkers and in Plato's three classes. In contrast to Duby's (1980) findings on the early Middle Ages, these "three orders" are hardly even mentioned from the mid-thirteenth century onward. This contrasts with the world of Islam, where a theory of four social orders

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(taken over from Iran) was prevalent from the ninth till the eighteenth century. Second, Nederman does not succeed in proving a connection between Gerson's and Cusa's addiction to hierarchy and the republican elements in their thought. It seems to me there is no such connection and that hierarchy did not contribute to constitutionalism or republicanism.

The ideal of social harmony certainly did contribute, but it also was hardly a specifically Christian idea. Christians adopted it early and have continued to preach it in various forms ever since, but it was Plato who first described the relationship between the state's parts as like a musical harmony (*Republic* IV.44.313). Generally speaking, harmony was a way of saying mutual respect and affection.

I suspect that Nederman has been misled by the fact that both hierarchy and harmony were discussed in the context of the organic simile. He argues that I failed to take account of the prominence of this. Does this constitute, as he argues, the medieval link between republicanism and Christianity? One obviously cannot disagree as to its importance in medieval thought generally (Black 1992, 15–18) and in theories of constitutional monarchy in particular. One reason for its popularity was that (like monarchy) it could mean different things to different people. It was used sometimes to justify the authority of the "head," sometimes to justify the necessity of collaboration among *all* "members." In the latter sense, it became one of the main justifications for representative assemblies and their powers of legislating, controlling taxation, and supervising aspects of government, precisely because these assemblies mirrored in a visible microcosm the entire society and could act "as one body" (Burns 1988, 482, 498–512; Kantorowicz 1957). And, since the *whole* of society could so assemble, it was argued by conciliarists and, later, parliamentarians, that the representative assembly has more authority than the monarch acting alone, just as the whole is greater than any of its parts (Black 1979; Oakley 1962).

But the organic simile was not, once again, a specifically Christian idea. It went back, for example, to the resolution of the conflict between the Roman plebs and patriciate in the fifth century BCE. Its principal formulator in political philosophy was, again, Plato; the comparison between the soul (*psyche*) and the state (*polis*) plays a crucial role in the argument of the *Republic*. It was, to be sure, then enshrined in the New Testament by St. Paul (Rom. 12:4–8; I Cor. 12:12–14, 18–21; Eph. 4:12, 16) and subsequently played an especially prominent role in Christian social thought. (For some reason it does not appear at all in Islamic thought.) As for its contribution to republican thinking, it was prominent in theories of representative government, but it was also used to justify more absolute forms of monarchy.

There is, finally, no reason for seeing connections between earlier republicanism with its theological affiliations and Rousseau as merely "superficial" (Nederman 1998, 917) on account of Rousseau's rejection of certain (not all) Christian doctrines and medieval hierarchical ideas. One can be profoundly influenced on some points by people with whom one profoundly disagrees on others.

Behind these specific points, one must recall that Christianity did not start out with an explicit *political* agenda. It tended to take on its political coloring from its environment: Roman imperialism, modern social democracy. Such permeability was evident early, when the church Fathers adopted Stoic and neoplatonic ideas, including the notion of natural justice, and the view that ranks, government, private property, and slavery are artificial and second-best. My conclusion is that Christianity was as open to a republican interpretation, as in Savonarola (Skinner 1978, 147–8) or Milton (Gooch [1898] 1927, 265–70) and an alliance with republicanism as it was to a connection with absolute monarchy. The pious Lutheran and self-educated weaver Clemens Jaeger (c. 1500–61) urged the craftsmen and community (*gemaind*) of Augsburg to maintain their "stable, long-lasting, unaltered constitution, namely the free state and guild government" (Black 1984, 117).

Another example of permeability was evident in the fact that Cicero provided, after the Bible, the most popular moral text (*De Officiis*) of the Middle Ages and Renaissance (Bolgar 1958, 197–8, 206–8). It so happens that he was the most devout republican ideologue of all time. The view that one should prefer the common to the private good (a standard element in republican teaching because it overrode special claims for a privileged individual or family) was one example of a *conjunction* between Cicero and Christian morality. It was constantly stressed by Cicero, and it is easily deduced from the Old and New Testament.

The ideological malleability of Christianity is, of course, again evident in the way that it underwrote absolute monarchy in the East under Byzantium and, later, Russia. But even there, modern experience suggests that it was not as inescapably committed to absolute monarchy as, for example, Judaism and Islam have proved to be committed to legal arrangements adopted in their early history.

Christianity, then, was *inherently* neither monarchical nor republican. We must beware of essentialism in the history of religions or philosophies. We should not assume that being a Christian meant the same thing in the second, fifth, thirteenth, and sixteenth century CE. The use of the word *Catholic* has led to the unfounded assumption that the post-Tridentine Roman church had more in common with pre-1517 Latin Christendom than had the Protestant churches. We make the same mistake, for example, if we assume that all Muslims are of the same political orientation.

Nederman makes no reference to the evidence I presented for conjunctions between Christianity and republicanism *before* and *after* the Middle Ages. He does not comment on my argument that at least some specifically biblical teachings were used to defend republican practices, for example, the idea of leadership as service (*ministerium*, whence the ruler—the pope, for example—as *primus minister*) (Black 1970, 38) and the duty to treat others with love (*caritas*). This was sometimes used in favor of putting one's country

(*patria*) before oneself. He has, therefore, not disproved my thesis that a series of de facto historical connections exist between Christianity and republicanism and that we can no more say that Christianity was inherently monarchical than that it was inherently republican.

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Correction to "Separation-of-Powers Games in the Positive Theory of Congress and Courts"

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In my article, "Separation-of-Power Games in the Positive Theory of Congress and Courts" (Segal 1997) I compared the theoretical and empirical validity of the separation-of-powers model and the judicial attitudinal model as applied to U.S. Supreme Court decision making. I found that the preferences of the justices often fell inside the set of irreversible decisions, and that even when the preferences fell outside the set, the justices, with the exception of Earl Warren, showed little evidence of constraint. I concluded, consistent with the attitudinal model, that the justices overwhelmingly engaged in rationally sincere behavior.

Tim Groseclose and Sara Schiavoni (1998), whom I thank for their efforts, have alerted me to two errors in my calculation of the set of irreversible decisions. First, I used the formula 2CC-G (where CC = the conference committee and G = the gatekeeper) for calculating indifference points in the constraint set rather than 2G-CC. Second, I implicitly allowed Congress to pass legislation that would be vetoed and could not be overridden. Correcting this error changes the optimal behavior of the conference committee, which will not propose legislation that can be vetoed but cannot be overriden, and the gatekeepers, who must consider the conference committee's actions.

In this note I replicate the study using the corrected data and find first that the sets of irreversible decisions are a bit broader than I had originally found. This means that fewer justices could be constrained by Congress than I had originally concluded. Second, among the justices who could be constrained, there are fewer cases of significant congressional constraints. In my corrected tables 2 through 9 (numbered as in the original), the number of justices with significant coefficients ($p < .05$, one-tailed) in the proper direction drops from 17 (out of 126) to 12 (out of 102). Nevertheless, I again find support for constraint by Warren, who alone accounts for half of the positive and significant relationships. Finally, the attitudinal model continues to outperform the separation-of-powers model in direct comparisons (see corrected Table 8). I continue to find strong and consistent support for the attitudinal variable, whose coefficient remains significant ($p < .0001$) and consistent (the coefficient ranges from .32 to .33) in all six models. Alternatively, the constraint coefficient is significant in the one and only place where it was significant in the earlier work, the multiple-veto model using the constitutional preference scores. Finally, in as shown in corrected Table 9, I had found no significant coefficients for the median interaction and I still find none.

Given the tremendous similarity in the results, I conclude exactly as I did before: "As for the U.S. Supreme Court, the theoretical evidence combined with the empirical results cast serious doubt on the whether the justices vote other than sincerely with regard to congressional preferences, except on the rarest of occasions. . . . [E]vidence of strategic behavior at other stages of the Court's decisions . . . suggests that the justices can act in a sophisticated fashion *when they need to* do so. But the institutional protections granted the Court mean that with respect to Congress and the presidency, they almost never need to do so" (Segal 1997, 42–3).

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CORRECTED TABLE 2. Maximum Likelihood Estimates of Committee-Power Model

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Clark	1.02	(.59)	.06	.16	1.12	(.74)	.08	.18	18
Douglas	−.06	(.18)	.36	.51	−.02	(.16)	.46	−.04	29
Reed	−.48	(.94)	.32	−.41	.19	(1.42)	.45	−.58	11
Black	−.01	(.23)	.49	.48	−.15	(.31)	.32	.39	25
Harlan	.53	(.77)	.26	.08	.47	(.90)	.31	.24	17
Marshall	.17	(.09)	.03	−.23	.20	(.10)	.03	−.23	24
Brennan	.08	(.12)	.27	−.06	.04	(.12)	.39	−.03	34
Warren	.58	(.31)	.05	.31	.43	(.23)	.05	−.46	16
Burger	.09	(.46)	.43	.12	−.03	(.38)	.48	−.15	17
Rehnquist	−.20	(.27)	.23	.19	−.12	(.27)	.34	.25	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

CORRECTED TABLE 3. Maximum Likelihood Estimates of Multiple-Veto Model

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	.39	(.30)	.11	.36	.12	(.28)	.34	−.04	29
Rehnquist	−.21	(.19)	.14	.11	−.22	(.19)	.13	.11	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

CORRECTED TABLE 4. Maximum Likelihood Estimates of Party-Caucus Model

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Clark	1.22	(.63)	.04	.37	1.42	(.78)	.05	.45	18
Douglas	−.02	(.13)	.46	.51	.00	(.14)	.49	−.05	29
Reed	.19	(.81)	.42	−.36	−.07	(.90)	.48	−.58	11
Black	−.03	(.20)	.44	.47	.01	(.23)	.49	.39	25
Harlan	.95	(.80)	.13	.27	1.70	(.83)	.04	.75	17
Marshall	.12	(.10)	.12	−.25	.13	(.11)	.13	−.21	21
Brennan	−.04	(.17)	.41	−.04	−.10	(.17)	.30	−.02	34
Warren	.78	(.36)	.03	−.10	.89	(.31)	.01	−.61	16
Burger	−.44	(.31)	.09	−.00	−.51	(.21)	.02	−.41	17
Rehnquist	−.16	(.18)	.20	.17	−.22	(.18)	.12	.13	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

CORRECTED TABLE 5. Maximum Likelihood Estimates of Committee-Power Model with Segal-Cover Scores

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	−.06	(.23)	.40	.52	−.07	(.22)	.38	−.03	29
Frankfurter	−.21	(.39)	.31	.55	−.36	(.44)	.22	.46	15
Reed	−.07	(.31)	.43	−.34	−.35	(.31)	.16	−.65	11
Black	.00	(.25)	.50	.48	−.11	(.27)	.35	.40	25
Harlan	.29	(.45)	.27	.04	.30	(.46)	.27	−.17	17
Stewart	.17	(.30)	.39	−.07	.35	(.55)	.27	−.34	23
Marshall	.11	(.07)	.08	−.20	.17	(.10)	.05	−.22	24
Brennan	.08	(.11)	.34	−.07	.02	(.12)	.45	−.03	34
Warren	.63	(.27)	.02	.35	.54	(.26)	.04	−.49	22
Burger	.01	(.38)	.49	.11	.03	(.33)	.46	−.13	21
Blackmun	.08	(.46)	.44	.24	−.28	(.35)	.22	−.22	22
Powell	−.66	(.38)	.06	−.11	−.62	(.39)	.07	−.15	16
Rehnquist	−.23	(.28)	.22	.18	−.16	(.28)	.29	.24	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

CORRECTED TABLE 6. Maximum Likelihood Estimates of Multiple-Veto Model with Segal-Cover Scores

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Black	.35	(.44)	.23	.41	.61	(.47)	.11	.20	25
Blackmun	.11	(.44)	.41	.23	−.21	(.34)	.28	−.19	22
Rehnquist	−.19	(.21)	.19	.15	−.25	(.22)	.14	.11	21

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

CORRECTED TABLE 7. Maximum Likelihood Estimates of Party-Caucus Model with Segal-Cover Scores

	Without Controls				With Controls				
Justice	β	S.E.	p	ρ	β	S.E.	p	ρ	n
Douglas	.12	(.25)	.33	.43	.10	(.25)	.35	−.08	29
Frankfurter	−.07	(.43)	.44	.57	−.13	(.47)	.40	.50	15
Reed	.03	(.29)	.47	−.36	−.17	(.32)	.31	−.63	11
Black	−.12	(.20)	.28	.51	−.16	(.21)	.24	.45	25
Harlan	−.72	(.66)	.15	.23	−.83	(.58)	.10	.55	17
Stewart	−.31	(.84)	.36	−.02	.32	(.81)	.35	−.33	23
Marshall	.08	(.09)	.20	−.22	.08	(.10)	.21	−.17	24
Brennan	−.04	(.13)	.40	−.04	−.10	(.13)	.24	−.02	34
Burger	−.39	(.25)	.08	−.04	−.41	(.18)	.03	−.41	17
Blackmun	−.28	(.36)	.22	.16	−.42	(.23)	.04	−.32	22
Powell	−.35	(.32)	.16	−.00	−.34	(.33)	.16	−.05	16
Rehnquist	−.23	(.20)	.13	.13	−.27	(.19)	.09	.10	21
Stevens	−1.24	(.67)	.05	−.69	−1.28	(.70)	.05	.75	17

Note: The dependent variable is annual voting in statutory cases; β's are unstandardized regression coefficients of independent variable constraint; significance tests are one-tailed; ρ = first order autoregressive coefficient. Controls are for the term of the Court and the aggregate direction of the lower court decisions being considered.

CORRECTED TABLE 8. LSDV Estimates of Pooled Models

	Constitutional Preference Measure			Segal-Cover Preference Measure		
Estimates	Committee-Power Model	Multiple-Veto Model	Party-Caucus Model	Committee-Power Model	Multiple-Veto Model	Party-Caucus Model
Constraint						
β	.00	.31	.06	−.01	.15	−.03
S.E. β	(.08)	(.14)	(.08)	(.09)	(.16)	(.08)
p	.46	.02	.21	.44	.19	.36
Std. β	.00	.08	.03	−.01	.05	−.02
Ideology						
β	.32	.33	.33	.32	.33	.32
S.E. β	(.06)	(.06)	(.08)	(.06)	(.06)	(.06)
p	.0001	.0001	.0001	.0001	.0001	.0001
Std. β	.42	.42	.43	.41	.43	.41

Note: n = 412 for all models. Dummy variable coefficients will be provided by author on request.

CORRECTED TABLE 9. LSDV Estimates of Pooled Models with Median Interaction

	Constitutional Preference Measure			Segal-Cover Scores		
Estimates	Committee-Power Model	Multiple-Veto Model	Party-Caucus Model	Committee-Power Model	Multiple-Veto Model	Party-Caucus Model
Constraint						
β	.01	.31	.06	−.02	.16	−.04
S.E. β	(.08)	(.14)	(.08)	(.09)	(.17)	(.08)
p	.47	.02	.22	.41	.18	.31
Std. β	.00	.08	.03	−.02	.05	−.03
Median						
β	.47	.42	.41	.20	−.15	.20
S.E. β	(.47)	(1.92)	(.49)	(.15)	(.35)	(.18)
p	.17	.42	.21	.10	.34	.14
Std. β	.03	.01	.02	.04	−.01	.03
Idelogy						
β	.32	.33	.33	.33	.33	.33
S.E. β	(.06)	(.06)	(.06)	(.07)	(.06)	(.06)
p	.0001	.0001	.0001	.0001	.0001	.0001
Std. β	.42	.42	.42	.43	.43	.42

Note: n = 412 for all models. Dummy variable coefficients will be provided by author on request.

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BOOK REVIEWS

Political Theory

The Limits of Rawlsian Justice. By Roberto Alejandro. Baltimore, MD: Johns Hopkins University Press, 1997. 208p. $39.95.

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"Liberalism," declares Roberto Alejandro, "is a cry of fatigued souls inhabiting a worn-out century" (p. 4). In his irreverent introduction to *The Limits of Rawlsian Justice*, Alejandro accuses recent liberal theorists of trying to make complacency comfortable. In his view, liberals aim to justify and even exalt the status quo, with the ultimate hope of discouraging challenges to existing social and economic arrangements. While liberal theorists may seek to quiet citizens, they certainly have galvanized scholars. This is especially true in the case of John Rawls, whose landmark treatise *A Theory of Justice* (1971) continues to evoke reams of response. Even those weary of the endless stream of secondary literature on Rawls will be refreshed by Alejandro's elucidating, engaging, and genuinely original contribution. In *The Limits of Rawlsian Justice*, the author argues for nothing less than a "farewell to Rawlsian justice" (p. 179). To achieve this ambitious goal, Alejandro seeks to show that Rawls's project is "incoherent" and thus fails even when judged on its own terms.

Alejandro develops his case in a series of close encounters with key Rawlsian texts, focusing significant attention on *A Theory of Justice* (1971). In this work, Rawls presented an elaborate justification for the so-called difference principle as a guiding norm in a just regime. Alejandro summarizes the idea this way: "By arranging inequalities to the benefit of the most disadvantaged, society recognizes their moral agency and treats them as ends" (p. 68). Largely on account of the radical potential of this aspect of justice as fairness, Rawls has been widely credited with supplying a principled basis for the redistribution of wealth in the modern welfare state. Alejandro thinks this view is far too generous. He suggests that Rawls's support for the difference principle rests on "prudential calculations" not moral considerations. As far as Alejandro is concerned, the difference principle amounts to little more than "an insurance for the better-off and a kind of political bribe for the most disadvantaged aimed at guaranteeing social cooperation" (p. 65). One may expect Rawlsians to approve of social and economic inequalities which, while significant, do not provoke violent resistance. After arguing that Rawls's commitment to egalitarianism is morally shallow, Alejandro questions the utility of the difference principle as a pay-off for peace. Because Rawls leaves the specific implications of the difference principle open to political debate, it is likely to produce rather than quash just the kind of disagreement and instability Rawls hopes to avoid.

As his argument evolves, Alejandro takes aim at other aspects of the Rawlsian paradigm as well. In a chapter provocatively titled "Rawls's Communitarianism," Alejandro challenges the assumption that as a liberal, Rawls has no serious conception of community. Through an insightful reconstruction of Rawls's original argument, Alejandro shows that Rawls relies on an ideal of community as "a utopia of harmony, cooperation, and a uniform human nature that best expresses itself by following the dictates of justice" (p. 113). In a surprising reversal of Sandel's well-known critique, Alejandro suggests that the problem with the Rawlsian conception of community is not that it assumes selves that are too "thin" to forge meaningful bonds, but rather that Rawls embraces a too simple conception of human nature, one which assumes that liberal subjects place justice above all other values. Alejandro's intervention breaks the liberal-communitarian stalemate by shifting the terms of the debate, and one can only hope that interested scholars will give his argument the careful consideration it deserves.

In highlighting forgotten aspects of Rawls's theory, Alejandro does not mean to enhance the credibility of the ideal or to generate more interest in it. Quite the contrary. Alejandro repeatedly insists that it is high time to move beyond Rawls. While he officially seeks to discredit Rawls's theory of justice, however, Alejandro's method inadvertently reaffirms its relevance. Alejandro may question the logical integrity of the Rawlsian ideal, but he readily admits a close connection between Rawlsian reasoning and liberal political culture in the United States. In his own analysis, Alejandro moves seamlessly between close readings and cultural criticism. For example, he declares Prozac an "emblem of the liberal culture" and then proceeds to assail Rawls for encouraging the same kind of self-satisfied indifference he imputes to liberal pharmacology (p. 12). At his best, Alejandro forces us to consider whether in a liberal society apathy is the price we must pay for stability. But the resonance of Alejandro's criticisms suggests that the enduring scholarly fascination with Rawls may have less to do with the philosophical rigor of justice as fairness and more to do with Rawls's ability to articulate the unspoken assumptions lurking beneath the surface of politics in a liberal society. If this is the case, then "bidding farewell" to Rawls requires more than revealing the internal flaws of the Rawlsian ideal: It demands a shift in critical attention from theory to society. Disavowing Rawls is a first step but a small one if we do nothing to change the reality that Rawls merely reflects.

Regrettably, Alejandro shies away from this more ambitious enterprise. Instead, he fiercely defends his conscious decision to focus on the narrow question of coherence, insisting that his internal analysis provides sufficient grounds for a rejection of Rawlsianism. Nonetheless, in the end Alejandro exceeds his own mandate. He reveals in Rawls an effort to justify, legitimize, and even "moralize" a status quo which evidently falls short of Alejandro's own ideal. Reflecting on Rawls, Alejandro confesses that "deep inside our moral consciousness an inarticulated something might revolt at the well-intentioned argument that calls for restraint in the face of crass oppression" (pp. 171–2). Alejandro's admission of outrage hardly comes as a surprise, for it is clear from the outset that he is opposed to Rawls's mission, not just disappointed by Rawls's failure to execute it more smoothly. Internal inconsistencies, while serious, do not doom the Rawlsian project. Indeed, Alejandro's effort to identify fatal flaws might be used merely as constructive criticism by a Rawlsian sympathizer. Until Alejandro reveals more fully the grounds of his opposition, we will have to wait for a definitive rejoinder to Rawls.

It would be a shame, however, to let what is missing overshadow the significant contribution that Alejandro does make. This book is one of the very best in the crowded field of scholarship on Rawls. While many have observed that Rawls's project is deeply conservative in its effort to justify rather than transform liberal society, Alejandro offers the most sustained treatment of this important subject to date. *The Limits of Rawlsian Justice* will be appreciated both by those seeking an accessible exposition of Rawls's central ideas and by those interested in the broader political and philosophical significance of Rawlsian liberalism. Of course, the success of this effort suggests a certain irony. Even as Alejandro implores us to move beyond Rawls, his reinterpre-

tation inspires one to return to the old texts once again. So much already has been said about Rawlsianism, but Alejandro's innovative intervention demonstrates that there is still yet more to say.

Utilitarianism, Institutions, and Justice. By James Wood Baily. New York: Oxford University Press, 1997. 203p. $49.95.

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Baily starts his argument with the claim that utilitarianism raises two related problems that lead to one paradox. The first is that utilitarianism "requires too much" by imposing impossible costs on some people to alleviate costs to others. The second is that whatever help we may extend, we are unlikely to do much good before we run out of resources. The paradox entailed is that "utilitarianism tells us to make each and every choice such that it leads to the best consequences overall. But an insight made clearer by the development of game theory is that no choice has consequences in isolation, only in conjunction with . . . choices of many other agents" (p. viii). The solution Baily offers may be called institutional utilitarianism: "Both problems and the paradox can be resolved simultaneously. The resolution of the paradox of individual responsibilities is achieved by elaborating an account of institutions as equilibria in complex games" (p. viii).

Chapter 1 discusses some interesting issues surrounding the debate on utilitarianism. An interesting remark is that this philosophical school lends itself better than others to empirical accountability. It should be noted that deontological theories do not show this openness by principle, while pragmatic liberalism and other consequentialist theories, most important welfare economics, are just as open to empirical accountability. Baily's defense of utilitarianism against egalitarian and prioritarian ("Rawlsian") theories of justice is convincing and effective.

Chapter 2 is an effort to draw support from the works of Harsanyi, Rawls, and Hardin (and Hare). The main gain here is a clear exposition of some issues that concern the relationship between *contractualism* and *utilitarianism*.

Chapter 3 is an attempt to study how utilitarians and players the author characterizes as "minimalists" may behave in a prisoners' dilemma type of environment. This is probably the weakest chapter in the book. The author attributes nonorthodox utilities to the players who completely change the nature of the game. With due respect to the substantive argument, I think that readers familiar with the high standards set forth by three decades of systematic research on the prisoners' dilemma in the social sciences will find the treatment of this subject lacking in rigor.

Chapter 4 is a nice discussion of the role of institutions in society. In the end of the chapter the author proposes a selection of a subset v of equilibrium outcomes and the use of a concept k to rank cases by the extent to which there is uncertainty concerning the possibility of interpersonal comparison of utilities. The v selection is not very well motivated. Baily combines arguments from axiomatic social choice theory and noncooperative game theory to motivate this selection. If such a mix is at all acceptable, then more care should have been put into justifying it. As for the k concept, interpersonal comparison of utilities is not an issue of degree. It is an issue of principle. How can anyone decide what types of goods or pleasures are more amenable to interpersonal comparisons of utilities? This is exactly where J.S. Mill failed in his version of *utilitarianism*. Speaking of whom, I was surprised to find that Mill does not appear in the reference list and, as far as I can recall, is mentioned only once in the entire book.

In the following chapters Baily applies his k and v analytical apparatus. Chapter 5 is an amusing comparison between the "utopian" communist and libertarian "ideal" polities and the imperfect "real" political structures in the eye of a utilitarian. Both chapters 5 and 6 contain discussion of problems of distribution and redistribution. The author struggles to convince the reader that his analytical apparatus helps us understand (or "solve") these issues. Chapter 7 is an attempt to answer the question: What is left of utilitarianism? Unfortunately, the answer the reader gets is: "not much." As the author struggles to prove both the usefulness of his analytical apparatus and the viability of utilitarianism, the argument becomes more vague and less convincing.

Utilitarianism has one simple and elegant recent justification provided by Harsanyi and cited in the text (pp. 38–44). Harsanyi observes that maximizing the welfare of a community, under the restriction of impartiality, entails the maximization of every individual in this community and vice versa. The problem of Harsanyi's argument is the problem with utilitarianism in general, and that is the problem of interpersonal comparison of utilities with which all possible defenses of utilitarianism must deal.

To conclude, Baily tackles an important set of issues in modern political theory. Because utilitarianism relies so heavily on the notion of interpersonal comparison of utilities, any socially acceptable, normative theory must depart from any narrow interpretation of utilitarianism. The book has a nonnegligible value, as it helps clarify some of the more dicey issues in the debate around utilitarianism as a moral social theory. The main contribution of the book is to suggest that institutions and incomplete information may reduce the danger of the "parade of horribles" of which utilitarianism is often accused.

Rethinking Generosity: Critical Theory and the Politics of Caritas. By Romand Coles. Ithaca, NY: Cornell University Press, 1997. Contestations Series. 246p. $39.95 cloth, $16.95 paper.

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According to Romand Coles, there is an intrinsic relationship between giving and receiving. No doubt your parents told you the same thing. Still, it is unlikely that we recognize what this teaching demands. Our apparent acts of generosity are often less than generous because we fail to recognize our effects upon the others to whom we give. More specifically, we may not see how we seek to master others' difference from us in order to find them worthy of our gift or in the course of giving to them. The chief culprit in this theater of misrecognition is a sovereign conception of the subject. Modern individuals seem especially prone to the delusion that we are makers and masters of ourselves, dependent on neither God nor others for what we most fundamentally need and know. It is this conception of the human person and social life which Coles wants to challenge. But he wants to make this challenge without recourse to divinity and, thus, in the name of a postsecular caritas. What Coles calls receptive generosity allows otherness to appear in its otherness. Through a form of generosity which receives as it gives we may transfigure ourselves as well as others for the better.

Coles begins by demonstrating in some detail why Kant's effort to imagine human beings as at once sovereign and generous proves untenable. Almost every undergraduate

forced to read Kant feels and protests against this untenability. It is another thing, of course, to explain the emotional truth of the protest in the face of a thinker who denies reason access to such resources. According to Coles, sovereign subjectivity's generosity often gives less than it thinks because it does not adequately receive. Indeed, we may unwittingly do harm. He argues, "Kant needs otherness in order to avoid the arbitrariness of a pure self-giving subject, but receptivity appears to confront him with a heteronomy that is equally disruptive of a sovereignty of objective necessity. Kant, then, must construe receptivity and otherness so that they are drained of their disruptive contingency" (p. 27). Such a "pacifying receptivity" is manifest too in an insidious form of liberalism that seeks "to establish its singularly supreme legitimacy on the basis of the array of diversity it claims to have respected and allowed into its order" (p. 27). More often than not, however, these others have been purged of their radical alterity so that they have become what Coles names "otherness-not-otherness" (p. 36). Coles finds some signs of an alternative to such injurious generosity in Kant's *Critique of Judgment*, but his real inspiration is Theodor Adorno.

One might think that Jürgen Habermas's intersubjective theory of communicative action would be the more logical resource since Habermas believes that he overcomes Kant's shortcomings when he altogether jettisons his sovereign subject. Moreover, Habermas thinks that overcoming the philosophy of the subject is exactly what Adorno fails to do. But Coles turns to Adorno precisely to develop an alternative to this predominant form of dialogical ethics. At issue is not only whether we aspire to be sovereign subjects but how we relate to what we cannot readily see, hear, or recognize. Habermas seems oblivious to what is invisible, silent, or outside the communicable, whether nature or human (pp. 159–79). But for Coles, as for many others influenced by poststructuralism, what is not readily communicable is of crucial concern. In my view, this concern is animated, quite simply, by a desire for truth. What exceeds communication reminds us of the truth of our nonsovereignty; it is also intimately related to truth in all its fulness. What is communicable is not untrue, but it does not exhaust truth. For Coles, following Adorno, "the establishment of rational communication—even the ideal of communicability as the centerpiece of our understanding of truth—is already a foreclosure, an ideological constitution of our idea of truth" (p. 121).

I do not know that Adorno offers the most powerful critique of the problems associated with sovereign subjectivity (p. viii) as Coles suggests. Coles does do a great service, however, in reminding us if we had forgotten, or in introducing us if we have never known, of the riches Adorno has to offer. Coles' reading of negative dialectics as a reformulation of generosity is unique. Invoking "an ethical constellation of diverse solicitations that are agonistically juxtaposed" (p. viii), Coles shows how Adorno's texts cultivate receptive generosity where others have found only a totalizing utopia or a performative contradiction (pp. 76–84). Negative dialectics is "an activity of giving and receiving through which concealed suffering is revealed and higher possibilities are evoked and explored" (p. 81). Adorno "employs declarations in a constellation of concepts whose relations of tensions and overlap disrupt the declarative and solicit an active morality of thinking" (p. 82). In this way the truth of a declaration is sustained while its limits are acknowledged. But a careful relation with otherness is as reverential as it is agonistic (p. 168). Adorno's constellations, like Coles' own, seek to cultivate selves "pulled by, and develop[ed] through, the demands for consistency *and* the demands to resist it" (p. 178). Ultimately, if paradoxically, negative dialectics serves communication by attending to what is not immediately, if ever, communicable.

Both political scientists and deductively oriented political theorists often wonder at the preoccupation some of us show with what seem to them the insignificant minutia of narratives. Coles' reading of Adorno's writing practices and texts exemplifies what many political theorists of narrative argue: As Kirstie McClure puts it, attending to the narrativity of a text can unsettle and recirculate political meanings. This is not to say that reading a text is itself necessarily a political or ethical action. It is to say that when we read texts with care—receptive to the multivalent meanings they may awaken in us and generous in the transfigurations we offer in our readings of them—we may become capable of actions previously unimaginable. Coles himself exemplifies these hidden political possibilities in his concluding reflections on coalition politics and political economy (pp. 180–219).

Coles desires a postsecular caritas, a receptive generosity which need not depend upon God for inspiration. He directly confronts Charles Taylor's challenging claim that "high standards need strong sources" and his worry that our sources are dangerously depleted (*Sources of the Self*, 1989). Coles acknowledges that the ethical generosity he affirms is demanding, but he does not believe that it need be parasitic upon theological convictions it explicitly rejects as Taylor might suggest (pp. 136–7). In this regard, Coles is least persuasive when he seeks to "make a case for" receptive generosity or to articulate it as a "regulative ideal." As he well knows, Taylor worries not that we are without justifications but that our moral resources now only castigate rather than inspire. When asked what inspires us to care for truth rather than untruth in our perceptions of otherness, or to prefer good rather than not good in our relations with it, Coles best reply is that "one adopts this vantage point out of an overwhelming sense that where it is lacking 'life does not live'" (p. 137, quoting Adorno). Finally, Coles can gesture toward the event of inspiration but can neither explain nor guarantee it. In this respect Coles' reading of Adorno's negative dialectics, like the best theology, acknowledges and engages the problem of inspiration while the predominant forms of dialogical ethics seem simply to ignore it. The care Coles exercises in his writing, and the beauty it sometimes achieves, attests to the importance of inspiration as he cultivates it.

Emancipation and Illusion: Rationality and Gender in Habermas's Theory of Modernity. By Marie Fleming. University Park, PA: Penn State University Press, 1997. 243p. $40.00 cloth, $17.95 paper.

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In this insightful and much-needed book, Marie Fleming notes that feminists have done too little to investigate Jürgen Habermas's work, especially his theory of rationality. Apart from a few notable exceptions, feminists have tended to dismiss Habermas's universalist theories, or to critique ancillary elements of this work (in which effort they are spoiled for choice). Fleming instead aims for the very heart of Habermas's body of work, the view of rationality implicit and explicit in his theory of communicative action. Rather than join the chorus of critics denouncing his universalizing imperative, she exposes gender inequality structured into Habermas's philosophy and concludes that his work is not universalist enough.

Ranging as far and as wide as the Habermasian corpus itself, Fleming examines Habermas's forays into debates over just about everything: modernity, epistemology, rationality,

linguistics, discourse ethics, social evolution, art, and the bourgeois public sphere. On each of these topics, Fleming reveals a "pattern" in Habermas's theory whereby he presumes certain gendered roles but ignores the gender dimension. Fleming thereby argues that the problem of gender in Habermas's theory is deeper and more pervasive than other, similarly sympathetic feminist theorists have acknowledged. Rather than merely attack his "system/lifeworld" distinction as does Nancy Fraser, or reject the androcentric universalization principle of his discourse ethics as does Seyla Benhabib, Fleming uncovers Habermas on the one hand making socialization central to his theory of communicative action, yet on the other ignoring the gender inequality both historically and theoretically built into his model. Similarly, his rational reconstruction of historical materialism excludes from the definition of social labor the work females did in the primitive family, yet it also presupposes this labor.

Thus, Habermas's view of communicative action, based as it is on ideas of socialization and social labor, is exposed as essentially flawed. Assessing the implications of this "undertheorizing" of gender, Fleming uncovers how the weaknesses of his rationality theory undermine the formal-pragmátic analysis of the validity basis of speech; the result is that Habermas's universalization theory fails, leaving his morality/ethics distinction—already much criticized—gender-coded and fundamentally weakened. Ultimately, Fleming rightly concludes, we need a new, reconstructed universalist theory that lives up to the promise of inclusion and equality (p. 225).

In writing a book revealing structural gender inequalities in Habermas's work, Fleming is herself battling a number of problematic features built in to any such argument. First, since Habermas has drawn from a dizzying array of disciplines throughout forty prolific years, following him leads Fleming and her readers in myriad directions. This difficulty, when combined with the dense prose typical of Habermasian studies, has the reader doing considerable heavy lifting. Yet, making her argument (and Habermas's) more accessible would almost certainly make it less faithful to the breadth and depth of his work. Fleming prudently stays true to the vast and penetrating nature of his theory and her own critique.

Secondly, given Habermas's "few references to gender" (p. 109), Fleming is forced to make the most out a handful of passages that Habermas does little to explain. This, of course, is a predicament to which feminists have generally grown accustomed. Fleming's argument, however, seems especially conjectural, littered as it is with suggestions, possible readings, and conclusions that she considers "hard to avoid" (p. 115). Similarly, while her argument admirably draws from throughout Habermas's vast corpus, an entire chapter (chap. 5) revolves around a single, brief example he mentions in passing during an interview.

Although these difficulties may be largely unavoidable, Fleming often aggravates rather than alleviates them. For instance, her argument is made more abstruse than it need be by a tendency to introduce one line of inquiry only to postpone it until some point later in the argument. Furthermore, it is never made clear why she eschews a chronological analysis of the development of Habermas's theory, relegating his *Structural Transformation of the Public Sphere* ([1962] 1989) to the last substantive chapter and leaving opaque the way his "undertheorizing" of gender evolved as his project unfolded. These added complications are especially regrettable because they obscure some interesting and important detective work: Why is it that Habermas, who is clearly sympathetic to many feminist claims, largely dismisses issues of gender equity? Is this simply an oversight, understandable given the ambitious breadth of his argument, or part of a deeper problem? As Fleming's book ultimately makes clear, it is the latter.

What is less clear is why those who are skeptical of Habermas's universalist approach to rationality have any reason to want to return to it, except Fleming's belief that "feminists generally" are "very much within the philosophical discourse of modernity" (p. 225). I suspect that she is right that we inexorably move within that discourse, but this is unsatisfying given her claims (pp. 27–35) that Derrida is successful in destabilizing the very rationality upon which modernity and its universalizing tendency are based. How exactly will rendering the theory even more universalist make rationality more stable?

Fleming leaves a number of other loose ends, which is perhaps to be expected given that she is introducing a new direction of inquiry. But where, for example, is the sustained analysis of *Between Facts and Norms* ([1992] 1996)? Of course, Habermas's latest volume has only recently been translated; yet Fleming leaves few other stones unturned, at times drawing from early and as-yet untranslated essays. Apart from a few, seemingly ad hoc references to Habermas's all-too-brief comments about feminism in the new book, she ignores it. This is especially problematic since it is in this work that Habermas really attends to the role of the law in his theory of communicative action, and Fleming recognizes that an increased juridification of the lifeworld may be necessary to address issues of equality and inclusion. In particular, despite Habermas's remarks about feminism, one might argue that his new dual perspective on law opens up the possibility of a lifeworld colonized less by a de facto, bureaucratic, systems rationality, and more by validated, legal norms of equal respect.

Fleming's concluding section is largely an appeal to feminists, imploring them to reconsider Habermas's powerful if significantly flawed theory. They should. Indeed, as Fleming notes, a few have. But her critique really poses questions for all those interested in the Habermasian project. We can only hope Fleming and others now turn to answers—answers that lead to a reconstructed, universalist, and more genuinely inclusive theory.

Social Darwinism in European and American Thought, 1860–1945: Nature as Model and Nature as Threat. By Mike Hawkins. New York: Cambridge University Press, 1997. 344p. $69.95 cloth, $27.95 paper.

Gary R. Johnson, *Lake Superior State University*

"Social Darwinism" is a moral and political indictment in contemporary discourse. Use of this term is therefore a contested practice. Since virtually no one claims to be a Social Darwinist, the contest is really over how broad a group may be indicted under this epithet. Mike Hawkins's book, *Social Darwinism in European and American Thought, 1860–1945*, is (1) an attempt to expand greatly the size of the group that may be charged under this indictment, and (2) a defense of some who would otherwise be found guilty under the expanded definition of the crime. The overall purpose of Hawkins's book is to condemn as a Social Darwinist anyone who applies an evolutionary perspective to human behavior (except for some confused but otherwise enlightened souls whose political views Hawkins is in sympathy with).

Contrasting his definition with more conventional definitions, Hawkins says that Social Darwinism is not a discrete ideology, but rather a world view based on five evolutionary assumptions. The most crucial of these is that evolutionary

theory applies not only to human bodies but also to human social and psychological attributes. The effect of this definition is to make a Social Darwinist out of anyone who uses evolutionary theory in an effort to understand human behavior.

Hawkins refers to this fifth assumption as "biological determinism," and applies it to any thinker who believes that human biology has *anything* to do with human behavior. Of course, no one believes that human biology is the sole determinant of human behavior. It is this empty cell that could most appropriately be labeled biological determinism, but Hawkins chooses instead to apply this term to anyone who is not a cultural determinist (his own position). Hawkins employs this simplistic biology/culture dichotomy to reconstruct selectively intellectual history following the publication of Darwin's *The Origin of Species*. He begins by arguing that Darwin himself was a Social Darwinist: "It is both perverse and inaccurate to deny his status as a Social Darwinist" (p. 36).

Part II is devoted to "Pioneers," with one chapter each devoted to the emergence of Social Darwinism, and Social Darwinism in France and Germany. These chapters briefly review the Social Darwinism Hawkins says he finds in a variety of mostly nineteenth-century thinkers and seek to show the "janiform quality of Social Darwinism discourse"—that nature is sometimes viewed as model and sometimes as threat (p. 18).

Part III is devoted to selectively chosen "case studies." There are chapters devoted to liberal/radical Darwinism, racism and militarism, eugenics, sex differences and sexism, and Nazism/Fascism. In effect, these chapters provide a defense of the view that applying an evolutionary perspective to human behavior implies support for conservatism, capitalism, racism, militarism, imperialism, eugenicism, sexism, and Nazism.

Hawkins acknowledges that evolutionary approaches have been put to a wide variety of ideological uses. Thus, liberals, socialists, communists, anarchists, pacifists, racial egalitarians, anti-imperialists, anti-eugenicists, and feminists have often been Darwinians. Hawkins says, however, that Darwinism has "discursive boundaries." For that reason, those he considers progressive thinkers either are not really Darwinians—despite what they say—or their theories suffer from "incoherence" or "inconsistency" or "tensions" because of the Darwinian elements. Conservatives, capitalists, militarists, imperialists, racists, eugenicists, patriarchalists, and Nazis, by contrast, may use evolutionary theory coherently in defense of their doctrines.

Knowledgeable readers will be deeply skeptical of these claims. They will notice, for example, some suspicious convenience in the selection and coverage of relevant thinkers. There is no mention, for example, of Karl Kautsky or Michael Bakunin, both of whom were communists and evolutionists. Influential communist anarchist Peter Kropotkin warrants only three pages, and these are mostly an apologia. In contrast, more than nine pages are devoted to an obscure French librarian, Georges Vacher de Lapouge, who tried to use a Darwinian framework as a ground for his virulent racism.

Knowledgeable readers also will be disturbed by the author's distortion of the scholarly record. For example, Hawkins is clearly sympathetic with the political values of Kropotkin. Since he does not want Kropotkin to be tainted as a "Social Darwinist," however, he tells us that Kropotkin's view of the world is really not Darwinism, and that "mutual aid was not, for him, a consequence of natural selection" (p. 179).

These claims are simply untrue. Kropotkin tells us explicitly that his ideas about mutual aid and evolution are "nothing but a further development of the ideas expressed by Darwin himself" (*Mutual Aid*, 1914, p. x). He also makes clear throughout his famous work that cooperation is a product of natural selection. Indeed, he says—in a paragraph preceding one from which Hawkins quotes—"competition is not the rule either in the animal world or in mankind. It is limited among animals to exceptional periods, and *natural selection* finds better fields for its activity. Better conditions are created by the elimination of competition by means of mutual aid and mutual support. In the great struggle for life . . . *natural selection* continually seeks out the ways precisely for avoiding competition as much as possible" (1914, p. 74, emphasis added).

If it is useful to Hawkins's agenda for Kropotkin to be an anti-Darwinian, it is also useful for the twentieth century's worst monster to be a quintessential Darwinian. Thus, Hawkins tells us that Darwinism was a "foundation" for Nazism. Indeed, he says, Nazi "theory and practice were predicated upon the [Social Darwinist] world view *to a remarkable degree* (p. 284; emphasis added). Involuntary sterilization, euthanasia, and the Holocaust itself "received their legitimation from the Social Darwinist world view" (p. 280). And by contrast with the liberals, socialists, pacifists, racial egalitarians, and feminists who thought they were Darwinians—but whose theories are "inconsistent" with Darwinism according to Hawkins—Hitler "showed a *consistent* adherence to the premises of Social Darwinism" (p. 282).

It apparently does not trouble Hawkins that Hitler makes no mention of Darwin or "evolution" in the 688 page of *Mein Kampf*. And there is no indication that Hitler had anything other than an extremely crude understanding of natural selection. Of course, Hitler does rant regularly in *Mein Kampf* about a "struggle for existence," applying this phrase sometimes to his own life, sometimes to individuals, sometimes to nations or peoples, sometimes to races, and sometimes to "mankind" or "the species." On the basis of these passages, we may fairly say that Hitler sometimes used a crude, pseudo-Darwinian framework as a rationalization for his policies. We cannot on this basis, however, say that Hitler was a Darwinian.

Hawkins also uses forms of distortion. Thus, those familiar with social evolutionary thinking in the nineteenth and early twentieth centuries know that the terms "savage" and "barbarian" were used regularly by a wide variety of thinkers. These terms are offensive to contemporary ears. Hawkins takes full advantage of this sensitivity by regularly noting the use of these terms by those whose social and political views he disdains. However, he almost never acknowledges use of these terms by those with whom he is in political sympathy. Thus, Hawkins quotes use of one or both of these terms in his discussions of Walter Bagehot, Cesare Lombroso, Herbert Spencer, William Graham Sumner, Ludwig Gumplowicz, and others. But there is no mention of these terms in Hawkin's discussion of Kropotkin, even though Kropotkin used these terms throughout *Mutual Aid*, including in chapters entitled "Mutual Aid among Savages" and "Mutual Aid among the Barbarians."

The selectivity and the distortions are useful for Hawkin's agenda, but his enterprise also relies upon misrepresentation of evolutionary theory. In introducing his discussion of Kropotkin, for example, he uses "natural selection" to mean head-to-head competition: "The idea that *cooperation* was at least as important as *natural selection* in evolution—especially human evolution—appeared early in the history of Social Darwinism" (p. 177, emphasis added). This misuse is

convenient for his purposes—cooperation can obviously not be the *product* of natural selection if it is the *antithesis* of natural selection. This conceptual sleight of hand—and misuse of evolutionary theory—probably grows out of the moral discomfort Hawkins feels about cooperation at one level being the product of something akin to competition (natural selection) at another level. Kropotkin himself, however, had no trouble with this concept (*Mutual Aid*, 1914, pp. 17–18).

The book's final chapter (which has the moral tone of an exposé), provides an extended caricature of contemporary sociobiology. Using the same kinds of distortion and misrepresentation he uses in earlier chapters, Hawkins says that sociobiology is "a particularly powerful example of Social Darwinism" (p. 292). He acknowledges that contemporary sociobiologists hold a wide variety of political views; however, those who hold morally acceptable views are apparently, like their predecessors, confused. Consistent with this theme, the book's final sentence warns sociobiologists that they cannot claim "innocence" about the implications of their theories.

Social Darwinism in European and American Thought, 1860–1945 is a deeply biased presentation of scientific and intellectual history in the service of a political agenda. The author demonstrates considerable erudition, but it is an erudition contaminated by his ideological commitments. The book is certainly more sophisticated than the website of Humanitarian International, which proclaims that "Social Darwinism is the root cause of all of the world's current problems" (www.geocities.com/Athens/1058). Unfortunately, it is not substantially more credible.

Seeds of Virtue and Knowledge. By Maryanne Cline Horowitz. Princeton, NJ: Princeton University Press, 1998. 373p. $49.50.

Alan Levine, *American University*

This book has impressive breadth. It traces the use of vegetation as an image for human development through 2,000 years of Western history in numerous cultures and ways of thought, including Hellenic Greece, Christian and Jewish throught from ancient through medieval times, the Renaissance, and early modern times (especially in France—Bodin, Montaigne, and Charron each get separate chapters). The book examines texts primarily, but it also examines other media in which the analogy between culture and horticulture is found, including architecture (Avignon's papal palace), painting, book illumination, and other fine arts. In support of this approach, and uncharacteristic of books reviewed in the *APSR*, *Seeds of Virtue and Knowledge* has pictures, descriptions of which are elegantly woven into the multimedia tale that Horowitz tells. Horowitz discusses her array of topics with an astute knowledge, as this study is the culmination of many years of work. With such a wide scope, her discussion of each topic must necessarily be somewhat abbreviated, although she typically says enough to make her points convincingly.

The book is very informative on the nature and history of Stoic thought, a subject which since the works of Quentin Skinner and Richard Tuck is becoming increasingly popular. Beginning with the Stoic claim that every human being possesses sparks of reason, common notions, "seeds of virtue" and "seeds of knowledge" (hence the title) in his or her soul, Horowitz shows the various ways that this teaching has been interpreted and merged with other vegetative imagery in other intellectual traditions (the "burning bush," "trees of knowledge," etc.) throughout time. Hence, the book is full of arguments in favor of self-cultivation and individual blossoming, and it locates these arguments in the context of the blossoming of particular cultures (Rome, Florence, Paris). Because it is so comprehensive, the book will cover some material familiar to specialists of the figures and traditions discussed, but it is illuminating in many of the connections it makes and is bound to be full of little nuggets of knowledge for everyone, from a casual reader to the most sophisticated scholar. This reviewer, for example, was intrigued by Horowitz's discussions of the meaning of the word *renasci* (the root word for the Renaissance) as "to grow again" not only as "to be reborn," on early uses of the phrase "human condition" (Pope Innocent III), and the whole genre of *floriligia*. With all these tidbits, the book is suggestive and provocative beyond its immediate claims. There is some confusion about whether the book is simply tracing Stoic imagery through time or rather tracing vegetative images wherever they are found. But in fairness these two possibilities are related and the book does both, tracing how these images are incorporated into and used by other traditions, which may or may not have had pre-existing vegetative imagery of their own.

The book has two goals. Its more moderate, scholarly, goal is that "this book joins others in providing evidence that the idea of human potential for knowledge and virtue is a key idea of Western culture" (p. 19). This is amply demonstrated throughout. Horowitz's concern here seems to be with both those who ignore the West's traditions of virtue and those who in her view unduly emphasize the religious aspects of virtue. Horowitz's second, more ambitious, goal concerns the importance of Stoic ideas for today. "My hope," she writes, "is that greater understanding of the history and diverse applications of a cluster of phrases and of their accompanying educational and civic strategies will be constructive for reaffirming human dignity" (p. 19) and that these "may aid ecumenical efforts for building mutual respect and community among peoples of diverse faiths and traditions" (p. 19). Horowitz's choice of Stoicism to support such ecumenical goals is a good one as her work nicely explains and articulates various manifestations of the Stoic teaching that every human being, male and female, master and slave, etc. is born with seeds of knowledge and virtue already implanted in their souls. The goal of a good society and each of its members should therefore be to help these seeds "blossom," to "cultivate" and "nurture" them, so that everyone's potential may be fulfilled. Indeed, Horowitz's book is filled with an impressive array of thinkers from ancient, medieval, renaissance, and early modern times who used such arguments as evidence for the existence of free will, to provide an epistemologial foundation for the scholarly disciplines, for having a program of and open access to learning, to extending the humanist education to women, and in general to argue for human equality (women, slaves, peasants, and the Amerindians included) and against tyranny and cultural domination. Horowitz's book shows that there is much about the Stoics' arguments which is attractive. They systematically work through and make arguments for claims about virtue that today are either assumed or ignored. Any argument which takes seriously the need for knowledge and virtue is welcome, and these arguments, as Horowitz presents them, serve well her ecumenical aim.

More explicit treatment of a few points, however, would strengthen the book. For example, while the Stoics did argue for equality in terms of potentiality (seeds), they also recognized a limited circle of only a wise few in whom this potential would actually be realized. Horowitz knows this, but her discussion of this elitist strand could be more explicit throughout the book. Her discussion of Montaigne, for

example, rightly identifies peasants and the Amerindians as embodying a kind of universalistic ideal, yet she might have balanced this by noting Montaigne's more elitist praise of figures who possess self-knowledge, such as Socrates. Horowitz, like some of the later thinkers she discusses, self-consciously downplays the elitist strands of Stoic thought in order to make it more egalitarian and hence palatable. Other kinds of qualifications of Stoic thought could also be noted in several figures, such as Cicero, Aquinas, and Bodin. In the absence of the Stoics' own elitist (and other kinds of) qualifications of their universalistic seeds teaching, Horowitz is left with what she herself calls an "optimistic and inclusive pattern of thought" (p. xiii) and an "optimistic assessment of human nature" (p. 254). Her approval of Anne Frank's naive optimism as she is about to be murdered ["Yet I keep them [my ideals], because in spite of everything I still believe that people are really good at heart" (p. 20)] further reinforces this point. The gap between this optimistic description of human nature and the one that underlies the views of the great liberal thinkers is striking and raises several questions. What kinds of political institutions would be built upon such an optimistic view of human nature? Horowitz and the authors she discusses are, of course, aware of the nasty and cruel acts human beings commit all the time. To the extent that Horowitz's Stoicism emphasizes universal goodness, might it be less able to foresee and protect against cruel and destructive acts? One wonders whether such an optimistic theory of human nature would lead to less alert and less vigilant institutions, and thus not to more but to less just treatment of human beings in the real, actual world. Perhaps this explains why Stoic philosophy has never served as the basis for any existing government. In any case, a more explicit confrontation of the less "optimistic" aspects of Stoicism and other, more realistic aspects of Montaigne, Bodin, and others might give more satisfying answers to these political problems and strengthen Horowitz's already good book.

If this book's optimism is questionable, perhaps it is because the Stoics themselves barely extend their claims about individual human potentiality to practical political institutions. The strengths of Stoicism as discussed by Horowitz rest more in the educational as opposed to the political programs it implies. In any case, Horowitz's book does an excellent job of bringing out Stoic arguments and showing how in numerous times and places they have been influential, especially in education, in arguing for the pursuit of knowledge and human virtue. Why not today?

Shakespeare and the Good Life: Ethics and Politics in Dramatic Form. By David Lowenthal. Lanham, MD: Rowman & Littlefield, 1997. 274p. $68.00 cloth, $24.95 paper.

J. Scott Johnson, *Saint John's University*

On a lazy summer afternoon, political theorists who love Shakespeare will enjoy perusing David Lowenthal's *Shakespeare and the Good Life*. They will find excellent prose, ideas from classical philosophy strikingly juxtaposed with Shakespeare's thought, and evidence of a keen intellect reading and rereading seven of Shakespeare's best known plays. No doubt every student of Lowenthal will wish to own a copy of this book as well. These essays clearly reflect an approach which he says he has developed over three decades of teaching Shakespeare and political theory at Boston College. I can imagine these chapters being delivered orally before a rapt audience of devoted students.

Lowenthal's basic claim is that Shakespeare "generally sought to conceal, rather than reveal, his own systematic thought or philosophy" (p. vii). He asserts that "Shakespeare wrote for two kinds of people simultaneously—the viewers and the readers" (p. 7) and that contemporary critics' "dogmatic" assumption that Shakespeare was primarily a playwright prevents them from seeing the complexity of his thought. For Lowenthal the story or the plot is only the "surface" of the play. Lowenthal, as a reader, proposes to take us more deeply into the play in order to reveal the "philosophical intention from which [Shakespeare] began" (p. x). He takes "Shakespeare to be an independently thinking follower of classical philosophy—of Socrates, Plato, and Aristotle—whose understanding of their teaching and that of their Pre-Socratic predecessors was profound" (p. x).

In the seven substantive essays that make up the bulk of this book, three of which were published previously in *Interpretation* and a fourth elsewhere, Lowenthal reveals several different aspects of the meaning that Shakespeare was hiding from his audience but not his readers. In *The Tempest*, Lowenthal discovers a consideration of Plato's philosopher-king, in *King Lear*, "natural versus conventional justice," in *Julius Caesar*, contending views of the best regime and best life." In *The Merchant of Venice*, Lowenthal shows Shakespeare's preference for a classical cosmology over both Judaism and Christianity. *Macbeth* deals with "natural necessity and the moral character of the universe," *Measure for Measure* with "the Christian view of temperance," and *A Midsummer Night's Dream* "deals with the origins of Athenian philosophy, democracy, and drama" in twelve short pages (pp. x–xi). These themes should be familiar to anyone who has read Leo Strauss. Indeed, when discussing *King Lear*, Lowenthal writes, "It is amazing to discover how much of the play parallels Strauss's account of how the idea of natural right originated" (p. 84).

The main strength of this book is the clarity of its prose and the brilliance of its commentary in juxtaposing diverse images from a variety of sources. For example, in the first essay Lowenthal uses Plato's cave to explore the significance of Prospero's Epilogue at the end of *The Tempest*. Lowenthal describes how Prospero the character sends the other characters back into his cell, which Lowenthal likens to a cave. Then the actor playing Prospero is left alone facing the audience in the theater, another cave, and ends the play. "The audience, representing the city or political life, with its conventional opinions, thinks itself free but is in chains, and must be appealed to by the philosopher-king (and poet) who is free but must pretend to be in chains, and at the mercy of the audience-city" (p. 63). Regardless of whether this was Shakespeare's intent, Lowenthal's juxtaposition forces us to rethink the issues he has raised even if we ultimately reject his claims as unsubstantiated. The work Lowenthal asks us to do in reading his book exercises our minds and frees us to read Shakespeare anew. Lowenthal could have done an even better service to the plays and political theory had he allowed himself more freedom from the Straussian chains that bind him.

Leo Strauss was original. His thought was persuasive in part because of its originality. There is much that can be learned from Strauss's work and that of his followers, yet, it often seems that the followers are more concerned with being true to Strauss than to their own insights into the works they are commenting upon. This is often true as well of the followers of Freud or Marx, persons whom Foucault labels "founders of discursivity." These founders "are unique in that they are not just the authors of their own works. They have produced something else: the possibilities and the rules for the formation of other texts" ("What Is an Author?" In Josué Harari, *Textual Strategies*, 1979, p. 154). Leo Strauss produced

the possibility and the rules for the formation of Lowenthal's text. I cannot imagine what he would have written had Strauss not existed.

When different theorists discuss a particular text using different discursive models they often find that some parts of the text's structure appear alike in their disparate models. If we read Shakespeare as a Freudian we will read a somewhat different play than if we read it as a Marxist or a Straussian. Yet, some facts remain. These facts (for example, the exact words of dialogue) must be taken account of in each of these different readings of the play and, while they may appear somewhat differently in each, these facts will constrain the readings. Understanding the "permanence" of some facts between readings and what this permanence might imply for the meaning of the play requires multiple readings of the play within conflicting traditions of discourse. Lowenthal's book contributes to this broader project by providing us with an excellent example from the Straussian project.

While I enjoyed this book immensely and believe it contributes to political theory, I have two suggestions. First, the literature review is much too abbreviated and ends with works published more than thirty years ago. Students of Shakespeare have published an enormous amount of exciting and influential scholarship on each of the plays discussed which seems to have been read by Lowenthal but is neither explicitly commented upon nor noted. This leads to my impression that instead of naming the authors he disagrees with Lowenthal creates straw men to succumb to the four dogmatic assumptions, which, he says, prevents them from seeing the plays properly or as he does (pp. vii–ix). The dogmatic assumptions are not well explained, and I doubt that he can show persuasively that these prejudices are "well-nigh universal" (p. viii). My second suggestion is closely related to the first. Lowenthal's book needs more thorough citation. His two statements, "I rarely refer to other things written about these plays" and "I took for my models two classics of Shakespearean interpretation . . . both of which are also quite niggardly in citing others" do not excuse this oversight on his part (pp. xi–xii). Senior scholars cannot play fast and loose with their debts to others. It diminishes the scholarly enterprise and sets a bad example for us all.

Given these serious limitations, I reiterate that this book is delightful to read and I recommend it quite highly for its heuristic value. It will make every reader think again about Shakespeare. However, it is not a book that is engaged in the contemporary debates about Shakespeare and it does not attempt to respond to those who disagree with its insights.

Socrates' Education to Virtue: Learning the Love of the Noble. By Mark J. Lutz. Albany, NY: State University of New York Press, 1998. 214p. $19.95.

Judith A. Swanson, *Boston University*

If liberal democracy stands above all for freedom, then should it promote virtue? Lutz's very readable book not only gets directly to the crux of the matter—namely, "what is virtue?"—but also gives a clear and compelling answer. The central claim of the book is that if we want to understand virtue, then we should study the best account of it—Plato's account of Socrates. Socrates discovered that the chief human virtue is self-knowledge and the chief revelation of that knowledge is a desire or longing not simply for freedom as such but for the good or noble. Indeed, Socrates shows that love of the noble is what animates or inspires reason or philosophical reflection. Lutz suggests then that unless liberal democracy encourages Socratic philosophy or education, the freedom it preserves will not gratify human nature.

Unfortunately, according to Lutz, two predominant ways of thinking or cultural legacies discourage the cultivation of virtue by mocking either love of the noble or philosophical reflection. Although Lutz does not make it his project to critique Enlightenment rationalism, he suggests that it refuses to recognize such apparently nonrational endeavors as poetry and religion as expressive of a fundamental human longing for self-transcendence or the noble and thus it repudiates the ancient view that such longing, and not just the selfish passions, animates the soul or reason.

By contrast, and worse from the point of view of Socratic virtue, postmodernism dismisses objective, universal reason as Western, historical myth, thereby declaring all judgments relative. In an early chapter, Lutz masterfully debunks Richard Rorty's use of Nietzsche's historical conception of the self to defend liberal democratic morality and politics. According to Rorty, liberalism need not presuppose universal reason or a human nature because its justification does not have to be philosophical; political community is justified or moral sui generis, not by virtue of its rational foundations. Indeed, Rorty says, because community is constituted by conversation, its participants exhibit Socratic virtues. According to Rorty, it is Plato, not Socrates, who holds the illiberal view that conversation is worthwhile or virtuous only if philosophically purposeful, revelatory of truths. Lutz worries, however, that if a person believes that his "language, conscience, morality, and highest hopes [are] 'contingent products,'" then what "would prevent him from unleashing whatever political projects he might envision"? (pp. 31, 38). If, Lutz implies, alternatives to radical contingency are not kept in view by philosophy as aided by Plato's doctrine of the ideas, then we have no moral or political constraints. Lutz thus argues contrary to Rorty that Socratic dialectic is not simply depicted but is also vindicated by Plato.

Lutz makes clear that although Plato or "Platonism" vindicates Socratic philosophizing, Socratic wisdom is not, contrary to the contention of many commentators, simply apprehension of theoretical truths or ideals. Lutz shows, through discussion of *The Apology, Symposium, Republic*, and the lesser known *Alcibiades Major*, that Socrates not only inquires into political, moral, and religious questions or human concerns, but characterizes the whole of his knowledge as knowledge of love.

Lutz explains nonetheless that Socrates claims expert knowledge of erotic matters because he is a philosopher and thus exemplifies love's purest condition. Eros is not "desire in general" but "a single, particular desire, linked to but distinct from human sexuality and rationality" (pp. 87–8). It seems to be between mortal and immortal and thus what moves the soul.

Souls are moved by eros to find what they need, and the needs of souls, as well as the persistence of their eros, differ: "We love only what we believe we need and what we believe we have the resourcefulness to acquire. Should we believe that we are not in need or that we are powerless to acquire what we need, our eros dies" (p. 88). An important aim of Socratic conversation, Lutz notes, is to stimulate desire for knowledge by making individuals aware of their ignorance.

Lutz argues, however, that Plato deliberately complicates or at least questions Socrates' beliefs about eros through challenges put forth by two interlocutors in particular—the comic poet Aristophanes and the ambitious young man Alcibiades. Lutz observes that the account of eros given by the character Aristophanes, which depicts eros as the desire to be reunited with our own particular "other half," Freud

treats as Plato's own account. Plato's message is rather, Lutz implies, that such desire is futile: "Just as our ability to live without our other halves testifies that we are not literally halves of one being, our capacity to love new beloveds suggests that erotic attraction is based not on true and perfect 'complementarity,' but on a recurring illusion that such complementarity exists . . . lovers are *always* in search of their one and only; the perfect complement for whom we search is never to be found. . . . Love affairs eventually fail because no one turns about to be the true one and only. Eros inclines us all to adultery rather than to lasting marriages" (pp. 68–9).

Although Plato does not, according to Lutz, view eros as intense psychic desire for another person but rather as such desire for the beauty of knowledge, Lutz's discussions of *The Republic* and *The Clouds* suggest that Plato and Aristophanes nevertheless agree that love of the city, the family, and the gods is politically salutary. Whereas city and family inspire the courage to be perfectly selfless and perfectly noble, the gods inspire reverence with the hope that they will recompense us with our true other half. Although Plato the philosopher and Aristophanes the comedian seem to believe, respectively, that only private prudence and irreverent laughter yield justice, neither openly spurns piety.

As for the young and vain Alcibiades, who wants both to rule Athens and arouse Socrates, he is critical of Socrates' understanding of virtue because it requires becoming unpreoccupied with politics and sex. Alcibiades gripes that Socrates merely pretends to be erotic or in love to attract students but is in fact in full self-command and self-sufficient—a "real man" (p. 137) who yearns for nothing noble or sensual not even him, the gorgeous Alcibiades. More to the point, Alcibiades thinks that Socrates must be a freak to pass up the chance to do to him what every other grown man wants to.

But Alcibiades' indignation is noteworthy, Lutz suggests, because it confirms Socrates' belief that souls are moved by longing for the noble and good. That Alcibiades is incredulous that Socrates does not want to let himself be overcome by him reveals not only Alcibiades' vanity or *thumos* but his deep psychic belief that beauty can overpower. What Alcibiades fails to understand, of course, as Lutz explains, is that his beauty or nobility does not rival the beauty of speeches partly because of that lack of understanding; if he were truly erotic, he would not let pride preclude gratification of his soul and could truly admire rather than resent Socrates.

Through such perceptive discussion of a number of Plato's dialogues, Lutz answers the question he began with. Unless liberal democracy promotes education that gratifies the human soul by inducing reflection about what is noble, good, just, and sacred, its citizens will learn secular relativism and never long for excellence.

Natural Causes—Essays in Ecological Marxism. By James O'Connor. New York: Guilford, 1998. 350p. $39.95 cloth, $19.95 paper.

Eric Darier, *Lancaster University*

This book is the synthesis of James O'Connor's lifetime attempt to explain why Marxism is relevant to an analysis of contemporary capitalism. For example, his *Fiscal Crisis of the State* (1973) is a standard key seminal text on the significance of the post-Fordist era. O'Connor's more recent achievement, however, was to build strong theoretical and practical links between Marxism and environmentalism. James O'Connor, formerly at the University of California in Santa Cruz, is editor-in-chief of the journal *Capitalism, Nature, Socialism: A Journal of Socialist Ecology* and the "Democracy and Ecology" series editor at Guilford Press, a formidable achievement in a U.S. academic climate generally fairly hostile to Marxism. The collapse of the Berlin wall and the subsequent end of state socialism provide an opportunity to reassess Marxism out of the context of the Cold War.

The central point that O'Connor presents is that to understand the current environmental crisis it is vital to understand the broader economic and political conditions which created that crisis in the first place. For the author, human history and natural history are "dialectically interconnected." Therefore, Marx's analysis of historical materialism is central to O'Connor in a structural understanding of the current environmental crisis. In the final analysis, O'Connor adopts a strong theoretical "economic determinism" that interprets the ecological crisis as the manifestation of the "second contradiction of capitalism." In a sense, O'Connor seems to be fighting a rearguard action against three sets of "enemies." The first is methodological individualism and its allies ranging from "rational choice," "social choice," to moralism. The second set is composed of those he would consider to be "environmental determinists," like Deep Ecologists. The third set is what O'Connor calls "post-Marxists," sometime "postmodernists," or "cultural determinists." These three sets of "enemies" are undermining what O'Connor understands to be "historical materialism." In fact, O'Connor's introduction is devoted in large part to a rebuttal of more recent and trendier versions of (postmodern) Marxism, which tends to focus less on "economic determinist" explanations and more on other elements such as "difference" and "social construction."

I am still puzzled when O'Connor accuses the third group of being commited to "identity politics" (instead of "historical materialism," I presume). I always thought that "class politics" was about the emergence and the sustaining of "class consciousness" that historical materialism analysis successfully rendered "visible." I am also assuming that O'Connor's central practical objective is to articulate a "Red-Green" "identity" (or "consciousness") and not simply to wait for the economic determinist condition to create it. No? There are numerous examples of economically similar capitalist countries in which "class" consciousness is deployed very differently. For example, "working-class" consciousness is still generally more prevalent—as a self-consciously expressed identity—in the United Kingdom than in the United States. A simple economic determinist analysis is not sufficient to explain differences in how "class" ("identity") is experienced in countries that might have very similar capitalist economies nevertheless.

O'Connor seems to be stuck in an epistemologico-practical double bind. On the one hand, "historical materialism" is deployed as a superior epistemological tool that enables telling a "true" story. On the other hand, the seductive power of that story is crucial to the creation of the conditions for the emergence of a new "class"/"Red-Green" consciousness although it cannot be revealed as a "story" but as "truth." This sharp epistomologico-practical divide might explain, on the one hand, the virulence of O'Connor's theoretical critique against partisans of "identity politics" and, on the other, his more conciliatory tone about the practical necessity of building coalitions between ("identity") social movements.

To appreciate critically the importance of O'Connor's contribution to "ecological Marxism," I would strongly suggest that potential readers also consult Ted Benton's edited collection *The Greening of Marxism* (1996), one of the four parts of which presents critical commentaries on O'Connor's central concept of the "second contradiction of capitalism."

Despite some of the problems inherent in trying to offer a

synthesis between Marxism and ecology, O'Connor's *Natural Causes* is already an important point of reference in current debate about environmental politics, theory, and philosophy. In addition to Deep Ecology and Social Ecology (as promoted by Murray Bookchin), O'Connor offers a third perspective on environmental politics.

W. E. B. Du Bois and American Political Thought: Fabianism and the Color Line. By Adolph L. Reed Jr. New York: Oxford University Press, 1997. 282p. $35.00.

Lawrie Balfour, *Babson College*

W. E. B. Du Bois's declaration that "the problem of the Twentieth Century is the problem of the color-line" has become a truism. Much of the authority of Du Bois's claim stems from the economy with which it captures both a central failing of American democracy and a fact of political life across the globe. Yet, the claim also derives its authority from the stature of Du Bois himself. A scholar and an activist whose lifetime spanned the period between Reconstruction and the 1963 March on Washington, Du Bois was, among other things, a leading participant in the development of American sociology, a founding member of the National Association for the Advancement of Colored People (NAACP), and an architect of twentieth-century Pan-Africanism. Still, despite the frequency with which one encounters Du Bois's words in scholarly articles, newspaper editorials, and books dealing with one facet or another of "the problem of the color-line," much work remains to be done to explore the content of Du Bois's scholarship, the intellectual context in which it was produced, and its contemporary political resonance.

Adolph Reed's thought-provoking study of Du Bois performs an important piece of that exploratory work. In his introductory chapter, Reed identifies two tendencies that have inhibited rigorous analysis of Du Bois's political thought: the treatment of African-American experiences and issues of race as exceptions to otherwise general rules of American politics and the temptation to counter such marginalization with anachronistic, uncritical interpretations of Du Bois. Reed responds to both tendencies by offering readers an historically situated Du Bois, one whose work can be linked to the intellectual trends and political imperatives of his time. From outset to finish, Reed makes it clear that the stakes of his project are not merely scholarly: the volume opens with the eighth of Marx's "Theses on Feuerbach" as its first epigraph, and it gives the eleventh thesis the final word. In between the warning about the susceptibility of theories to "mysticism" and the admonition that the purpose of philosophy is to change the world, Reed presents Du Bois as "one of the very most consistently and resolutely *political* of all twentieth-century black American intellectuals" (p. 177, emphasis in the original).

The first half of the book is a model of the sort of historically grounded scholarship Reed advocates. Reed writes learnedly about the dominant currents in American intellectual life in the late-nineteenth and early-twentieth centuries and evaluates probable influences on Du Bois's thinking about such topics as racial group identity, Pan-Africanism, and socialism. He makes a strong case for the collectivist underpinnings of Du Bois's thought and fleshes out the political implications of Du Bois's faith in the role of scientific knowledge as the engine of social reform and the role of the black middle class as agents of racial uplift. One result is that Reed complicates assumptions about Du Bois's elitism—most famously articulated in the 1903 essay on "The Talented Tenth"—thereby providing a richly textured portrait of *what kind of elitist* Du Bois was. Reed also helpfully traces changes in Du Bois's ideas about the relationship between social class and virtue, noting how Du Bois refashioned the concept of the "Talented Tenth" in response to his disappointments about the consumerism of the black middle class in the post–World War II period. Likewise, Reed unpacks assumptions about Du Bois's commitments as a democrat. Although Du Bois is rightly remembered as a champion of political and civic equality, Reed's examination of *The Philadelphia Negro* (1899) chastens against the temptation to gloss over the antidemocratic ramifications of Du Bois's faith in social evolution by relating Du Bois's comment that social stratification among black Americans is a natural product of their having democracy "prematurely thrust upon them" (Du Bois, quoted on p. 38).

Questions of leadership figure centrally in Du Bois's thought and in Reed's account of it. As Reed notes, Du Bois's conflicts with Booker T. Washington and Marcus Garvey were crucial in defining black politics in the early part of the twentieth century. Reed's attention to these internal debates provides a reminder that the thought of Du Bois and his interlocutors ought not only to be viewed within the parameters of protest. Instead, Reed indicates how much work there is to be done to recover the broader aspirations of these and other black thinkers by raising "the simple question, seldom asked of black political figures": "What sort of society would they create if they could restructure the world according to their own designs?" (p. 89). Beyond the historical value of this line of inquiry, there is the promise, for political theorists especially, of finding relatively untapped resources for thinking about democratic leadership in a racially hierarchical society and about the nature of representation more generally.

By insisting on the significance of intellectual context and defending the normative unity of Du Bois's writings, Reed strives to prevent their incorporation into agendas that Du Bois would have found uncongenial. While Reed's dedication to this end yields illuminating readings of Du Bois's thought, it also unnecessarily constrains those readings. For instance, Reed's arguments for the view of social evolution underlying Du Bois's conception of double consciousness enhances the reader's understanding of Du Bois's famous articulation of the experience of "twoness." Yet, to say that "Du Bois was unambiguously in step with the conceptual orthodoxy defined by the Lamarckian social science of his day" (p. 121) is to ignore the polyphony of Du Bois's thought and of a book like *The Souls of Black Folk* (1903) in which the idea of double consciousness is elaborated. Among the achievements of *Souls* in its display of the limitations of scientific discourse to capture and to understand the anguish of the post–Civil War period. Moving fluidly from history to polemic to elegy to fiction, Du Bois describes men and women, black and white, whose wounds cut deeper than ignorance and are not susceptible to remedy by the better organization of society. *Souls* also qualifies Du Bois's confident assertions about the "Talented Tenth." Both the chapter on nineteenth-century nationalist intellectual Alexander Crummell and the short story about John Jones evoke the alienation of educated blacks from the beauty of African-American life. Read in light of Du Bois's comment that "the price of [modern] culture is a Lie" (*The Souls of Black Folk*, p. 167), these chapters expose Du Bois's reservations about the costs of racial uplift and provide a more nuanced understanding of his politics.

It is in the latter half of Reed's book that the reader most senses a missed opportunity. The longest chapters shift the focus of discussion from Du Bois himself to disputes about

the proper direction of African-American scholarship. Reed challenges the appropriation of Du Bois's legacy in the constitution of a black intellectual tradition that, he contends, privileges literary accomplishment and "cultural authenticity" over political history and progressive political action. What is neglected in Reed's critique is the development of his own views, which are laid out briefly in the introduction, about the study of African-American political thought in relation to American political thought and political science as a whole. Where Reed laments the influence of Henry Louis Gates Jr.'s definition of an African-American literary tradition, he might instead have offered valuable insights into the role of canon formation in the study of politics. Such a discussion, moreover, would have allowed Reed to say more about the dangers of adding Du Bois's name to the pantheon of "Great Men and the occasional Great Woman" (p. 184) and about how his call for renewed attention to Du Bois as a political thinker escapes such pitfalls.

Animating Reed's argument is a sense of alarm about what he sees as a disconnection between the study of African-American political thought and the achievement of racial justice. Thus, his second epigraph, Hegel's statement that "the owl of Minerva spreads its wings only with the falling of the dusk," calls to mind the concluding words of Sheldon Wolin's "Political Theory as a Vocation" (*American Political Science Review*, December 1969, p. 1082). "In Greek statuary," Wolin writes, "Minerva's pet was a screech owl, for a screech is the noise both of warning and of pain." Even readers who do not share Reed's views about other interpreters of Du Bois's texts, or who wish for more tempered assessments of his own interpretations, can nonetheless appreciate the sense of urgency that pervades this book. That Du Bois's pronouncement about "the problem of the color-line" still rings true in the twilight of the twentieth century indicates the timeliness of such a call to arms.

The Cambridge Companion to Mill. Edited by John Skorupski. New York: Cambridge University Press, 1998. 591p. $59.95 cloth, $18.95 paper.

Robert Devigne, *Tufts University*

In the winter of 1854 coughing fits and the spitting of blood convinced John Stuart Mill that he was suffering from tuberculosis, the disease which killed his father. Confronting the possibility of an early death, Mill wrote to his wife about producing a series of essays designed to summarize their views on the most important questions. Most are not surprised to find on Mill's agenda the themes found in *On Liberty*, *Utilitarianism*, and *The Subjection of Women*, but few expect to find the topics found in *Three Essays on Religion* and "Grote's Plato."

The *Cambridge Companion to Mill* takes valuable, albeit hesitant, steps toward helping us understand why Mill found substantive reflections on religion and classical political thought so important. Mill, according to several contributors, saw the need for liberal societies to discipline themselves through a commitment to the general good while at the same time encouraging the forces of human creativity. Mill hoped to accomplish this double task by joining modern individualism to a new ethical and religious sensibility. In addition to the essays discussed below, informative analyses of these complementary political goals are found in essays by the late John Robson on civilization and culture as moral concepts, Jon Riley on political economy, Wendy Donner on utilitarianism, and Mary Lyndon Shanley on women. Peter Nicholson's essay on the reception of Mill's thought has a particularly interesting analysis of the way British Idealists at the turn of the century transformed Mill from a "public moralist" to an atomistic individualist. As he points out, this reinterpretation of Mill was a part of their effort to recast liberalism as a doctrine centering on the state rather than civil associations. John Skorupski, Philip Kitcher, and Geoffrey Scarre analyze Mill's contributions to the study of logic, mathematics, and the scientific method.

This volume reflects an emerging debate over the lessons to be learned from Mill's thought. We are all familiar and comfortable with the depiction of Mill as the champion of the individual against a tyrannical majority. But to what extent was he concerned with the dangers of moral disintegration and more interestingly still, to what extent was he engaged with joining these two concerns? The traditional view of Mill, notably articulated by Isaiah Berlin, recognizes competing emphases in Mill's thinking but is unable to find coherent aims. Indeed, in *Four Essays on Liberty*, Berlin (1984) suggests the central tenet of liberalism—choice—is based on Mill's and liberalism's ambivalence concerning what is right or wrong, good or bad. Revisionists such as Alan Ryan have identified a coherent doctrine in Mill's thinking but neglect many of his moral and religious concerns in favor of a focus on when behavior infringes on the rights of others.

An approach more fruitful than either of these is indicated by T. H. Irwin in his essay in this volume concerning classical influences on Mill. He notes that both Mill and the influential classicist George Grote—considered by Mill an heir to Bentham's utilitarianism—approved of Plato's dialectic which challenged standards of good and bad through cross-examination. To Mill, the Platonic dialectic establishes the premises for the argument on behalf of freedom of discussion in *On Liberty*. The dialectic not only serves those who wish to discover truth, it also aids those who aim to understand truth, requiring them to defend their assertions through conflict and debate.

Like Grote, Mill finds Plato's concept of the Ideas—ultimate understandings that are eternal—cannot answer the questions generated by the dialectic. Unlike Grote, however, Mill does not completely reject this positive side of Plato's thinking. Mill finds that the doctrine of Ideas reflects Plato's understanding that an idea of the good must be fostered to generate human sociality. Mill's regret is that Plato did not discover a firm basis for such a morality and ethics to allow societies to rely on more than the interdependence of interest. Plato depended exclusively on art. Nonetheless, Mill learns from Plato the need for a value system with two distinct, but equally valid ends: the promotion of creative individuals who are liberated from the constricting norms of everyday society and the development of a civilization characterized by sympathetic bonds among its members. Irwin explains that Mill's differences with Grote's view of Plato coincides with his rejection of the restricted view of human nature typical of classical utilitarian doctrine.

Irwin also could have considered other classical sources which contributed to Mill's outlook. Mill extolled the creative willfulness of the Athenians while praising the role that education played in fostering stability in Sparta. At the same time, Mill maintained that Athens was too dependent on the qualities of its most forceful citizens, and criticized Spartans for being too acquiescent. Mill consistently addressed these respective strengths and weaknesses of Athens and Sparta as a backdrop to his project of developing a new understanding of the good and the place of modern individuality within it. Mill was deeply influenced by Aristotle as well as he attempted to formulate a modern morality, weaving together the arguments that

ethics must foster habits that overcome our natural instincts, but that virtues, far from being mere conventions, reflect an assessment of the ways human nature can be either base or noble. Consequently, while Mill argues that nature itself is incapable of establishing an ethical standard of the good and is a source of more evil than benificence, nature is to be studied and amended, not ignored.

As Alan Miller points out in his essay on religion, Mill knew that religion spoke to the human desire to live a meaningful and beautiful existence. Fearing that Christianity is no longer capable of generating the ethical underpinning that a liberal society needs, Mill discusses the need to augment it with a public opinion animated by a "Religion of Humanity," an ethos that would elevate the human intellect, channel willfulness, and incline citizens to meet minimal social obligations. While Mill reserved his most comprehensive statements on a Religion of Humanity for his posthumous publications, diary, and private correspondence, Miller explains how the premises of this new ethic can be found in *Utilitarianism* as well where Mill explains that modern equality provides the basis for extending and deepening the bonds of human sympathy.

What is missing from *The Cambridge Companion to Mill* is a comprehensive assessment of Mill's overall political philosophy. While Alan Ryan's essay situates Mill among key liberal thinkers, Ryan continues to eschew or revise those parts of Mill that conflict with his own outlook. In a previous work, Ryan, upon identifying a Platonic strain in Mill's thought, dismissed any further investigation of this point since "Mill's age had not learned to see Plato as a totalitarian" (*J. S. Mill*, 1974, p. 130).

In his essay offered in this volume, Ryan revises Mill's views on religion in the context of Mill's argument on human excellence in Chapter 3 of *On Liberty*. In that chapter, Mill bemoans the Calvinist inspired outlook that denies the "self-will" and associates the good solely with obedience. For an alternative idea of excellence one must reach back beyond Christianity and Plato to the Periclean age where "pagan self-assertion" was encouraged. But, Mill realizes that a society's praise for self-assertion can produce individuals who assert themselves in order to pursue the public good (Pericles) as well as individuals who put forward their will at the expense of others (Alcibiades). Consistent with the essays noted above, Mill concludes that there is a need to develop an idea of excellence that "blends" the creative qualities of Pericles with the moral ideas of the good expressed by Plato and Christianity. "There is a different type of excellence . . . a Greek ideal of self-development, which the Platonic and Christian ideal of self-government blends with, but does not supersede" (*On Liberty* in *Collected Works*, vol. 18, 1977, p. 266).

Ryan appears to miss Mill's point. Where Mill sees a dialectical relation between individual willfulness and an ethical or religious idea of the good, Ryan only sees the rejection of religion: "The target of Mill's assault was self-abnegation. He contrasted Christian self-abnegation with pagan self-assertion..... One can forgive Mill for following Machiavelli in praising pagan self-assertion to the detriment of the Christian ideal of self-abnegation. In gross at any rate that contrast holds up perfectly well" (pp. 517–8).

Ryan's failure to appreciate the influence of either Plato or religion on Mill's thinking reflects an approach to liberalism that inhibits the study of its foundational principles. Commentators in this tradition recognize that liberalism's highest goals are a radical break from ancient philosophy's and medieval faith's focus on the virtues required to live in conformity with a transcendent order. But this understanding of liberalism's new aims can be overemphasized to the point of overlooking how alternative currents of thought either contribute to or buttress the modern project. Given this interpretation, it is not surprising that in recent decades liberalism has become identified with indifference to substantive moral concerns.

As long as contemporary readers of Mill restrict their analyses to what they find as original and beyond the past, they will continue to misidentify him with a liberalism that is far more narrow and one-dimensional than Mill's own political philosophy. It also means many of the important insights contained in the volume under review will be overlooked and lost.

Ecofeminist Natures: Race, Gender, Feminist Theory and Political Action. By Noël Sturgeon. New York and London: Routledge, 1997. 260p. $69.95 cloth, $18.95 paper.

Cynthia Halpern, *Swarthmore College*

In his article "Love Thy Neighbour? No Thanks!" in *The Plague of Fantasies* (London: Verso, 1997, p. 45), Slavoj Zizek recapitulates Lacan's distinction between knaves and fools: The right-wing intellectual is a knave, "who refers to the mere existence of the given order as an argument for it," while the left-wing intellectual is a fool, "who by means of his lucid procedures destined to 'subvert' the existing order, actually serves as its supplement." In her book, *Ecofeminist Natures: Race, Gender, Feminist Theory and Political Action*, Noël Sturgeon skirts Lacans designation of left-wing intellectuals with a discomfiting closeness by analyzing the precarious divide between scholarly and activist debates. She alerts us to an element of silliness in arguments about name-calling and in-fighting among a very small number of people engaged in political actions in the context of the much larger necessities of global politics. She also reminds us of the tasks of left-wing intellectuals.

Ecofeminist Natures is about two contested terms of identity in contemporary feminist discourse—ecofeminism, and more improbably, essentialism. The author does very well in addressing the history and differentiating struggles out of which the various organizations and meanings of ecofeminism have evolved, especially in feminist and environmentalist politics. She does less well explaining why "essentialism" has become a term of disapprobation and a weapon of attack in these discourses, or in exploring systematically the meaning of the term in theory in contrast to the way it has been used in practice. That is the larger and more interesting question that is raised here.

What is at stake in these arguments over names? Sturgeon looks to address the unfairness of labelling as "essentialist" certain feminist variants, especially ecofeminism, in order to dismiss them from the ranks of legitimate feminisms, even though, she admits, they are in part, or in some versions, "essentialist." She argues and demonstrates, however, that the "essentialism" of ecofeminism has been strategically effective in mounting a political opposition against other sexist environmentalist adversaries at different times.

Sturgeon operates throughout with a pretty murky understanding of the problem of essentialism. Strategic essentialism derives from the problem of validating essentialist concepts, which originated in early modern chemistry from chemical essences, that which cannot be reduced further, that is, that which obviates scrutiny because it is already and purely itself. As Gayatri Spivak has so productively demon-

strated elsewhere, by using and contesting essentialist concepts, we promote scrutiny of something which effaces or disavows scrutiny. Philosophically, essentialist concepts can only take their meaning from their use in particular contexts; they function as essences only in context. Sturgeon fails to recognize the highly contextual nature of the nature/culture distinction, or the woman/nature distinction; she ignores the indispensable role of context in explicating or using any such terms, as affirmed by theorists from Levi-Strauss to Sherry Ortner. One of the most important conclusions here, both consciously argued in the book and unconsciously demonstrated by it, is the vast distance that lies between serious theory and the political practice of activists, even feminist theory and feminist practices.

Sturgeon recounts the story of ecofeminism in feminist environmentalism in order to set the record straight. In her first chapters, she gives us some fascinating recent history: the relations between ecofeminists and Murray Bookchin's social ecology, between ecofeminists and deep ecology, between ecofeminism and Earth First! She describes the origins of ecofeminism in the Antimilitarist Nonviolent Direct Action Movement and gives a very detailed picture of the women and issues at stake in the development of WomenEarth and the WomenEarth Feminist Peace Institute.

Sturgeon writes her best chapters on the nature of race, "indigenous women and white goddesses," and global environmental politics. She highlights the problem of racism throughout her book. She moves from a discussion of the peculiar white-nonwhite concept of racial parity in WomenEarth to a much more serious discussion of the abuse of Native American spirituality in New Age environmentalism and ecofeminist spirituality. She is clear about the racism of "borrowing" the spiritual practices of Native Americans and using them out of context. She is also clear about the abuse inherent in idealizing third world indigenous women as icons of racial and environmental purity in the international environmental and feminist movements.

Finally, to the major point in the book. Sturgeon argues that "in general, feminist theory is not used as much as I think it should be to illuminate radical activist movement practice" (p. 6), and "the most vital radical political theories develop in tandem with radical movement practice; one does not preceed [sic] the other" (p. 195). She knows that "doing feminist theory" is political, because it involves "teaching, promotion, and publishing practices that form, critique and resist unequal arrangements of power" (p. 7), but theory should be "articulated" to feminist practice. Sturgeon asserts the priority of practice over theory and the importance of activist politics, in contrast to "power-elite theorists" who run things, it seems, and have no respect for the hard-working, and suffering, activists in the trenches. This is seriously to misunderstand theory and the political nature of theory.

Political theory is not primarily, or even secondarily, about justifying or producing political practice, and the greatest theorists have not taken their cue from those on the front lines. They have designed those lines. Theory is its own justification and its point is knowledge, which is by its nature power, because through knowledge reality is structured. Theory, if taken seriously (not as typologizing), can help unravel some of the knots that bind the participants described in this book, but that is not its trade. Theory translated superficially into micropolitics, as "essentialism" is shown to have been, is no longer theoretical, but instrumental, and the difference is crucial.

The Fetishism of Modernities: Epochal Self-Consciousness in Contemporary Social and Political Thought. By Bernard Yack. Notre Dame, IN: University of Notre Dame Press, 1997. 182p. $28.00.

Stephen K. White, *Virginia Tech*

The polemical tone of this book is evident all the way from the title to the back of the dusk jacket, where the blurb reads: "Postmodernists of the world: beware . . . " Yack's targets are intellectuals responsible for the "outbreak" of a "contagion," a disease of the mind: the propensity to "fetishism." Borrowing from Marx, who criticized capitalist society for its treatment of commodities less as human creations than as independent entities with a life of their own, Yack contends that the current theoretical landscape is dominated by the fetishization of the idea of modernity; more specifically an "obsession" with "the idea of modernity as a coherent and integrated whole" (pp. 13, 29–30). Yack wants to round up the agents of disease and cleanse them (an epigraph from Ernest Gellner at the beginning of one chapter reads: "The sooner this nonsense stops, the better"). Who exactly needs to be rounded up? The list includes some "big names": postmoderns like Lyotard, certainly, but also the "Left Kantians," a pretty extensive group including Marx, Marcuse, Heidegger, Habermas, and others—although Yack admits that he will use them "only as illustrations of my argument rather than as the subject of detailed commentary" (pp. 15, 42–3, 48–9). When an author moves with such persistent polemical intent and yet admits some lack of attention to detail, perhaps not just postmoderns should beware.

The fetishists' tendency to portray modernity as an integrated whole is born of a consistent failure to distinguish between "temporal and substantive concepts of modernity" (p. 24). The former relate to a modern age; the latter to some set of ideas and institutions we might want to claim are distinctively modern. One could, for example, think of the modern age as beginning with a cataclysmic event like the French Revolution; but one should separate this temporal delineation from what one might wish to contend are specifically modern phenomena such as a certain notion of scientific method. When these concepts are not clearly separated, we continually stumble into the implausible claim "that everything after the modern divide is characterized in some important way by distinctly modern ideas and practices" (p. 29). This leads us to accord modern life more coherence than it really has. And when we do that, it is easy to begin inflating a discussion of specific changes in contemporary life into talk of the "end" or "collapse" or modernity, or of the onset of a new unity: postmodernity. (Since talk of such a new age is parasitic upon the illusion of a coherent modernity, Yack calls postmodernity "a mere figment of our modernity fetish" [p. 66].)

The real harm arising from fetishism is that its images of wholeness hide from view the fact that the modern age exhibits "a large degree of incoherence" resulting from a varied mix of modern ideas and institutions with ones that "either resist or are indifferent to the unprecedented dynamism" introduced by scientific-technological change and the demystification of traditional authority (p. 36). It is only after accepting this incoherence that interpretive justice will be done to important contemporary political phenomena like nationalism or religious fundamentalism. This point extends as well to the character of liberal democracy. Yack argues that we get a better understanding when it is comprehended not merely as the political face of a seamless modern whole, but rather as an "incongruous" combination of modern ideas like individual rights and social equality, on the one hand,

and constitutionalism and the rule of law, which are inextricably entangled with nonmodern notions of veneration of authority and the past, on the other. When these issues are occluded by fetishism, we miss "some of the most interesting tensions in contemporary political experience" (p. 89).

Despite his distaste for talk of postmodernity, Yack is careful to point out that he is actually "quite sympathetic" to postmodernism, at least in its endorsement of "skeptical, pluralist, perspectival attitudes" (pp. 64, 155). These remarks, as well his assertion that liberal democracy rests to a good degree on the affirmation of tradition, seem to position him reasonably close to someone like Richard Rorty.

Yack's ultimate goals are ones I would endorse to a substantial degree. Theoretically, we should strive for "a more cautious and self-critical approach to modernity" (p. 137). Politically, we need to enliven our sense that we live in an incongruous mix of modern, nonmodern, and postmodern ideas and institutions; and that the real questions have to do with how we "balance" all these (p. 40). These issues are immensely complex, and Yack is right that little good comes from immersing them in oversimplified frames of modernity or postmodernity.

Yack is at his best when he is following his own advice about caution and self-criticism. His key suggestion that we keep teasing apart temporal and substantive claims in discussions of things modern and postmodern is a very useful one. But his polemical bent mars this book. He might, of course, quickly reply in his defense that some of the targets of his attack have hardly been shy when it comes to the polemical excoriation of liberals like himself. There is no doubt a good bit of truth in such a defense. But Yack is counseling caution, common sense, and a wariness toward getting trapped by one's own generalizations. It is fetishism "to treat our own creations as if they have a life of their own" (p. 7). Yet, Yack may have let his own concept of the fetishism of modernity turn into a fetishization of his opponents, who are sometimes made to dance to a prescribed drum beat. I will point to only one instance of this problem. The leading example Yack employs to introduce the reader to a fetishized view of modernity is a quotation (p. 18ff) from William Connolly's, *Political Theory and Modernity* (1993 ed.). Even assuming that the alleged confusion between the temporal and substantive notions of modernity is in fact present in that text, one might have expected that Yack would go on and draw out systematically how this confusion produces substantial, harmful consequences in the author who has been singled out. But little is done in this regard. Had Yack been more concerned to look carefully at his opponent's work, as opposed to using it as an "illustration," he might have discovered discussions of phenomena like religious fundamentalism that in fact treat contemporary society as displaying just the kind of incongruous mix that Yack sees as characteristic. (See *The Ethos of Pluralization*, 1995.)

Intersecting Voices: Dilemmas of Gender, Political Philosophy, and Policy. By Iris Marion Young. Princeton, NJ: Princeton University Press, 1997. 195p. $49.50 cloth, $12.95 paper.

Nancy J. Hirschmann, *Cornell University*

This collection of previously published essays by a leading figure in feminist philosophy touches on some central controversies in feminist theory. Although I almost always find something to disagree with in Young's arguments, her essays are stimulating and thought-provoking, and the ideas that they introduce—gender as seriality, asymmetrical reciprocity, a rethinking of justice and the family, the need to replace deliberative with communicative democracy, the notion of home—are creative and invigorating to feminist philosophy.

Though Young could have done a better job in the introduction tying these articles together—indeed, she makes no effort to hide the fact that she thinks the essays are rather disparate, raising the question of why she wanted to have them collected—in fact there is more coherence here than Young herself recognizes. In particular, the theme of communication, particularly of listening, is one that threads through almost every essay in the volume. A key notion she is drawn to is that of respecting all members of a community, which requires not only an expansion of "community" to recognize the ways in which exclusion defines membership but also a deep and genuine listening that attends to others' subjectivity as well as to the institutional parameters that determine inequities of power, both formal and informal, overt and subtle. Central to this enterprise is the construction and meaning of identity, and the constitution of contexts within which meaning takes place.

These themes are particularly illustrated in "Asymmetrical Reciprocity: On Moral Respect, Wonder, and Enlarged Thought." Young problematizes a notion that has become important to much feminist theorizing, of being able to envision the self as other, because such moves often hide differences in power that foster appropriation and hamper listening. Citing a telephone poll in Oregon to which able-bodied citizens responded that they would rather be dead than disabled, Young points out that people with privilege do not really listen to the perspective of the less fortunate but rather superimpose their privileged position on the other's circumstances. Because they do so from their position of privilege, they generally misunderstand the other's perspective, thus reinforcing power inequalities. In this case, Young claims that this survey provided the justification for policies denying coverage of certain health-care benefits to the disabled, which in turn perpetuated the inability of the disabled to be heard. The solution is to strive for reciprocity—for exchange, interdependence, common effort—that begins from the recognition of asymmetry in social relations. Similarly, in "Communication and the Other: Beyond Deliberative Democracy," Young argues for a political practice of listening, suggesting that communication requires genuine respect and the willingness not only to acknowledge the limits of one's own understanding but also to be changed by what one hears. She opposes this vision to the conflictual character of deliberation where outcomes are measured in terms of wins and losses by entrenched positions. Listening is also a theme in "Punishment, Treatment, Empowerment: Three Approaches to Policy for Pregnant Addicts." Focusing on pregnant drug addicts, Young attempts to develop an approach to policy that does not infantilize women, that empowers them by recognizing their subjectivity and agency—by listening and attending to their experience—and that highlights the dangerous assumptions and coercions that state treatment programs often demonstrate, and yet seeks to protect unborn babies.

In "House and Home: Feminist Variations on a Theme"—the only essay in this collection not previously published—she again points out the ways that power systems of sex and class prevent some women's stories from being heard. Thus, Young tells her own mother's story of the struggle to support her family in an age of intense social control over unmarried women, a story with lessons for today's welfare surveillance. Though home can and has been an oppressive notion, one of exclusion, erasure of difference, and a withdrawal from politics, Young criticizes many feminist rejections of home as

often elitist refusals to listen and argues instead for a feminist political reconceptualization of home that incorporates notions of safety, preservation, privacy, and individuation.

Other essays give less overt prominence to listening and communication but incorporate them as subtextual and even methodological messages. In "Gender as Seriality," Young tackles a central controversy in feminist theory: how to establish the term "woman" as a theoretically useful category without replicating the modernist imperialist gesture of treating the experience of privileged groups as a universal category. The issue of "difference" among women has been one that is both empowering to feminism, in bringing to view the diversity of experiences that the people called "women" have, and yet at the same time crippling, by silencing any invocation of the term "women" as by definition universalizing and hence imperialist. Gender as a series, an idea Young borrows from Sartre, gets around this impasse; regardless of how "different" women in fact are, the fact that they are treated as if they are all the same makes women, in some sense, actually the same. This sameness may exist only at the surface level—a fact which neither contradicts deeper differences among women nor is meaningless politically or morally—but needs to be recognized if women are to be able to communicate their experiences and to listen to each other as well as to be heard by men.

In "Reflections on the Family in the Age of Murphy Brown: On Gender, Justice, and Sexuality," in many ways the best essay of this collection, Young addresses strategies for being listened *to*. Specifically, she takes on the issue of how family is defined, and in particular attacks the state privileging of the marriage relationship. In keeping with the work of others like Martha Fineman, Young questions gay and lesbian activists who advocate for the right to marry and instead suggests that the legal privileging of the marital relationship should be abolished; the efforts of disempowered groups such as gays to access the power of this privilege in order to gain the power of legitimacy, and thus be heard, does not equalize power, but instead unthinkingly perpetuates unjust privilege and inequality. It is thus politically, if not morally, self-defeating. She makes this claim in the interests of a feminist notion of justice; in critiquing the dominant distributive model of justice, Young finds conceptual links between that model and assumptions about the giveness of heterosexual marriage as the foundation for family. Uncoupling these involves a simultaneous redefinition of justice and an enlargement of the notion of "family" to allow for a greater variety of forms within the parameters of legal sanction.

In all these essays, Young puts her finger on the pulse of central controversies within feminist theory. The ideas of gender as a series and asymmetrical reciprocity in particular are intuitively appealing, provocative, and stimulating. This is not to deny that her essays display inconsistencies and other problems. For instance, in asymmetrical reciprocity, the question remains: Why should those with power listen to those without? Doesn't that presuppose a kind of symmetry, that is, an equality of power and right? And where does this equality come from if not power? Young does not address this circularity. Indeed, in "Communication and the Other" she does assert that equality is a necessary condition of making communicative democracy work, but she does not suggest how such equality is to be attained; or more precisely, she fails to acknowledge the vicious circularity of inequality, and how power begets power, that is recognized in some other essays. Furthermore, she glosses over the issue of disagreement; how much is allowable within community, how much room for dissent should there be, how much is consistent with meaningful and productive communication? Can you really listen to someone if you disagree deeply and inequality defines your relationship? But if listening is the key to equality, how can that be achieved?

Moreover, Young does not always give credit where it is due; for instance, her view on the problems of seeing the self as other is actually put forth by feminists of the "care perspective," of which she is critical. In *Women's Ways of Knowing* (1997), for instance, Clinchy et al. explicitly reject putting oneself in the other's place as individualistically self-referential and argue that adopting the other's perspective requires listening and empathy. It may be that such feminists are overly optimistic about the ability to hear and understand different others, but that is not Young's objection. Her critique of "deliberative democracy" also seems at times overdrawn; as Jane Mansbridge points out in her theory of "adversary democracy," deliberation can often be conflictual but is not necessarily so. Indeed, the deliberation envisioned by a number of democratic theorists such as Benjamin Barber does not resemble Young's picture. Similarly, her distinction between identity and series, although innovative and attractive, may reduce the complexity of identity somewhat artificially and even arbitrarily. Indeed, Nancy Chodorow, whose view of identity Young explicitly rejects, in fact defines identity in terms that already incorporate Young's definition of seriality. Identity has both active and passive components, involving ascription by others in a complex dynamic; but Younger asserts that identity is active self-identification and saves the more passive components for "series." While theoretically convenient, however, and seductive in terms of its ability to resolve a key controversy in feminist theory, I am not entirely convinced this is correct: For instance, the *identity* "black" originated in white Europeans' attribution of race-difference on which the institution of slavery was built. This created a conception of "difference" for Africans and their African-American descendants based on continued treatment by white race-prejudice. The active self-identification "black" can be a powerful act of resistance, but inevitably works interactively with how whites have treated and defined blacks, as well as how they do so today, as well as with how racism constructs and contrains both black and white actors. Thus, to call race a "series"—and the comparatively pedestrian example Sarte offers is people waiting for a bus—runs the risk of trivializing and weakening this historical construction of racial identity, as it may do with gender.

Though these criticisms are not minor, however, they do not really detract from the strength of the individual essays, which are solid illustrations of Young's creativity, originality, and strength as a theorist. Indeed, it is a unique quality of Young's work that the strength of the criticisms that can be lodged against her testify to the provocativeness of her theorizing.

American Politics

The Price of American Foreign Policy: Congress, the Executive, and International Affairs Funding. By William I. Bacchus. University Park, PA: Penn State University Press, 1997. 343p. $55.00 cloth, $18.95 paper.

Heidi H. Hobbs, *North Carolina State University*

The success of American foreign policy initiatives are dependent on the amount of money Congress appropriates to them. Bacchus explores the special relationship Congress and the executive enjoy in international affairs funding from an insider's point of view. Currently the executive director of the Quality Council of the U.S. Agency for International Development, Bacchus brings both academic credentials and more than twenty years of government experience to bear in his examination of this often neglected topic.

The central thesis of this volume is that many of the trials and tribulations of American foreign policy in the past quarter century can best be examined through the intricacies of the budgeting process. The fine balance that exists between the different branches of government are particularly exacerbated by the extent to which executive leadership is supported by congressional allocations of resources. Of particular concern is the attempt by Congress to become a "co-equal partner" with the executive in foreign affairs through the budget process (p. 39).

Bacchus examines this relationship by first delineating the exactitudes of the budgetary process—where funds appear and the percentages of the overall budget that they constitute. Unlike defense, the foreign affairs budget is fragmented both as it is proposed by the executive and then processed through the Congress. Policy initiatives as they come from the president and particularly, the Department of State, are more focused on "high policy" as opposed to the "low" concerns of the budget. The result is that initiatives and allocations are often mismatched, both in dollar amounts and timetables established for the operationalization.

The process is also subject to political whims and the current topics of the day. At several points in the book, Bacchus identifies areas where congressional intervention was driven by hot issue areas, political concerns, and domestic influences. For example, concern over security at the Moscow embassy when it was found that the Soviets had been "bugging" the building during construction in 1986 resulted in extensive funding for securing the building. Congress responded with allocations of more than $4 billion in the first four fiscal years (1986–90) following the discovery. Yet, it took ten years to decide upon a course of action that would effectively address the problem. Further delays and debates over the best way to respond to the issue have continued with the expected completion of the project estimated now for 1999, more than twenty years after the initial appropriation.

Several myths have grown up around this process that the author methodically dismisses. Most notably, he argues that interbranch differences are *not* now "worse than ever," despite partisan differences; foreign affairs funding *is* often bipartisan; monies *are* allocated even when the pay-off is not clear; iron triangles *are* alive and well and have even broadened in scope to reflect ethnic interests (Israel and Egypt, Greece and Turkey); and finally, funding *does not* always follow policy changes (for example, continued extensive funding of the Voice of America).

Efforts to change these processes have been limited. The congressional election results of 1994 were anticipated to bring major change to what at best could be characterized as an uneasy relationship. In fact, as Bacchus profiles the events of the subsequent year, efforts to downsize executive branch control of foreign affairs through "merger" were unsuccessful as the president vetoed the hard fought bill that was ultimately produced. The following year, however, the president himself introduced the concept of "merger" as a downsizing strategy.

Bacchus believes these institutional conflicts will continue, regardless of who is in power, and it behooves the government to develop effective procedures that will streamline and depoliticize the process. Toward that end, he makes several recommendations for both the executive and Congress. Specifically, he argues for an integrated executive branch foreign affairs budget that is more directly tied to policy initiatives, linking resources to policy. On the congressional side, Bacchus urges consolidation of the foreign affairs budget. This will streamline policy prioritization, conserve members' time and energy, and ultimately, promote better relationships between resource allocation and policy priorities. He also favors a two-year budget cycle toward this end.

Overall, this is a thorough review of the structure and functions of the budget process over the last 25 years as they are related to foreign affairs. The volume is rich in details of the three-fold process—budget, appropriations, and authorization. It explores the many efforts to streamline the process and does a very good job in presenting the intricacies of the executive-legislative relationship in foreign affairs funding. The writing is personable and organizationally refreshing, with examples and case studies successfully integrated throughout the volume. It will be of interest to congressional scholars and particularly illuminating to American foreign policy scholars, whose knowledge of the budgetary process may be limited.

Presidential Responsiveness and Public Policy-Making: The Public and the Policies that Presidents Choose. By Jeffrey E. Cohen. Ann Arbor: University of Michigan Press, 1997. 289p. $49.50.

Michael Margolis, *University of Cincinnati*

Those who would develop theories that relate public opinion to the policy leadership role of the American presidency face the daunting task of generalizing from a small number of observations. Starting with Teddy Roosevelt's modern "rhetorical presidency" (p. 240), only seventeen men have occupied the office of President of the United States, nine since 1953. That the American presidency is a unique institution and that each president has his own policy agenda, ideology, political skill, and style make the theorist's task even more difficult. Add the major political events that affect each presidential administration, and the number of potential independent variables begins to approach the number of observations on the dependent variable(s). Keenly aware of these difficulties Jeffrey Cohen nonetheless makes a fine—though inevitably flawed—effort to develop and test a theory of presidential responsiveness to (and leadership of) public opinion.

Americans have dual—and sometimes contradictory—expectations of their president. They want him to follow public opinion, but they also want him to provide policy leadership. The public urges the president to adopt its preferred policy alternatives; but it blames him if those alternatives fail. The president cannot escape the dilemma. As a public servant, he must respond to public demands. What can a president do?

Building upon the insights of Richard Neustadt (*Presidential Power: The Politics of Leadership*, 1960 and Murry Edelman, *The Symbolic Uses of Politics*, 1964), Cohen theorizes

that presidents will normally attempt to protect their popularity and professional reputation by responding symbolically rather than substantively to public demands. Symbolic responses are by no means devoid of substance, but they eschew specific commitments whenever possible. They leave presidents with room to work out details with advisers, party leaders, the cabinet, Congress, and other relevant actors and interests.

Cohen devotes most of his chapters to empirical tests of this theory. He considers four stages of policymaking: problem identification, substantive rhetoric, policy formulation, and policy legitimization. He uses State of the Union addresses from 1953 through 1989 to develop measures of problem identification and substantive rhetoric. He then employs these measures as both independent and dependent variables in regression models that relate them to problems and preferences reported in public opinion polls. He also compares presidential rhetorical liberalism to positions taken by the Americans for Democratic Action and to "public moods" as measured by Stimson (*Public Opinion in America: Moods, Cycles and Swings,* 1991) (policy formulation). Finally, he compares public mood with announced presidential positions on congressional roll calls (policy legitimization).

By and large the results affirm the theory. Presidential rhetoric is highly responsive to public opinion regarding problem identification. It is less responsive or not at all responsive regarding substantive rhetoric, policy formulation or legitimization. Cohen includes eight case studies that illustrate his results.

Whether or not to accept these results rests upon the validity of Cohen's measures. Does classifying the sentences of 33 State of the Union Addresses (and of four substitute speeches) comprise a valid operational procedure for measuring presidential responsiveness to public opinion? Is the latest Gallup poll preceding the presidential address a valid indicator of the public's policy priorities or its preferences? Cohen is the first to admit that "much policy making happens between Addresses" (p. 37), and in his case studies he makes little or no mention of them. He is also candid in admitting that he has so few cases and so many independent variables (often correlated with one another) that his multivariate equations include only the best-fitting subsets of independent variables. Statistically, however, if you try enough combinations of these variables, you are bound to find good fits here and there.

Regardless of whether they accept its conclusions, readers will find *Presidential Responsiveness* useful for its thorough summarization of previous theory regarding presidential responsiveness to public opinion and for the clear explanation of its methodology. The argument is well organized and presented straightforwardly. The author lays out his assumptions, explains his measurements, and uses regression paradigms appropriate to those measurements.

Although Cohen hurts his argument by reifying some measures of dubious validity, I am hard pressed to find a more thorough attempt to develop and test a theory of the responsiveness of the American presidency.

Muffled Echoes: Oliver North and the Politics of Public Opinion. By Amy Fried. New York: Columbia University Press, 1997. 308p. $49.50 cloth, $19.50 paper.

Barry J. Balleck, *Georgia Southern University*

This book raises a question that some have been wondering about for years—What is the true character of American public opinion? Today, American public opinion is most likely to be reported by the mass media as an amalgamation of the percentage of Americans who respond to some set of predetermined and close-ended questions. There is in such reports little, if any, of the spontaneity that once marked "true" public opinion. Indeed, gone are the days of mass demonstrations, wildcat strikes, and marches on state houses and the seats of government that once were the hallmarks of American public opinion. The information that is presented as public opinion is calculated and massaged, after which "experts" offer interpretations of what they think the public thinks. In the end this process makes the public more often than not the consumers rather than the producers of public opinion.

Amy Fried contends that the reciprocal discourse between the public and elected officials has today been replaced by a mass of survey technologies and techniques which mask the real meaning behind public opinion. In effect public opinion has become a one-way process, one in which various elements of the political system—such as the media, interest groups, and political elites—manipulate and craft public opinion to serve particular political ends. This state, she contends, means that "it is less important politically what the public thinks than what the media and political elites believe the public thinks" (p. 12).

To guide her research on the current state of American public opinion, Fried draws on theories of V. O. Key and Jürgen Habermas. Key argued that public opinion exists within a "marketplace of ideas" wherein opinions are continually in competition with one another in a dynamic political environment. Public opinion, then, is but an echo in an "echo chamber," creating a situation in which there are many conflicting meanings and ideas which must be interpreted and given meaning by the existing political system. These interpretations and meanings are made possible within the context of Habermas's thought. Habermas held that expressions of public opinion are largely the product of mass culture and the ongoing historical process of a given society.

Evidence that American public opinion is but an echo in an echo chamber can be found in the case of the Iran-Contra hearings, particularly in the appearance before the joint House-Senate Investigating Committee of Lieutenant Colonel Oliver North. Common belief holds that the American public saw Oliver North as a hero in the Iran-Contra scandal—one who had become the scapegoat for the Reagan administration's policy of trading arms for hostages, and then funneling proceeds to the Contra "freedom fighters" in contravention of the Boland Amendment prohibiting such aid. North's designation as a "hero" was suggested first by President Reagan's designation of him as such, as well as the spectacle of a defiant North standing before a Congress intent on establishing his guilt. North himself was successful in perpetuating the hero myth as he was able to draw on a variety of cultural and symbolic themes shared by the collective American conscience.

Apart from Reagan's pronouncement and North's self-promotion, Fried contends that the portrayal of North as an heroic figure was largely the creation of (1) the media, which misinterpreted or disregarded public sentiments which questioned North's motivations, (2) zealous interest groups, which saw in North the opportunity to advance their political agendas, and (3) political elites, who used the spectacle of North to criticize existing policies prohibiting Contra aid. Using a variety of results from personally conducted interviews and interpretations of media reports of North's appearance before Congress, Fried concludes that, contrary to popular belief, there was not a large groundswell of public support for Oliver North and his actions. Rather, the public

was somewhat indifferent toward North, although there were expressions of admiration for him coupled with expressions of disdain for what was seen as his blatant disregard for the constitutional process. In effect, the phenomenon of Oliver North was one which was carefully orchestrated by elements of the American political system, including the mass media, vocal interest groups, and political elites—each advancing its own agenda. This orchestration was, as Murray Edelman contends, a "spectacle of public opinion."

But how is it that these political elements could construct such opinion? Fried argues, at least in part, that the muffled opinions expressed in the echo chamber of politics left little clear guidance as to what the public's opinions were toward Oliver North. In fact, that North was quickly made into an heroic figure in the Iran-Contra scandal suggests that those elements that wished to portray him as such were better organized than those that may have had an opposing view. There were never any serious public demonstrations against North, nor was there serious questioning of his motivation in the press. Some criticism did come from members of Congress, but they carefully crafted their dissent so as not to upset what was being portrayed as a growing tide of sympathy and admiration for Oliver North and his actions. As Fried points out, much of what happened to create the spectacle of Oliver North was the result of a sort of self-censoring on the part of those in the public who may have mobilized to express their opposition to North. Due to the fact that the media and other elements were lauding North, however, these portions of the public became reticent to express themselves because of their perception that their views were in the minority. This analysis seems to be correct as Fried's research shows that there was never the overwhelming support for Oliver North that was so often reported in the media.

The book is important in that it confirms research that has been conducted for years which shows that people with intense preferences are more likely to be active in the political process. Its unique contribution, however, is in asking the question which is at the heart of such research—What are the implications for the democratic process of the crafting of American public opinion? Fried contends "that the use of public opinion today poses serious problems for democratic citizenship, leadership, and the political process" (pp. 216–7). Though not "all powerful," the "use of public opinion" does provide a tool by which citizens, political leaders, and the political system itself can be manipulated (p. 217). If correct, Fried raises serious questions about the functioning of democracy. Indeed, the implication is that an aggressive media and determined interest groups can manipulate the public and even elected officials to such an extent so as to influence a variety of outcomes. Such is the conclusion to be drawn from the Oliver North case.

Notwithstanding these possibilities, there may be alternative explanations to the conclusions offered in the book. First, Oliver North may have been portrayed as a hero because of the perceived lack on the part of the media and other political elements of such heroes in the American political realm. In the aftermath of several foreign policy failures, it may have been refreshing to the public to see an American taking a stand against a relentless enemy—in this case Sandinista communism. More likely, though, is the possibility that cases like that of Oliver North are simply ones where the public does not choose to involve itself. In other words, it could be argued that the stakes simply were not high enough for the opposition public to become involved in the Oliver North case. Fried seems to allude to this point in her intriguing typology of the politics of public opinion found on page 199. Here, Fried seems to imply that issues which involve strong symbolic issues (such as abortion) will mobilize an even group distribution and there will be legitimate debate and opinion in the public sphere. However, other issues will not elicit such debate and will be susceptible to influences by media-driven, interest group-driven, or elite-driven dynamics. Though it is easy to agree with Fried on the surface that Americans are uninvolved and susceptible to manipulation, it is also possible to argue that the public must pick its battles and will therefore not concern itself with what it views as peripheral issues. One need only look to the public opinion generated prior to the Gulf War to realize that the public is alert and active in political events. Still, it is impossible to deny that there exists what Noelle-Neumann calls the "spiral of silence," wherein the public has become increasingly mute on a variety of matters. One can only conclude, then, that the multitude of issues in the public arena and the limited time available to the public makes it necessary to prioritize where each battle will be fought.

The Dual Agenda: Race and Social Welfare Policies of the Civil Rights Organizations. By Dona Cooper Hamilton and Charles V. Hamilton. New York: Columbia University Press, 1997. 335p. $24.95 cloth, $17.50 paper.

Todd C. Shaw, *University of Illinois, Urbana-Champaign*

From a recurring debate within African-American politics, an uneasy consensus has emerged: Even though the civil rights movement must be credited for breaking down the most overt barriers of racial discrimination, it has not ameliorated stark economic disparities. Two of the most insightful commentators on race and class in America—William J. Wilson and Harold Cruse—have forcefully endorsed this consensus, albeit from very different standpoints. Both in *The Declining Significance of Race* (1980) and *The Truly Disadvantaged* (1987), Wilson concedes that the civil rights movement has qualitatively improved the life chances of the black middle class. It has, however, left unscathed the economic morass that entraps the so-called black underclass. The problem, according to Wilson, is that civil rights leaders have been so preoccupied with "race-based" social policies, such as the "black-targeted" War on Poverty program or affirmative action, that they have fundamentally misread the political climate's greater receptivity to universal programs. Thus, in not pursuing programs like universal health insurance that benefit *all* Americans, they have missed key opportunities to build broad, winning coalitions with the middle class. In *Plural but Equal* (1987), Cruse offers an even more sanguine assessment of the civil rights leadership's myopic focus upon a "noneconomic liberalism." Most, except Jesse Jackson, have embraced a liberal, Democratic, welfare state without likewise recognizing the imperatives of economic self-development.

With the expressed purpose of challenging this consensus, Dona Cooper Hamilton and Charles V. Hamilton assert that it is both unfair and erroneous to argue that the major civil rights organizations have not consistently stressed a "dual agenda"—civil rights *and* economic redistribution through universal social welfare policies. As a matter of fact, the authors provide exhaustive evidence that for sixty years groups such as the National Association for the Advancement of Colored People (NAACP), the National Urban League (NUL), the Southern Christian Leadership Conference (SCLC), and myriad others have been at the forefront of working for black citizenship rights and economic justice.

At issue for these organizations, as the authors explain, has always been the unequal and precarious two-tiered system of

social welfare benefits first enacted in the 1935 Social Security Act. The first and largest tier entailed an extremely popular and politically untouchable social insurance program that covered the gainfully employed. The second tier, however, was comprised of public welfare recipients whose benefits have been susceptible to retrenchment because they are stigmatized as lazy and unwilling to adhere to the work ethic. To highlight black counterproposals, Hamilton and Hamilton weave a narrative that explains why civil rights organizations pressed for (1) universal coverage that grants benefits to the employed and unemployed alike; (2) full employment; and (3) federal, as opposed to state, funding and administration. Though this work more specifically replies to Wilson than to Cruse, the authors delineate how the social policy agendas of the major civil rights organizations have gone unrecognized by their critics who are concerned about the economic plight of African Americans.

Hamilton and Hamilton must be applauded for writing a fascinating, well-conceived, painstakingly researched, and wholly readable account of the civil rights contribution to American social welfare policy from the New Deal to the New Democrats. It is evident that this book was a labor of many years during which they meticulously compiled and rigorously employed primary resources from several libraries and archives. To be sure, the "consensus" that civil rights groups are only concerned about racial matters made such rigor necessary. For the most part, I believe they have anticipated their critics by reminding some and informing many others that—from the social security, full employment, and health insurance proposals of the 1930s and 1940s to the workfare of the 1980s—civil rights organizations have consistently warned the nation of the dire consequences of not guaranteeing economic opportunities for all. As is painfully obvious from the punitive 1996 Personal Responsibility and Work Opportunity Reconciliation Act, the civil rights groups have won only a few economic battles, but they have (to invoke the biblical maxim) fought the good fight.

Having said all of the above, I believe there are points where Hamilton and Hamilton could have more effectively anticipated implicit criticisms from Wilson and Cruse. Wilson may contend that despite the embrace of a dual agenda and universalistic language, these organizations were (and are) first and foremost concerned with the plight of "the Race," and thus it is undeniable that their racial objectives shaped their social welfare stances. To respond to such a criticism, the authors should have considered whether there has been a periodicity to the dual agenda. From the 1930s to early 1960s, a virtually flat, black class distribution may have compelled these groups to see race as nearly indistinguishable from class—thus their attempt to get black agricultural and domestic workers enrolled in tier 1 when they were originally excluded from social security. Yet, from the mid-1960s to the current period, increasing black class stratification compelled these groups to pursue more aggressively race and class imperatives (e.g., affirmative action alongside the 1966 Freedom Budget). Various students of black class politics—from E. Franklin Frazier in the 1950s to Robin Kelley in the 1990s—have found that black class tensions within civil rights groups have been persistent if not predominant.

To better anticipate possible reproach from a Crusian perspective, the authors should have made their introductory chapter more than a brief rehearsal of their main points. It also should have been a discussion of where we might position the civil rights organizations in relation to various black nationalist or black socialist programs and ideals. The Hamiltons' failure to mention or discuss the disagreement W. E. B. DuBois had with the integrationist direction of the NAACP in the 1930s was puzzling. DuBois explicitly advocated black, economic collectives or "pockets of socialism" based upon the immediate reality of Jim Crow segregation. And though the authors get at some of the internecine conflict between these civil rights groups and the communist-inspired National Negro Congress or Malcolm X and the Black Power movement, a wider examination of those within the black community who might have been critical of the civil rights groups would have demonstrated that civil rights leaders did not formulate the dual agenda in a relative vacuum; rather they were motivated by both opportunities in the policy arena and various pressures from the African-American community.

None of these comments, however, detract from my conclusion that this is an outstanding piece of scholarship that is required reading for students of the civil rights movement, American social welfare policy, and interest groups. Given the task before them, I can fully understand why Hamilton and Hamilton closely followed the social welfare consensus among civil rights organizations at the risk of excluding dissenters. I simply contend that their already rich narrative would have been richer if they could have made their "big picture" just a bit bigger.

Congressional Caucuses in National Policy Making. By Susan Webb Hammond. Baltimore, MD: Johns Hopkins University Press, 1998. 257p. $39.95.

Burdett A. Loomis, *University of Kansas*

Over the past twenty-five years, informal groupings of representatives and senators, usually labeled caucuses, have come into existence with increasing frequency. Congressional scholars, while acknowledging their presence, have had some difficulty in coming to terms with these groups. Neither fish nor fowl, they did not fall under the domain of either the party organization or the committee system. Although they drew on the resources of individual legislators, they served the interests of a collection of lawmakers. And there was the continuing question of significance: How important could the Mushroom Caucus or the Auto Task Force be, anyway?

After two decades of tracking these work groups, Susan Webb Hammond, professor of political science at The American University, has written the essential book on congressional caucuses. Drawing on a series of data points, surveys, and extensive interviewing in the 1980s and 1990s, she has produced a definitive study, one that will serve as the baseline for any subsequent research on caucuses. Hammond places these groupings within the formal framework of legislative analysis, in terms of party strength, committee jurisdictions, and member goals, but more importantly she combines careful description of these groupings with systematic analyses of how different types of caucuses operate within the Congress to affect policymaking. Indeed, the systematic nature of her approach becomes both a major strength and a minor weakness of this volume.

Congressional Caucuses proceeds in the most regular of ways, first establishing the roots of caucus formation and membership, then developing six major categories of caucuses, and subsequently focusing on how caucuses relate to party leaders, committees, the executive, and floor action in both House and Senate. Given the ground to be covered, Hammond is never able to work up much of a coherent overall narrative, a shortcoming that derives directly from the great diversity among caucuses. There are a lot of good stories, but rarely will one capture the essence of all caucuses, or even those of a single type.

Hammond's analysis rests squarely on her classification scheme for caucuses. Based largely on extended observations of the motivations driving caucus membership, her six categories include caucuses based on party (e.g., Democratic Study Group), personal interest (e.g., Congressional Space Caucus), national constituency (e.g., Congressional Black Caucus), region (e.g., New England Caucus), state/district (e.g., Rural Caucus), and industry (e.g., Congressional Steel Caucus). Once establishing this typology, Hammond uses it to demonstrate differences among caucuses in how they become involved in national policymaking.

The most notable strength of caucuses may well be their capacity to adapt. Thus, they can work with party leaders, address issues that fall between the cracks of the committee system, and serve the particular interests of their individual members. Hammond notes that the members see "the caucus system as supplemental to, but not supplanting, the formal party and committee systems" (p. 108). And such supplementing enhances the strength of rank-and-file members. Indeed, the steady growth of caucuses from the 1970s through the 1990s demonstrates the continuing desires of virtually all members to play a role (or be seen playing one) in the policymaking process. Even in a democratized Congress, the ability of most individual members to affect policies is limited by the party and committee systems that dominate the legislative process. Caucuses offer a real, if limited, alternative, in which a legislator may have an effect dedicating some time and staff resources to collective activities. Caucuses offer not an alternative to either party or committee organization but an alternative way to affect party and committee actions—or, in some cases, the actions of the executive branch.

In the end, caucuses survive because they serve their members and they can adapt to changing circumstances on Capitol Hill. This is most aptly demonstrated in the aftermath of the 1994 elections, at the onset of Republican control of Congress. Many GOP party and committee leaders, such as House Agriculture Committee chair Pat Roberts (R-KS), distrusted caucuses and succeeded in abolishing the officially sanctioned groups known as legislative service organizations. Almost all of these groups restructured themselves and continued to exist, and in 1995–96 forty-three new caucuses were formed. Both individual caucuses and the caucus form have proven resilient.

What, then, are we to make of caucuses? However much they may look like interest groups that exist at the heart of the legislative process, Hammond argues that such an interpretation is misleading. Rather, "when members join caucuses to influence policy outcomes, they do so as representatives of citizens, not as individual citizens" (p. 224). But does such representation differ from that of a lobbyist hired by the AFL-CIO or the AARP? House members and senators represent the steel industry, the auto industry, agricultural producers, and a host of other interests, and this representation takes place at all stages (as Hammond demonstrates) of the legislative process. Interests often seek a legislator to "champion" their cause on Capitol Hill; what could be better than having an entire caucus, with ties to party leaders and various committees, working on their behalf?

By not viewing caucuses as organized interests, Hammond can interpret their effect, on balance, as fostering integration within a fragmented legislative system. Surely they do help make the entire process more coherent. But they also represent particular interests that desire specific policy outcomes. Regional caucuses want federal funding formulas written to benefit their areas; industry caucuses have clear policy goals that often put them at odds with other interests; national constituency caucus preferences, such as those of the Congressional Black Caucus, often diverge sharply from the preferences of most House members.

Still, this is an extremely valuable book that both extends and organizes our understanding of these groups. Caucuses are here to stay, as important parts of congressional life with important consequences for national policymaking. Their relationships to the interests they represent, however, and the implications of this representation, remain open to further interpretation.

Modern Manors: Welfare Capitalism since the New Deal. By Sanford M. Jacoby. Princeton, NJ: Princeton University Press, 1997. 345p. $35.00.

Charles Noble, *California State University, Long Beach*

As Sanford M. Jacoby's important and timely book about welfare capitalism reminds us, there are many different ways to organize the search for economic security in modern capitalist societies. Only one corresponds neatly to what we think of as the "welfare state." At one time, both liberal modernization theorists and neo-Marxists believed that all industrial democracies were being driven by the same economic, technological, demographic, and political forces to adopt the same full-employment cum social insurance cum public assistance model. To be sure, there were quite contentious debates about whether this model was emancipatory or cooptive, but few disputed the idea that democratic capitalism was taking us all down the same road: toward government responsibility for the economic well-being of workers.

Clearly, the accepted wisdom was wrong. Other options have always been on the table, including private retirement, accident, and health insurance; worker-sponsored mutual-benefit societies; and employer-provided, private-benefit programs. The welfare state has turned out to be less stable than once thought, developing through crisis rather than through steady accretion. Western societies have also followed different trajectories. Because worker organizations and the state are relatively weak in the United States, and private corporations are relatively large and powerful, America has proven exceptional, lagging in the creation of universal, public-benefit programs while leading in the proliferation of private ones, in no small part because large corporations have wanted to keep unions out and government small.

Welfare capitalism has not previously gone unexamined, of course. Indeed, it would have been hard not to notice such early welfare capitalist pioneers as National Cash Register, S. C. Johnson, and Filene's, which developed innovative fringe-benefit programs that anticipated much of what government would legislate and unions would bargain over. Profit sharing, personnel administration, human relations—all were developed by companies seeking to make themselves rather than organized labor or the state the center of the worker's quest for economic security. But earlier students of the subject (e.g., Stuart D. Brandes, *American Welfare Capitalism, 1880–1994*, 1970, and David Brody, *Workers in Industrial America*, 1980) focused on the origins of the welfare capitalist movement in the late-nineteenth and early-twentieth century and its apparent demise in the 1930s. In *Modern Manors*, Jacoby, a business historian, breaks new ground, arguing that the old-style, paternalistic, and often heavy-handed welfare capitalism that died in the New Deal was replaced between the 1930s and 1960s by a new-style, corporate paternalism that emphasized persuasion and consent

rather than coercion and control. Moreover, he shows that these private efforts to attach workers to firms laid the foundations for current innovations in social policy, including today's "worker-friendly" efforts by corporations such as AT&T and Microsoft to encourage employees to look to them, rather than the government or the labor movement, for their identity and sense of security.

The empirical heart of the book is a close examination, based on a wealth of primary sources, of the evolution of labor relations and wage and benefit policies in three vanguard welfare capitalist companies: Kodak, Sears Roebuck, and Thompson Products (renamed TRW in the 1960s). Jacoby selected these three because they illustrate the shared corporate commitment to prevent unionization and secure worker loyalty as well as the range of things that firms have done to achieve these ends.

Each story is carefully documented and well told. Kodak combined generous fringe benefits, including company housing and health insurance programs, with production planning to minimize layoffs; and it sustained these efforts through the Great Depression and into the 1960s. Sears Roebuck helped invent modern personnel management to keep its sales force loyal, including the study and influence of worker attitudes and the deployment of aggressive antiunion strategies at the edge (and often beyond) of the law. Thompson Products created company unions, pressured workers, and developed elaborate human relations programs to keep organized labor at bay, bringing it into direct conflict with the National Labor Relations Board in the late 1930s and 1940s. Nevertheless, all three companies ended up in about the same place: without real unions and with elaborate personnel and benefit programs designed to keep it that way.

There is much to recommend here, both for students of business and labor and for political scientists interested in the welfare state generally. The account of why some companies became welfare capitalist leaders and others did not is handled well. As it turned out, larger, capital-intensive, highly profitable firms that managed to preserve the antiunion mentality of their founders were most likely to be in the vanguard. Jacoby also offers a wealth of evidence showing how public policy, including the enforcement (or lack thereof) of the Wagner Act and the development of supportive tax and public benefit policies, affected what corporations did for, with, and against their workers.

Modern Manors is not without problems. These three cases are exceptions to the rule, as Jacoby readily admits. More commonly, firms either failed to develop significant welfare capitalist programs or dropped them when profits, or the threat of unionization, receded. How significant, then, are the experiences of these vanguard firms? Also, Jacoby does not always make the most of his evidence. In recent years, social scientists have hotly debated the relationship between class and state in democratic capitalist societies, including the role of business and labor in the development of welfare states. Unfortunately, Jacoby does not weigh in to the debate, despite the obvious relevance of what he has uncovered. Finally, while Jacoby's distinction between old and new welfare capitalism is useful, it must be handled with care. We are well into the era of persuasion, but welfare capitalist firms still routinely punish workers who prefer unions, fire organizers, and flout labor laws. Apparently, employers have expanded their repertoire rather than replaced one method with another.

Nonetheless, having clearly laid out the problem and carefully narrated these three case histories, Jacoby provides students of the state, interest group politics, and public policy with much to mull over. And his conclusion, that the foundations of the next round of social policy innovation are being laid not by welfare reformers but by corporate personnel managers and human relations experts, cannot be ignored by anyone who wants to understand where the American welfare state may be headed.

The War for America's Natural Resources. By William R. Nester. New York: St. Martin's, 1997. 256p. $59.95 cloth, $18.95 paper.

Evan J. Ringquist, *Florida State University*

In *The War for America's Natural Resources*, William Nester attempts to explain the dynamics of policymaking in the area of natural resources over the past 150 years. The substance of the book is contained in four chapters dealing with consumptive uses (i.e., farming, grazing, and ranching), extractive uses (e.g., mining and energy production), preservation and conservation (e.g., parks, forests, and wilderness areas), and the development of water resources, particularly in the West. To guide our way through the morass of natural resources policy, the author outlines three philosophical perspectives regarding the use of natural resources. Cornucopians believe that the earth's resources are endless and should be exploited to meet society's material needs. For cornucopians, nature is an obstacle to be overcome rather than a treasure to be preserved. Environmentalists (who include everyone from nineteenth-century transcendentalists like Thoreau to modern deep ecologists) express a more ecocentric view where nature is valued both instrumentally and intrinsically. Finally, conservationists lie somewhere between cornucopians and environmentalists. Like cornucopians, they are materialists who see the natural world as a resource to be exploited, yet this exploitation should be controlled to conserve the resource for future generations. For Nester, natural resource policy can be understood as the outcome of competition between advocates of these three perspectives.

Adequately addressing the panoply of natural resource policies and agencies is an heroic undertaking. Truth be told, however, this book experiences more failures than successes at this task. Some of the most serious problems stem from the theoretical content of the book. The discussion of the three philosophical perspectives outlined above is both informative and concise, but Nester does not use them to develop any criteria for separating good or acceptable policies from unacceptable ones. At the macro level, this means that Nester's critique of natural resources policy is without a yardstick. At the operational level, it means that the author can critique markets as the source of ecological destruction in chapter one, then call for the increased use of markets to solve the environmental problems of agriculture in chapter two (similarly, the author calls for increased use of hydroelectric power in the energy chapter, then decries the ecological consequences of large dams in the water chapter). It is possible, even likely, that tools (markets, dams, etc.) may be a problem in one area and a solution in another, but without some theoretical guidance we have no idea which is true where. Second, the author's discussion of iron triangles (the overriding theory explaining the majority of natural resources policy) is at least 25 years out of date. From Nester's conclusion, it appears as if theory development in the area of public policy ended with the publication of Leiper's *The Political Process* in 1965, and that the challenges posed to this perspective by Hugh Heclo, Paul Sabatier, Frank Baumgartner, Bryan Jones, and others never happened. A similar case could be made for Nester's use of capture theory as an explanation for policymaking in the area

of natural resources. First, most natural resources agencies (e.g., the departments of Agriculture and Interior) were always intended to be constituent agencies, so "capture" in the regulatory sense is a non sequitur. Second, the few true regulatory agencies in this policy area (e.g., the Environmental Protection Agency) are social regulatory agencies to which capture theory rarely applies.

The book also suffers from substantial deficiencies of content. While no book can cover all bases, and while substantive content is to some degree a matter of personal choice, I find it curious that the author neglects to include water markets when discussing water policy; neglects the nuclear fuel cycle, repeal of the Price-Anderson Act, and the decision of the NRC to double the length of power plant operating licenses when discussing nuclear energy; neglects below-cost timber sales when discussing the Forest Service; and spends more time talking about Reagan's strong dollar policy than the CCC or the FHA when discussing agricultural policy. More disturbing is the almost complete absence of references to recent (i.e., last fifteen years) scholarly work in the area of natural resources.

State Devolution in America: Implications for a Diverse Society. Edited by Lynn A. Staeheli, Janet E. Kodras, and Colin Flint. Thousand Oaks, CA: Sage, 1997. 286p. $58.00 cloth, $24.95 paper.

Sarah F. Liebschutz, *State University of New York, Brockport, and University of Rochester*

The 1994 congressional elections, resulting in Republican majorities in both houses of Congress for the first time in 42 years, were widely viewed at the time as the response to, as well as the harbinger of, sea changes in American social and political life. The "Contract with America," the legislative agenda of the Newt Gingrich–led House of Representatives, in particular, was hailed (or lamented) for its potential to affect American federalism to its core. The elections, which "elevated discussions of state restructuring from a bureaucratic issue—as in 'reinventing government'—to political philosophy" (p. xii), were the impetus for *State Devolution in America*. The book's purpose is to examine the sources, processes, and implications of the 1994 congressional elections from a geographic perspective. The book's 13 contributors—all social geographers—share the assumption that economic competitiveness and social well-being are tightly linked to geography. The geographers' perspective is spatial. It extends well beyond static and bounded space, however, viewing places, instead, as open and mutable and highly vulnerable to political decisions in Washington and to changes in the global economy. Local and regional labor markets, conditioned by long-standing institutional and economic patterns, experience transnational flows of capital and labor in different ways. The authors all make clear their anxiety about further political, economic, and social fragmentation as a consequence of the 1994 congressional elections.

The book considers separately the why, how, and what of those elections: (1) recent social and economic trends and their exploitation for partisan purposes; (2) general approaches to restructuring the scope and scale of government; and (3) policy changes initiated by the Republican congressional majority. On the whole, *State Devolution in America* focuses cleanly on these three topics within a self-conscious spatial construct.

The quality of chapters elucidating the sources of state change (Part I) is uniformly high. Robert Lake's explanation of the conjunction of disaffection among large and small capital, and among blue- and white-collar workers in the early 1990s, and its exploitation for political gain by Republicans, sets the broad context. Data presented by Janet Kodras on poverty by place, ethnicity, family composition, and age are not new to policy analysts, but she integrates them well with an argument about the political implications of globalization. Lynn Staeheli's chapter on the linkages between social and economic change and the search for community—set against the stage-setting points by Lake and Kodras—is especially creative.

"Transforming the Political Opportunity Structure" (Part II) is less well focused on its topic. Kodras delineates three restructuring strategies—devolution, privatization, and dismantling—and succinctly summarizes key issues that, over time, have characterized the iterations of American federalism. Julian Wolpert's brief summary of tensions between public and nonprofit sectors exacerbated by federal reductions is a logical extension of points made by Kodras. Don Mitchell's discourse on the importance of the advocacy of rights of "ordinary people instead of the putative rights of capital" (p. 135), however, is basically normative and not well linked to the book's main themes.

The book's third section deals with the implications of the 1994 Republican revolution for agriculture, labor, welfare, education, and environmental policy. The authors of these chapters focus on policy formulation associated with each policy area, presenting background factors that explain legislative action as well as inaction. Brian Page, in a chapter on the Federal Agricultural Improvement and Reform (FAIR) Act of 1996, examines the trade-offs by farm size, region, and commodity, and explains why small and medium-size farmers supported FAIR despite the fact that they stand to be losers. Andrew Herod, who analyzes labor relations, and Meghan Cope, who analyzes welfare reform, use the devolution/privatization/dismantling typology developed by Kodras. It is obvious that neither Herod nor Cope approve of the Republican approaches in these respective policy areas. Herod's analysis of labor relations, however, is more objective and penetrating, particularly with respect to the paradox of attaining devolution and privatization through increased federal government intervention. Though Cope acknowledges the early stage of implementation at which she wrote, her ideology overwhelms discussion of the causes and anticipated consequences of welfare reform. She fails to acknowledge the widely held belief, even among the poor, that work is preferable to welfare, or the interesting potential "second-order devolution" institutional effects of welfare reform (see Richard P. Nathan, "The Newest Federalism for Welfare," *Rockefeller Institute Bulletin*, 1998). The best demonstration of the geographic perspective is Richard Wright's chapter, in which he discusses the context that forces the national government to respond to contradictory demands from nationalists for protectionism and from transnationalists for free flow of capital, goods, information, and labor across national borders.

The geographic perspectives of *State Devolution in America* complements but diverges from the political science perspective. The general purpose of these social geographers is to provide macrocosmic context to the events of 1994 through examination of the "shifting relationship between three principal spheres of American life: *the state, capital, and civil society*" (p. xxi). This approach complements the most finely honed and explicit focus of political science on political institutions, actors, and strategies. Political scientists also stress broad trends, such as increasing public distrust of government, particularly Congress (see, for example, Gary A. Jacobson, "The 1994 House Elections in Perspective," in

Midterm: The Elections of 1994 in Context, ed., Philip A. Klinker, 1996), and the rise of the Christian right (see, for example, John C. Green, "The Christian Right and the 1994 Elections: A View from the States," *PS: Political Science & Politics* 28(March 1995):5–7). But they move from these broader backdrops to emphasize the centrality of political leadership in exploiting for partisan advantage perceptions of the inability of government to deliver physical and economic security, or to reverse moral and cultural decline. The singular role of Newt Gingrich in fashioning a national strategy to demonstrate to voters "that Democrats were no longer fit to run the House and that Republicans had the ideas and the determination to take command" has been especially emphasized (see, for example, Richard E. Cohen, "Campaigning for Congress: The Echo of 1994," in *Toward the Millennium: The Elections of 1996*, ed., Larry J. Sabato, 1997, p. 167). Other factors—Democratic retirements from Republican-leaning districts and President Clinton's unpopularity—are also demonstrated by political scientists to have been critical in the 1994 congressional election outcome (Philip A. Klinker, ed., *Midterm: The Elections of 1994 in Context*, 1996).

In sum, the book's overall value—its normatively argued chapters aside—is as supplementary perspective on the congressional elections of 1994. It is not a substitute for the explicit focus of political science on the interplay among political institutions, actors, and strategies.

Presidents as Candidates: Inside the White House for the Presidential Campaign. By Kathryn Dunn Tenpas. New York: Garland, 1997. 191p. $49.00.

Ken Goldstein, *Arizona State University*

Presidential elections may well be the most studied topic in American political science. Still, although journalists have chronicled presidential campaigns from the inside (Theodore White's *The Making of the President* series still stands out as the best of this genre), there have been few scholarly accounts of how presidential campaigns are conducted. Similarly, while much attention has been paid to the different roles that a president plays, there has been little scholarship on how incumbent presidents organize and manage their reelection campaigns.

In this study, Kathryn Dunn Tenpas addresses this question. More specifically, in studying "presidents as candidates," Tenpas sets out to learn how incumbent presidents prepare for their reelection battles; how they manage their dual roles as candidate and president; and whether reelection campaigns distract presidents and the presidency from the business of governing. Tenpas focuses her attention on the eight most recent presidents to have run for reelection, characterizing their campaigns as either "victorious" (Eisenhower, Nixon, Reagan, and Clinton); "defeated" (Carter and Bush); or "takeover" (Johnson and Ford).

In the first chapter, she describes the evolution of presidential campaigns from party-centered operations to White House–centered operations. In the succeeding chapters, she describes the composition of reelection teams, the campaign strategies used by the various presidents, the organizational structures governing the different campaigns, the relationship between president and national party committee, and the changing workloads and methods of decision making that take place once the campaign has started. Her principal conclusion is that partisan politics and the quest for reelection are taking up an increasing amount of a president's time and that "during an election year the business of governing is overshadowed by the business of campaigning" (p. 156). In the final chapter, she argues that this current state of affairs poses serious problems for effective governance and suggests that the country would be better served if presidents would rely more on party organization for electoral assistance.

Changing gears a bit in an epilogue, Tenpas turns political consultant and provides some advice to the winner of the 2000 election on how he or she should run for reelection in 2004. The advice includes the following ten tips: (1) Don't delay planning; (2) raise as much money as possible as early as possible; (3) look like a president; (4) don't hesitate, retaliate; (5) set the agenda; (6) follow the rules; (7) don't be the campaign tactician; (8) take advantage of surrogate campaigners; (9) "the vision thing"; and (10) be lucky.

Although not necessarily incorrect, these suggestions, like many of the findings in the book, are unexceptional and are not discussed in connection with any models or theories of presidential elections or presidential behavior. In fact, the lack of any sort of framework makes it difficult for one to discern the main thrust of the project. As noted above, Tenpas describes a typology of three types of reelection campaigns: victorious, defeated, and takeover. This typology (introduced in the early pages and not mentioned again in the text) and the epilogue suggest that the book is about how reelection strategies influence electoral outcomes. If so, what do we learn by grouping cases by electoral outcome? Did victorious presidents do everything right and losers everything wrong? What was the relative effect of a president's performance as candidate? Furthermore, if the book is about the effect of different techniques of organizing and running a campaign, there is scant discussion of the fundamental factors that influence both campaign strategies and election outcomes.

The book also concerns itself with the mechanics and timing of running a campaign from the White House. Ultimately, the main empirical findings are that presidents and their staffs now devote more time and exert greater control over reelection activities, paying even more attention to the political consequences of their actions as election day nears. This basic point seems sensible enough. Moreover, it is a legitimate exercise to document such a political phenomenon. What this study lacks, however, is an analytical model or theoretical framework to guide and structure the empirical research. Finally, if change is the central thesis, more effort should have been put into describing the causes of the change.

One possibility would have been to discuss how the increasing attention to electoral goals and the increasing White House control of reelection campaigns fit with the theoretical arguments and empirical findings of Samuel Kernell's work (see *Going Public*, 1997). Are both phenomena being caused by the identical set of factors? Do reelection activities differ from the seemingly permanent campaign tactics that a president may use to rally support for his person and policies? What effect do these changes have on the role of the president and broader theories of presidential behavior? Moreover, if one is concerned about the effect that reelection motives may have on policymaking and how that effect may increase over the course of a term, it would also have been useful to sketch out some sort of general model of presidential decision making. Although the author introduces data on legislative proposals and cabinet travel, no systematic or even anecdotal evidence is presented on how reelection imperatives may have changed the direction of public policy.

Finally, the author provides no account of a systematic research design or even a description of how the data were gathered. In fact, the only description of the research modal-

ities comes in a sentence in the series editor's preface. For example, the Appendix contains an impressive list of presidential staffers and strategists who were interviewed, but no other information. Elite interviews can provide extraordinarily valuable data, to be sure; however, readers need to know how subjects were chosen; how the interviews were obtained; who refused to be interviewed; what the ground rules were; and what questions were asked.

Defining the National Interest: Conflict and Change in American Foreign Policy. By Peter Trubowitz. Chicago: University of Chicago Press, 1998. 353p. $55.00 cloth, $18.95 paper.

William O. Chittick, *University of Georgia*

Just when the democratic peace has focused most of our attention on domestic values and institutions, Peter Trubowitz has produced a masterful book which shows that economic interests trump both values and institutions in their ability to explain changes in American foreign policy at critical junctures in our history. In the process of making his argument Trubowitz also challenges the realist notion that international structures have a uniform effect on the state. Using congressional voting patterns as a proxy for the country as a whole, Trubowitz shows how international influences affect different sections of the country differently due to uneven economic development in various regions. His analysis is based on the economic conflicts between three regions: (1) the Northeast as delineated by the Potomac, Ohio, and Upper Mississippi rivers; (2) the South, which includes Texas and Oklahoma, and (3) the West, which includes the rest of the Great Plains, the Mountain States, and the Far West.

Trubowitz organizes his story around three decades of conflict and change among these regions: the 1890s, 1930s, and 1980s. The imperialism of the 1890s is explained by Trubowitz in terms of the success of the Northeast in wooing the West from its sister agricultural region, the South. The Northeast needed new markets in Latin America and the Pacific, and they sought a "bargaining tariff" to pry more favorable trade arrangements with countries in those areas. The South opposed these bargaining arrangements because it feared that its trade with Europe might be jeopardized by America's competition with Europe in Asia and Latin America. Although the West was less dependent upon international trade than either the South or the Northeast, it had little to lose in seeking new markets abroad. As a consequence of these economic considerations, the Northeast and West also favored a strong navy and imperialist policies, whereas the South generally opposed both imperialism and expenditures for the navy.

Three decades later the Northeast had become a major center for world trade and investment. It wanted the federal government to open up new markets and prevent the continental powers in Europe from excluding those areas from the market. This time it was the South that sided with the Northeast because the South needed to maintain its trade with Europe. Both sections were eager to support the British and the French in their effort to block German control of Central Europe. Although the South was very weary of the growth of executive power during the New Deal era, it was prepared to support new executive power in foreign affairs in order to preserve its European markets. The West was less dependent on trade and was suspicious of the penchant of the Northeast to grow at its expense by garnering more and more federal dollars for military purposes. Trubowitz argues that the bipartisan consensus on foreign policy during the early Cold War period is actually based on this regional economic alliance.

By the 1980s the new industrial areas were largely in the West and the South. Manufacturing in the Northeast was no longer very competitive, and the region needed the tariff and other restrictions on trade to protect its jobs and income. The Northeast was down and almost out. Now it was the West that favored free trade as more and more of its agricultural production was being sold abroad. Because there was more industrialization in the South, that region had mixed views on free trade. But the Republicans in the West were able to woo the South to their side by emphasizing the need for an arms build-up since both regions received the lion's share of defense expenditures. Thus a West-South coalition was formed against the Northeast.

Trubowitz does a good job showing how these sectional economic interests influence partisan politics, institutional conflicts, and the battle of ideas. He employs a series of maps, tables, and line and bar graphs to present his evidence, most of which focuses on key congressional votes on selected issues. I wish he had introduced public opinion data to fortify his position in the more recent cases. However, I don't want to quibble very much with the author of a book that is as well documented and well argued as this one.

Changing Public Sector Values. By Montgomery Van Wart. New York: Garland, 1998. 328p. $60.00 cloth, $24.95 paper.

John S. Robey, *University of Texas–Brownsville*

This volume is intended to provide a theoretical base for the analysis of changing values in public administration. The author examines the sources of five "value clusters": personal or individual, professional, organizational, legal, and public interest. These are the same values that make up the American Society for Public Administration's Code of Ethics.

Van Wart views individual values as manifestations of "civic integrity," which includes the "universally held" values of honesty, consistency, coherence, and reciprocity. When these four values are established in an organization, the result is a "high-trust" organizational culture. No single definition exists for "profession," but the author suggests that it is characterized by self-awareness, a body of knowledge, a social ideal, a formal organization, a national academy, and ethical standards. The volume focuses on organizational values when they are in conflict with other sources of values. Some attention is paid to organizational health, design, and leadership styles. Legal values include respect for the Constitution, for federal, state, and local laws, and for judicial rulings. The author maintains that "at a minimum the law . . . and due process constitute a baseline for competing values." He provides several definitions of public interest values and speculates as to the implications of each. It is his view that ethical decisions result from thorough consideration being given to all legitimate values.

Van Wart examines basic assumptions for public administration for each of several "value domains." This exercise generates a list of 17 basic assumptions that may be used to understand decision making in the public sector. For example, legal values lead a public administrator to be subordinate to the law, legislative intent, political bureaucrats, and judicial mandates. Public interest values lead administrators to implement public policy but not usurp the policymaking process, to be efficient with public resources, and to support the public's right to know and be involved. Individual values lead administrators to make unique civic contributions and to

have a well-developed sense of civic integrity. Professional values lead them to value training and education and to be influenced by the knowledge of the professions. The first two parts of the volume emphasize values from the perspective of individual administrators. The third part analyzes values from a cultural perspective. The author examines the role of culture from within an organization and narrows his analysis to the values of subsystems and ultimately to individuals within the subsystems. This analysis relies on the work of Schein and Ott, which divides culture into value levels, such as basic assumptions and beliefs, and tangible levels, or patterns of actions and language, art, technology, and material goods. Van Wart then uses three paradigmatic alternatives to understand the belief level of public administration. These paradigms are the "weak administrative paradigm," which places a minimal importance on independent institutional authority; the "mixed administrative model," in which the public sector's uniqueness is minimized; and the "strong administrative model," in which independent institutional authority is enhanced. These models allow the basic assumptions of public administration to be evaluated and interpreted with more clarity. Each model has strengths and weaknesses.

The author then directs his attention to the selection of decision-making processes, with some emphasis on rationality. This leads to the development of a three-tiered typology of rationality: cognitively oriented, noncognitively oriented, and politically oriented. Cognitively oriented rationality includes rational choice, and competition leads to efficiency. Noncognitively oriented rationality includes tradition, and customs lead to consistency with the past. Politically oriented rationality includes elitism, and aristocratic means lead to support from key groups within the society. The author selects reasoned choice models for some emphasis and integrates culture values with operational decision-making values.

Van Wart notes that public administration is undergoing a mammoth transformation as the values of the New Deal and the Great Society give way to changes in modern management techniques and a redefinition of the pursuit of happiness. In the concluding chapter he ties together his basic assumptions, types of rationality, the science of values, and the importance of these concepts for public administration.

The volume addresses an important aspect of public administration. That is, how do we determine what is ethical behavior in a time of dynamic change and competing sets of values? The categorizations and precise use of definitions by the author minimize misunderstanding. Van Wart both clarifies and simplifies the analysis of value choices confronting public administrators. His treatment of ethics is in the tradition of Terry Cooper, Hal Gortner, and John Rohr. But the underlying significance of the work is that it goes beyond ethics and comprehensively examines the broader field of values. This perspective leads the author to discuss the literature from various academic fields (e.g., philosophy and anthropology) and schools of thought that sheds new light on the study of values and ethics.

Van Wart builds a logical and systematic rationale for the scientific study of public sector values. The volume seems at times unnecessarily wordy and obtuse, but the author is dealing with abstract categorizations that do not lend themselves easily to simplistic explanation. It is a thoughtful work that delves deeply into the subject matter. The book is intended for scholars and practitioners of public administration, but it also may find an audience in students of ethics, philosophy, and the social sciences in general.

The author is to be commended for presenting us with a provoking synthesis of the dynamics of various streams of thought relating to values and ethics in the public sector. His work is a valuable addition to the literature and a good first step toward his goal of "creating a field of public administration values."

Comparative Politics

State, Nation and Ethnicity in Contemporary South Asia. By Ishtiaq Ahmed. London and New York: Pinter, 1996. 326p. $110.00.

Ted Robert Gurr, *University of Maryland, College Park*

The new literature on ethnopolitics includes a growing number of regional comparative studies, but most are edited collections of case studies in which a common outline or framework is used as a weak substitute for theoretically relevant comparative analysis. It is also uncommon to find either case or comparative studies of ethnopolitics that are grounded in the general theoretical literature. The book under review is an important exception. Ishtiaq Ahmed, a Pakistani-born political scientist who has spent his academic career at Stockholm University, has written an impressive synthesis of South Asian and Western scholarship on the accomplishments and contradictions of the state-building project in India, Pakistan, Bangladesh, and Sri Lanka. Though he relies primarily on secondary sources, he also draws on extensive interviews with leaders of separatist parties and movements throughout the region.

Ahmed begins with an incisive review of theoretical issues in the literature on ethnic identity, nationalism, and the nature of state-society relations. This provides the point of departure for the core of his own analysis of communal and regional conflicts in South Asia. The essence of a complex argument is that state and society in the postcolonial South Asian states were weakly linked. The first generation of leaders were committed to secular, democratic principles but at the same time had to ensure the survival of new states in multicultural societies with disparate, often competing and mutually hostile identities and interests. The establishment of viable states in this situation meant achieving two main objectives by state action: economic growth and consolidation of state control over disparate cultural entities. The specifics of the policies adopted toward these ends differed among the states, but there were some common consequences. In the 1950s and early 1960s the "nation-state project," as Ahmed characterizes it, achieved a number of gains. But as the scope of state action expanded, modernization had differential effects on social and cultural groups, and the political process became unduly influenced or corrupted by particularistic and communal interests.

Beginning in the late 1960s, but mainly from the late 1970s onward, a number of separatist and communal challenges were mounted, affecting all states in the region. Many of these Ahmed characterizes as "separatist projects," including those that mobilized the Muslims of East Bengal and Kashmir, the Sikhs in Punjab, the Tamils in Sri Lanka, the tribal peoples of the Chittagong Hills in Bangladesh, and the Sindihis and Mohajirs in Pakistan. Most of these groups had supported regionally based parties, such as the Awami

League in East Pakistan and the Akali Dal in Punjab. With the radicalization of communal politics after the late 1960s, some of these parties shifted from the politics of group representation to campaigns for regional autonomy or independence. Most militant movements and leaders, though, emerged outside of and in opposition to conventional parties; examples are the Tamil Tigers (1976), Bhindranwale's violent campaign for an independent Khalistan (1978), and in Pakistan the Mohajir Quomi Movement (MQM, 1984).

Ahmed's analysis shows how policies of the central state contributed repeatedly and in diverse ways to the emergence and radicalization of communal politics. In some instances states pursued overtly discriminatory policies, such as the "Sinhala Only" legislation introduced in 1956 in bilingual Sri Lanka. In India the interests and electoral victories of regional parties in Kashmir and Punjab were sacrificed to the machinations of a Congress Party determined to maintain power at the center. In Bangladesh the Chakma peoples of the Chittagong Hills have lost their historic autonomy to the center and their land and resources to Bengali settlers from the lowlands. The reader is drawn inevitably to the conclusion that central governments have been principally responsible for the communalization of South Asian politics: Most separatist projects were at their outset reactive, not proactive, and they escalated from conventional politics to violent resistance in response to misguided state efforts to manage conflict.

Sectarian cleavages characterize some South Asian conflicts, but most of the potential separatists are distinguished from those who control central state power by language and separate historical status, not religion. Religiously based challenges are exemplified by militant Hinduism in India (the Bharatia Janata Party), militant Islam in Pakistan, and militant Buddhism in Sri Lanka (the Janata Vimukti Peramnua movement). Unlike the separatists who seek substate autonomy or sovereign statehood, these sectarian movements aim at controlling the center, or at least at imposing cultural orthodoxy on heterogeneous societies. In both Pakistan and Sri Lanka sectarians have largely succeeded: Pakistan has largely abandoned the moderate principles of the founding Muslim League to become a theocratic Islamic state, while the Sri Lankan state has given primacy to Sinhalese Buddhist language and culture at the expense of Tamil Hindu culture. In India Hindu nationalists are on the verge of power at the center but, because of the exigencies of coalition-building, have substantially trimmed their position with regard to the status of India's 12% Muslim minority.

It is evident from these examples that religious minorities in South Asia are under pressure mainly because of the efforts of (some elements of) dominant groups to establish the hegemony of their culture and belief system. In Pakistan the pressures are especially severe for the non-Muslims (mainly Christians and Hindus), the Ahmadiyya sect, and Shi'is. The most serious contemporary challenges in Pakistan are not sectarian, however. They arise from communal competition for regional political and economic hegemony. The Mujahirs, descendants of Urdu-speaking Muslim immigrants from Punjab, have come to dominate the urban centers of the province of Sindh, above all Karachi. Resentment against their advantages has prompted a strong Sindhi separatist movement. But the Bhutto family is also Sindhi, and when in power it has promoted Sindhi interests at the expense of the Mujahirs. In reaction, some Mujahirs support a rival separatist project pursued by the terrorist MQM movement.

International factors are implicated in separatist and communal conflicts throughout the region, as Ahmed makes clear in both the country studies and an excellent concluding chapter. The conditioning factor is the psychological legacy of partition in 1947 and the massacres and population displacement that followed. It is manifest in mutual insecurity of the Pakistani and Indian elites and extreme sensitivity to political activism on behalf of one another's coreligionists (Muslims in India, Hindus in Pakistan). The two countries' rivalry has distorted not only their international politics but also their policies toward minorities within and across their borders. It makes Hindu nationalists highly resistant to accommodations in Muslim Kashmir and has contributed to the ascendency of Islamist politics in Pakistan. It motivated Indian intervention in East Bengal in 1971, Pakistani training of Muslim Kashmiri separatists, and clandestine Indian support for opposition communal groups in Pakistan.

In summary, Ishtiaq Ahmed has written an impressive comparative analysis of state-building and separatism in a region on which the social science literature is overwhelmingly descriptive. The conclusion builds on and synthesizes the historical and case study evidence to show how state-led modernization and uneven development have led to the reemergence of communal identities throughout South Asia and to separatist movements based on those identities. These movements have deep cultural and historical roots but are not "primordial"; rather, they are a political response to state-promoted political and economic change. In response to these movements all states in the region have in varying degrees relied on force to maintain control and have compromised or abandoned the secular and democratic principles of the first generation of postcolonial leaders.

Ahmed concludes his analysis with recommendations for peaceful resolution of communal conflicts in South Asia. At the international level he calls for engagement by worldwide movements concerned with human rights and points out the vital importance of mitigating the India-Pakistan rivalry. He rejects particularistic ethnic solutions based on redrawing the political map of the region and argues instead for power sharing and regional autonomy within existing boundaries. "A greater share in development coupled with cultural and regional autonomy appears to be the only formula that can create stability and peace within the states and in the South Asian region" (p. 299). The emerging middle classes are potentially a major local force for what Ahmed calls "humane development," including a concerted political effort to reduce state reliance on coercion and to address inequities across communal, class, and gender lines. The vision is grand, and the analysis from which it derives is careful, detailed, and on the whole convincing. *State, Nation and Ethnicity in Contemporary South Asia* is, in sum, a major contribution to the literature on modernization and the resurgence of communal challenges in the global South. And it should stand on its own as a text for courses in South Asian politics.

Democracy and the Marketplace of Ideas: Communication and Government in Sweden and the United States. By Erik Asard and W. Lance Bennet. Cambridge, UK: Cambridge University Press. 243p. $59.95 cloth, $18.95 paper.

Jan-Erik Lane, *University of Geneva*

Can one do imaginative political theory without using rational choice? The hard-core rational choice scholars in our discipline would, of course, flatly answer "no." They should take a look at this little book, which theorizes about the present predicament of popular government in two entirely different countries in terms of size, institutions, and political behavior. Asard and Bennet present a beautiful exploration

of the relationship between politicians/parties and the electorate in the 1990s without all the paraphernalia of noncooperative game theory. The result is a most stimulating analysis of the malaise of democracy today.

Despite all their differences, American and Swedish democracy suffer from the same problem: voter dissatisfaction, expressed in electoral volatility if not apathy, with political elites becoming increasingly myopic and opportunistic in their communication with the electorate. Asard and Bennet suggest a new interpretation of these facts by building an intuitive model that combines political communication concepts with neoinstitutional notions. How do they explain the rise of voter individualism and elite cynicism in two such different institutional environments as the gigantic United States and tiny Sweden?

Political institutions, the book argues, are open to invasion from cynical forces when the communication between voters and the politicians/parties is no longer focused upon major ideas but degenerates into media manipulation. Two such major ideas were the New Deal and the so-called Folkhemmet, which canalized the political efforts of people for decades in the two countries, giving clear-cut meaning to electoral combats and legislative work. Since these major ideas have declined in relevance and nothing similar seems to be forthcoming, the authors look for remedy in institutional reform. Thus, for the American liberal democracy, they wish to stem the role of money in U.S. electoral campaigns, limit political advertising on television to make it more equitable, reform the journalistic profession, and finally have PR introduced in the election system. For the Swedish welfare state democracy, they suggest restricting the number of national parties but increasing the number of local government politicians, strengthening the famous system of public enquiries to foster intelligent policymaking, and regulating the use or abuse of political information in the mass media.

If something critical should be stated about this brilliant book, then I would focus upon the "ought" and not the "is." For students who seek a succinct analysis of the politics of the New Deal or the Folkhemmet, this book is worth consulting. But the recommendations seem contradictory and statist in tone. Perhaps government is no longer so powerful that it can accomplish these reforms? Maybe that is the reason the New Deal and Folkhemmet ideas are no longer relevant? I would look for the remedies in more political decentralization and a more vibrant civil society, à la Tocqueville, especially with regard to still very socialist Sweden.

Tethered Deer: Government and Economy in a Chinese County. By Marc Blecher and Vivienne Shue. Stanford, CA: Stanford University Press, 1996. 265p. $39.50.

Dali L. Yang, *University of Chicago*

The research for this book started in the late 1970s when China finally opened to American social scientists for field research and the authors sought to bridge the chasm between elite-centered analysis and studies of grass-roots politics, two trends then prevailing in scholarly research on China. Accordingly, the authors chose to focus on a Chinese county in order to examine how it related to upper levels of the state on the one hand and local governments and society on the other. Shulu (Tethered Deer; renamed Xinji municipality in 1986), the county that became the case for research, is located on the north China plain and not exceptionally advanced politically and economically. It remained more collectivist than many coastal cities as of 1990. The authors observe that Shulu's "conservatism" was related to its history as a Communist Party stronghold during the Chinese Civil War.

Based on extended field visits made over a decade, the authors have produced a dynamic and variegated portrait of the political economy of Shulu, with insightful comparisons between the late Mao period (1970–78) and the first decade of the post-Mao reforms (1979–90). After an overview of the historical and material settings, the authors present information about the county government and then analyze Shulu's financial structures and relations. They find a highly diverse array of ownership patterns and financial relationships. This is followed by separate chapters on the county government's role in industrialization, commercial development, and urban as well as rural development. In all these policy arenas, state involvement has persisted in the reform era and in some cases, such as the industrial economy, the role of the state has expanded. For leaders in Shulu, the authors point out, it is natural that the state should play a dominant role.

The most striking trend about the Shulu county government was the "unprecedentedly rapid expansion and elaboration" of the bureaucracy over the 1980s (p. 29). As the number of bureaus expanded and county-level bureaus established branch offices, the number of administrative personnel also grew rapidly. Ironically, the state expanded even while more and more socioeconomic activities took place outside state planning. In contrast to the currently fashionable Chinese official policy of government downsizing, the authors explain that the government expansion was neither surprising nor excessive because the state was taking "a much more active role in monitoring and regulating both the obviously beneficial and also the potentially disruptive activities of this burgeoning arena of unplanned endeavor" (p. 32).

In line with the conventional literature, the authors find that the county government was not so much bound to the hierarchical political system as it is tethered to it. While local leaders pay heed to directives and regulations coming from above, they nevertheless find ways to adapt them to local conditions. There is definitely room for local initiative and maneuver, even during the late Mao era. Indeed, the authors argue that the most noticeable role the Shulu county government has played is that of "the developmental state."

The concept of the developmental state may serve as a convenient shorthand, but the authors' use of that concept raises important questions. Implicit in their use of the concept is the notion that "the state itself does not seek to make profits, but rather to create the conditions for enterprises to do so" (p. 209). They claim that the Shulu county government as well as individual leaders have fostered conditions conducive to growth and entrepreneurship but have nevertheless refrained from getting into profit-seeking activities itself. One major case the authors cite is the Hebei Yiji Market, a marketplace set up by the county government to facilitate trade and strengthen regulation. The authors claim that the Shulu government undertook the entire Hebei Yiji project on a nonprofit basis and did not use it to make money (p. 139).

I find such an interpretation questionable, especially because the authors' fascinating discussion of Shulu's fiscal affairs mention that budget expenditures grew twice as much as budgetary revenues. This fiscal situation would provide much impetus for the county to seek new sources of revenue, much of which went into the extra-budgetary accounts. The authors mention elsewhere that many new and revived taxes came into being and Shulu officials both at the county and township levels tightened tax collection (p. 60). Leaving aside the fact that the county government continues to own a significant number of enterprises (thus making the county

government a major player in entrepreneurial activities), the Yiji case would prompt the following questions: Did not the Yiji facilitate the collection of management fees, taxes, and other surcharges and thus enhance revenue generation? Did not the government sell parts of the real estate to shopkeepers and thereby make some money? Did the commerce people see their bonuses rise as the Yiji flourished? If the answers to these questions are affirmative, then it is highly likely that the county government was not just developmental but also entrepreneurial and perhaps even rent-seeking.

It appears that the authors' conclusions were influenced by the way they conducted their fieldwork. They relied on briefings with government officials and collection of official statistics and "had only the most limited contact with the people of Shulu county" (p. 11). Needless to say, government officials emphasized the good side of government and avoided discussions of leadership conflicts and cadre corruption. In contrast, other studies that relied on grassroots interviews have tended to find the state to be rent-seeking and even predatory. The truth is probably somewhere in between and most likely varies significantly across time and space.

All in all, this admirable study sheds much light on complexities of the Chinese state, state-society relations, and central-local relations. Because this study focuses on one county (case), it necessarily leaves inconclusive answers to some questions. Yet, the care with which the authors have conducted this research means that this will be the study later research on intermediate levels of government in China must take into account and be compared to. More such case studies in China will also allow for comparisons across space so that we can better assess the determinants of government performance and state-society relations.

***Desenvolvimento*: Politics and Economy in Brazil.** By Wilber Albert Chaffee. Boulder, CO: Lynne Rienner, 1998. 231p. $49.95.

Elizabeth McQuerry, *Federal Reserve Bank of Atlanta*

Looking at the case of Brazil across a period spanning military dictatorship to elected civilian government, Chaffee tests the hypothesis that improved economic growth is correlated with popular political support. He argues that there is a positive association in Brazil and that successive administrations have avoided the hard choices necessary to combat inflation because such steps would have undermined their political support base. In Brazil this association was internalized by politicians through a particularized notion of *desenvolvimento*, or development, that was once popular in the leading Latin American economies.

Chaffee correctly identifies inflation as a key factor in the equation between political support and the economy in Brazil. Substantial price increases were a constant and defining characteristic from the 1940s to mid-1994, when the inflation-fighting Real Plan was introduced. The bulk of the analysis focuses on how various political decisions have derailed well-intentioned economic plans that, lacking political interference, might otherwise have attacked inflation and thereby improved the economic outlook: "Stabilization efforts have died because no sector of the economy is willing to pay the cost. . . .The basic problem is one of coordination. And a preference for current public goods over future public goods both contributes to and results from the failure of coordination" (p. 24).

Chaffee's analysis places the failure to lower inflation squarely if not solely on government. Indeed, successive administrations have made choices that undermined their own economic programs and served to aggravate rather than contain inflation. Such a focus may be too narrow, however. Although government (and its component actors) is the ultimate arbitrator of economic policy design, it has not been the only obstacle to lowering inflation in Brazil. Private sector actors, most especially the financial sector, possessed the means to hedge against or even benefit from inflation through dollar accounts and overnight accounts. Put another way, neither the general population nor nongovernmental elites provided incentives for politicians to make the difficult decisions necessary to halt inflation. As well, contrary to Chaffee's assertion that business and financial elites "strongly supported" privatization (p. 25), the private sector has not uniformly supported opening up Brazil's economy, and liberalizing policies have often encountered the opposition of business sectors that would lose their privileged position in an open economy.

The chapters do a very good job of describing the predominant political scenario in Brazil. Low party cohesion promotes rent-seeking behavior on the part of political actors, and the pursuit of political profit often interferes with responsible policy selection. Lacking in this discussion, however, is a thorough exploration of how these preferences are formed and exercised.

Chaffee devotes very little space to discussing his data and methods, but he does complement the short statistical findings with extensive descriptions of preferences and economic policy choices in varying administrations. His own review of the data finds "a highly significant, strongly negative association between economic conditions and public support of the government. Evidence is strong that changes in economic conditions result in alterations in public support of an incumbent" (pp. 109–10). While the assertion that political regimes seek public approval is an intuitive one, it demands empirical testing. Yet, this analysis did not delve below the surface of public opinion data to explore important questions, such as the effect of popular mobilization against military rule in Brazil. The arguments would also have been enhanced by the inclusion of some theoretical discussion of why, as in the Brazilian case, military dictatorships needed to implement economic policies designed to curry popular support.

Unfortunately, the analysis rarely mentions the role of institutions (and institutional representatives) in shaping or determining political and economic outcomes. This omission is curious, given the author's proposition that the political imperative of economic growth in Brazil fueled an adherence to the doctrine of *desenvolvimentismo* (developmentalism), a concept that relied heavily on the creation and use of state agencies to carry out ambitious development plans. In Brazil, influential public and private sector institutions have played key roles in shaping economic policy and in postponing the adoption of policies to reduce inflation and lower deficits. The bureaucrats running these agencies have, in many cases, been fairly isolated from direct pressures from politicians and even from members of the administration. The relative absence of institutions is especially notable in Chaffee's analysis because he relies on Kathryn Sikkink's description of developmentalism, which posits a strong role for the state (*Ideas and Institutions: Developmentalism in Brazil and Argentina*, 1991).

Chaffee frequently refers to the twin challenges facing Brazil today: fiscal and political reform. He argues, quite correctly, that genuine, sustainable fiscal reform is not possible without taking away the incentive structure for politicians (which he calls "political profit control") to obfuscate or postpone efforts to stabilize government accounts. This im-

portant argument is never fully developed. A thorough exploration of how politicians use legislation and government funding to advance their political bases could supplement the book's basic argument that economic growth translates into political support.

Ultimately, this book's contribution to the literature on economic policy and politics is limited because it attempts to place too many complicated variables into a neat box. Although the effort to further our understanding of "macropolitics," or "the aggregate politics of politicians involved with the decisions about public goods" (p. 3), is valuable and much needed, this book falls short of its goals. The statistical treatment of polling data will interest many, yet it would have been interesting to see it used more extensively (along with a discussion of the drawbacks of using public opinion data, especially in politically repressive environments). Nevertheless, the rich detail in the description of the policy environment will make the book a useful addition to comparative politics and modern political economy courses that treat Brazil as one of several case studies.

Hot Coal, Cold Steel: Russian and Ukrainian Workers from the End of the Soviet Union to the Post-Communist Transformations. By Stephen Crowley. Ann Arbor: University of Michigan Press. 277p. $42.50.

Lisa A. Baglione, *St. Joseph's University*

Stephen Crowley has written an excellent book on worker activism in Soviet and post-Soviet politics. He examines two sets of puzzles: worker behavior and the attitude of laborers. To solve them, he advances our theoretical understanding of grassroots and postcommunist politics.

Crowley's concern with worker quiescence may appear surprising to the nonspecialist. Did not the coal miners bring down the Soviet government, and have they not continued to strike in the post-Soviet period? While noting the importance of those strikes, Crowley is intrigued with the inactivity of steelworkers (and others) in the Soviet era and the relative dearth of strikes considering the level of deprivation workers have experienced since 1991. Moreover, the steelworkers' willingness to keep working while the miners walked off the job is remarkable because the two were literally next-door neighbors. The author resolves this puzzle with his "mutual dependence" argument. Previous models of the Soviet political economy (i.e., totalitarian, civil society, and social contract approaches) are inadequate, argues Crowley. The system was one of shortage; enterprises suffered from insufficient supplies of raw materials, well-functioning machines, and workers. Since management depended on its workers to fulfill the plan and labor was in short supply, workers had power in the workplace, but it was limited. On the output side of the shortage economy, there were too few goods available for consumption. Enterprise officials doled out provisions, and they rewarded hard or specially trained workers with more or better goods. Thus, workers needed scarce products from the enterprise, and the distribution policy helped keep labor divided.

As Soviet economic problems began to mount, workers saw their wages dwindle and goods disappear from store shelves. Under these conditions, benefits became increasingly important, and mutual dependence was a significant obstacle to activism. Steelworkers received a large proportion of their remuneration in the form of in-kind benefits. To have struck or challenged the system would have risked not only their salaries but also these benefits. Moreover, in the post-Soviet period, losing one's job means not only the loss of pay but also the loss of these extras. New jobs that guarantee benefits are hard to find, and even if workers find new employment, they are at the end of the line for the distribution of necessities like apartments. Thus, mutual dependence has served to link steelworkers to their enterprises and to eschew strikes. Unlike other workers, coal miners earned a very high money wage and few benefits. As the economy deteriorated, salaries were not enough to protect them from hardship. Mutual dependence was low, and the miners struck as a rational response to the erosion of living conditions.

But why did the miners' demands shift from economic to political ones, and why did they initially support liberalism and then abandon that position? Crowley argues that miners were seeking a way to destroy the countrywide redistribution system that asserted so much control over their lives. They hoped for the dissolution of the USSR so that the fruits of their labor would remain closer to home. Shortly thereafter, miners became market advocates because they concluded that communism was the fundamental source of their exploitation. Although they were completely disgusted with socialism, their values were imbued with socialist principles. Miners believed that the market would produce justice, which in their minds meant that wage rates would follow from the labor theory of value and that workers would be owners. When these expectations were not met, miners became disillusioned, turning away from the reformers. But where could they turn? The party system did not offer any attractive alternatives, and workers remained allergic to the word "socialist" or the concept of social democracy. Thus, they cast protest ballots for the communists (who abandoned class-based politics) and the extreme Right.

For several reasons, Crowley's book is outstanding. First, the study is carefully designed to allow for a number of relevant comparisons: across industrial sectors (coal and steel), time (before the break up of the USSR and after), and place (in the Russian Kuzbass and Ukrainian Donbass regions). Second, his work is theoretically innovative. Building on the literature on the political economy of Soviet-style systems, he develops the mutual dependence framework. To understand the transformation in miners' demands, Crowley looks to social movement theory. (Here, he is especially indebted to those who stress the importance of culture, among them Jack A. Goldstone, "Ideology, Cultural Frameworks, and the Process of Revolution," *Theory and Society* 20 [May 1991]:405–53, and Sidney Tarrow, *Power in Movement*, 1994). Thus, he develops his argument from two seemingly opposing social science paradigms—rational choice and cultural analyses. Crowley convincingly defends this approach as appropriately suited to answering two different kinds of questions, one concerned with behavior and the other with beliefs. Third, the database for this study is extensive, and the collection effort is an impressive feat.

Crowley is telling an important story. The coal miners were at the vanguard of the movement to bring down both the state and communism. Moreover, industrial workers still comprise the largest segment of the post-Soviet population. While their dependence has allowed for the relatively peaceful building of capitalism, it does not necessarily bode well for democracy. Today, workers have no party or other organization that represents their interests in this system. Some nondemocratic group could tap into their disillusionment and ride a wave of worker support to power. Thus, post-Soviet democracy remains uncertain, and Crowley's study of workers is required reading for anyone seeking to understand polities in transition and the obstacles to activism in post-communist settings.

Mr. Smith Goes to Ottawa: Life in the House of Commons. By David C. Docherty. Vancouver: UBC Press, 1997. 295p. $75.00 cloth, $24.95 paper.

Herman Bakvis, *Dalhousie University*

Reelection rates to the U.S. Congress that typically are well in excess of 90% are seen by many as a major shortcoming, allegedly making legislators less responsive to citizen demands and limiting the opportunity for institutional change. Yet, in lamenting this state of affairs, critics tend to pay less attention to the other side of the coin, that is, the consequences associated with a relatively high turnover of legislators. In *Mr. Smith Goes to Ottawa*, on the Canadian House of Commons, Docherty presents just such a case. In Canada, turnover rates of 30–40% are not atypical, even when the incumbent government is successfully reelected. In the 1993 election year the exit rate of MPs reached 66%, with well over half the departures the result of electoral defeat. The outcome can be a legislature populated with political "amateurs," "politicians who come to national office without the benefit of any prior elected experience" (p. 40). One important consequence is the domination of these amateurs, and the legislative process as a whole, by a small group of experienced party leaders.

Canadian exceptionalism with respect to relatively low rates of reelection and executive dominance of the House of Commons is of long standing. It is an issue that has been discussed in a number of studies (e.g., Franks, *The Canadian Parliament*, 1987; Ward, *The Canadian House of Commons*, 1950), and this research builds on and extends previous work. Docherty makes contributions in three areas. First, through detailed surveys and open-ended interviews, using a substantial sample of candidates and MPs over two electoral periods, he provides what is likely the definitive portrait of the behavior and orientations of Canadian MPs. Essentially, in examining Canadian legislators and their behavior, Docherty has done for Canada what Richard Fenno (*Home Style*, 1978) did for the United States and Bruce Cain et al. (*The Personal Vote*, 1987) for Great Britain. Second, in probing incentive structures in the Canadian parliament he makes an important contribution to the neoinstitutionalism literature, demonstrating how rules and institutions can powerfully reshape and transform the attitudes and behavior of a whole new generation of MPs. Third, by relying on the results of what can best be described as a unique natural experiment, the author is able to bring compelling evidence to bear on his arguments.

Specifically, the natural experiment arose in the aftermath of the 1993 election, when not only two-thirds of the seats in the House changed hands but also a coterie of MPs arrived who were committed to changing the traditional norms concerning the role of parties in the House in general and the norms related to party discipline in particular. The 54-member Bloc Québécois departed from traditional norms simply by virtue of their commitment to taking Quebec out of the Canadian federation. The 52-member Reform (up from one seat in the previous parliament), while committed to Canadian unity, was equally revolutionary in arguing that MPs ought to vote on the basis of constituents' wishes, not on the basis of party affiliation. It thereby attacked a fundamental tenet of Westminster-style government, namely, that governments are sustained and opposed in the House of Commons on the basis of collective support by party members. Furthermore, a sizable proportion of incoming Liberal MPs of the newly elected majority government expressed strong misgivings about having to follow the dictates of party discipline. In brief, 74% of MPs of the 35th parliament brought with them to Ottawa a view distinctly at odds with traditional understandings of party government: They intended to follow "the wishes of the constituency over all else" (p. 145); in the previous parliament only 30% were so inclined. Nonetheless, after two and one-half years working in the House, less than 60% of the 35th parliament still felt that way.

The change in attitudes, according to Docherty, can be attributed primarily to the perceived structure of incentives inherent in the Canadian parliament and to the active intervention by party leaders. Loyalty to the party leader is seen to be far more important than policy or legislative expertise. This holds particularly true for government members, who are expected to support measures developed and introduced in the House by the executive and to desist from moving changes to bills that are at the committee stage. Even in caucus, when members ostensibly can speak their mind to party leaders, the potential for influence is limited. The inexperience of new members makes them particularly susceptible to the authority of the leadership. Party leaders (including the prime minister) are selected in party conventions in which the parliamentary caucus plays only a limited role. Members of the cabinet and of the shadow cabinet, in the case of the opposition, are appointed by the prime minister and leader of the opposition, respectively. Hence, MPs have relatively little leverage vis-à-vis the party leadership, unlike other Westminster systems, in which party leaders are often directly elected by the parliamentary party. Those MPs tempted to sit as independents are confronted with almost certain defeat in the next election, given the overwhelming importance of party and party leadership as determinants of electoral behavior, and they have an extremely circumscribed role during their remaining time in the House.

Yet, according to Docherty, there is more than simply brute force and an imbalance in resources between members and party leaders. MPs also come to recognize and accept the legitimacy of the logic underpinning the need for party discipline and loyalty to the "team." In the case of those initially wishing to act as delegates of their constituency, MPs discover that ascertaining a consensus among constituency views is a tricky task at best, a dangerous game at worst. Careerists, defined as those with at least two elections under their belt, are much less inclined to believe that relaxing party discipline would lead to an increase in a personal vote (15% versus 54% for first-term "apprentices" [p. 220]). Thus, over time, party discipline is seen as much less a hindrance in representing constituents' interests and more an element of protection for MPs and a measure of mutual respect for fellow party members in the House.

The arguments in the book are carefully constructed. The chapters on arrival, coming to terms with parliamentary norms, and exit take us through the life cycle of the typical MP's career, discussing issues such as the nature of representation and gender differences in its conceptualization and reassessing conventional wisdom in a number of areas. The author notes, for example, that incumbents are caught in a double-bind: "A failure to engage in district work can kill members politically, but devoting more time to it might not make a difference at the polls" (p. 211). At the same time, while the effects of a personal vote in Canada are discounted by most studies, Docherty notes that incumbents who manage to survive at least two elections do significantly better than nonincumbents. In other words, at a certain point, the benefits of constituency work do kick in, and incumbents can more easily distance themselves from the party and its leadership and rely more on their personal following. Ironi-

cally, these two-term MPs are also less likely to express a willingness to buck the party.

The author has an excellent grasp of the material, although his literature review is probably too detailed, betraying the dissertation origins of the book. While focused on Canada, profitable comparisons are drawn with other legislative systems. Overall, this is one of the best recent books on Canadian politics and a significant contribution to the field of comparative legislative studies. If there is a limitation, it lies perhaps with the extensive reliance on surveys with MPs and candidates. First, as Docherty admits at the outset, the response rate by Bloc Québécois MPs to the surveys was extremely low, so that this highly ideological nationalist party is effectively excluded from the analysis. Second, in focusing on the parliamentary incentive structure as perceived by MPs, the author neglects the broader exogenous forces shaping the overall logic of parliamentary government in Canada. The country's linguistic cleavage and the strength of provincial governments tends to lead the national government to curb internal party dissent for fear of giving Quebec in particular but also other provincial governments opportunities for exploiting perceived weaknesses in the government's position. Also, the schizophrenic character of Canadian parties means they have little control over the candidate selection process at the constituency level, so the party leadership compensates in part by resorting to more stringent measures to ensure reasonable coherence of the party in parliament.

Finally, one of the more important themes of the book concerns the convergence among, and ultimately integration of, members of all parties into the norms of Canada's central governing institutions. This evolution is seen by Docherty by and large as a positive development, providing stability and continuity but still allowing for incremental change. Yet, while there may be a convergence among MPs, no such development appears to be occurring between MPs and citizens, a gap Docherty's otherwise fine book does not really address. Surveys of public attitudes toward parties and the role of MPs indicate not only that a majority believes the rule of party discipline and related norms ought to be eliminated but also that the gulf between citizens and MPs on these issues is growing. (See André Blais and Elizabeth Gidengil, *Making Representative Democracy Work: The View of Canadians*, 1991). While the public's expectations of elected representatives stress responsiveness to local concerns and a capacity to affect policies between elections, in good part a function of values that have flowed northward from the United States, the role of MPs in the House of Commons is shaped almost exclusively by Westminster parliamentary norms, reinforced by the factors noted above, so as to make it even more executive and party dominated than its present British counterpart. It is this gulf between the Canadian parliament and the broader context in which it operates that remains one of the more profound challenges facing the Canadian polity.

Principles of Electoral Reform. By Michael Dummett. Oxford, UK: Oxford University Press, 1997. 189p. $65.00.
Electoral Change since 1945. By Pippa Norris. Oxford, UK: Blackwell, 1997. 254p. $59.95 cloth, $21.95 paper.

Vernon Bogdanor, *Brasenose College, Oxford University*

Michael Dummett is a leading philosopher and logician of his generation, and his work on Frege has revolutionized the subject. In *Principles of Electoral Reform* he seeks to apply the tools of his trade to the theory of voting. He reiterates some of the arguments of his previous book, *Voting Procedures* (1984) but in rather less technical language. In the earlier book, Dummett had sought to construct a bridge between mathematical theorists of voting and political scientists, deploring the fact that so much of the modern theory of voting seemed unknown to political scientists. That criticism is perhaps less applicable now than it was in 1984. *Principles of Electoral Reform* presents some of the main arguments of voting theorists in a form easily accessible to nonmathematicians and to those untrained in formal logic. There is thus even less excuse than before for political scientists to remain in ignorance of this important body of work.

Principles of Electoral Reform is a work of rigor and profundity that needs to be read by any political scientist with an interest in electoral systems. Yet, its central arguments seem to be misconceived. Dummett is concerned less with recommending a particular electoral system than with formulating principles and clarifying issues. His central contention, however, is that the single transferable vote (STV) is inferior to a system of voting in multimember constituencies based on the Borda criterion, which he calls the Quota/Borda system. Dummett shows that, contrary to the arguments of many of its advocates, STV does not eliminate tactical voting and also that it is nonmonotonic, that is, in some cases, one can harm a candidate's chances of election by ranking her highest. This is because the order in which candidates are eliminated can make a crucial difference to the result, since it is the preferences only for eliminated candidates that contribute to the final outcome. STV, for Dummett, has the weakness that, at later stages of the counting process, it gives the same weight to some voters' later preferences as it does to other voters' first preferences, while never giving any weight at all to some voters' later preferences, depending solely on the accidents of which candidates are eliminated and in which order. In particular, STV "gives no weight to the second or later choices of the supporters of a candidate who survives until the final stage, only then to be defeated" (p. 105). Thus, "the sequential redistribution of votes under STV can magnify the effect of a slight change in the initial allocation of the votes" (p. 145). Hence, supporters of a candidate may not always be doing their best for her by ranking her first.

The question, however, is how often nonmonotonicity is likely to occur. Dummett criticizes one claim, that it will occur only in .028% of cases, as a "ludicrous underestimate," arguing on the basis of "a rough calculation," which he does not supply, that "with three candidates under AV [the alternative vote] 2% would be a conservative estimate" (p. 105, n). Yet, Patrick Bradley, Chief Electoral Officer for Northern Ireland, where STV has been used since 1973 for all elections except those for Westminster, claims that "the experience of the use of STV in Northern Ireland over the past 22 years, involving a range of election types and sizes, reveals no evidence to support *in practice* the lack of monotonicity" (*Representation* 33[Summer/Autumn 1995]:47; italics in original). Even if this were not the case, it would be difficult to see how the voter can *manipulate* STV to take advantage of this property of nonmonotinicity. Indeed, the Borda system Dummett recommends is, as will be shown below, far more susceptible to manipulation than STV.

Dummett argues that the concept of the wasted vote, which plays a prominent part in arguments for STV, "is thoroughly confused and unhelpful in thinking about electoral systems" (p. 118). Yet, his understanding of this concept is different from that of the advocates of STV whom he criticizes. For Dummett, a vote is wasted under any electoral

system if, "by voting differently, one could, *under that same system*, have obtained an outcome one preferred" (pp. 110–1). Advocates of STV, by contrast, regard a wasted vote as one that is not used to elect a candidate. Under STV, the only such votes are those cast for the runner-up, and it is usually the case that supporters of all the major candidates in a multimember constituency have a candidate of their favored party elected. The concept of the wasted vote, employed in this way, may be as incoherent as that criticized by Dummett, but Dummett does not prove it so.

An electoral system, in Dummett's view, has to answer two questions. First, how is the legislature to be composed? Second, who is to represent each constituency? Dummett argues that Borda's criterion or, alternatively, Condorcet's criterion, with the Copeland extension, is most likely to choose the most representative candidate in a single-member constituency. Dummett admits that "there can be no actual proof of this contention" (p. 66). This is because, to have a proof, we would require a criterion for the most representative candidate and show that it tallied with the Borda or Condorcet criterion. But it is just such a criterion that we are seeking. Therefore, the only test can be one of intuition. Dummett further admits that the Borda criterion may result in the defeat of a candidate who has an overall majority of first preferences.

Dummett claims in addition that, although no electoral system can avoid tactical voting, the Borda system is the only one which cannot be manipulated by a majority. That is an odd contention. Under the Borda scheme, as under any points system of voting, later preferences count against and may defeat a candidate who is the first preference of an absolute majority of the voters. Therefore, a sensible tactical strategy would be to use one's earlier preferences on those candidates least likely to be elected, giving the last preference to the most dangerous rival of one's favored candidate. It seems much easier for a voter to manipulate the Borda system in this way than it is to manipulate STV.

There are further incoherences in a points system of the Borda type. If the system yields 1 to the first choice, 2 to the second, 3 to the third, and so on, then the larger the number of candidates, the higher is the proportion of points allocated to the second and to later preferences as compared to the first. With three candidates, three points are allocated to later preferences, one in two votes counting against the first preference; with eight candidates, twenty-eight points are allocated to later preferences, so that seven-ninths of the votes cast count against the first preference. Thus, the number of points allocated to successive preferences is determined not by the voter but by the number of candidates. It would, of course, be possible to adopt a different weighting, for example, geometric rather than arithmetic. But the weighting one adopts, reflecting as it does assumptions about the relative strengths of voter preferences, is essentially arbitrary. The voter's ranking, after all, may be either 1, 2, 3, 4 or 1, 2, 4, 9. We have no means of knowing which of these is the correct one, no means of knowing how the voter perceives the relative strengths of the various candidates.

Electoral Change since 1945 is a very different sort of book from *Principles of Electoral Reform*. It does not pretend to be a work of original research but instead offers a synthesis of theories of electoral change in Britain since the end of World War II. It analyzes the development of the party system, from two-party dominance until 1970 to what the author calls a declining two-party system since that date, a characterization Norris prefers to such competing descriptions as multiparty system or predominant party system. The analysis is conducted in terms of theories of voting behavior and theories of party competition, but the author is in general cautious about drawing conclusions, being concerned more to expound than to evaluate.

Electoral Change since 1945 summarizes an astonishing amount of material. It will be of enormous use both to students seeking an overview of the subject and to established political scientists unfamiliar with the field. It is undoubtedly the best survey book of its type dealing with British electoral behavior that is currently available.

French Presidentialism and the Election of 1995. Edited by John Gaffney and Lorna Milne. Brookfield, VT: Ashgate, 1997. 305p. $76.95.

Roy Pierce, *University of Michigan*

This collective work on the French presidential election of 1995 is in the vein of the earlier volume edited by John Gaffney on the 1988 election. There is, however, a considerable turnover among contributors. Of the earlier team, in addition to Gaffney (who contributes a chapter on the mainstream Right, as well as coauthors the Introduction and provides the Conclusion), only Alistair Cole (on Mitterrand), David Bell (who provides a chapter, with Byron Criddle, on the leftist candidates), and Susan Milner (on the unions) appear in the more recent work.

The newcomers, some of whom are veterans, are the late Peter Morris, who provides an historical background; Lorna Milne, who writes on the myth of the president; David Goldey, who contributes an acute and well-informed analysis of the results; Irene Hill, whose chapter on political finance would be even more useful if less legalistic; Catherine Fieschi, who examines the "other" candidates, including Le Pen; Mairi Maclean, on the role of business; Pamela Moores and Christophe Texier, on the media; Jean and Monica Charlot, who provide expert commentary on the polls; Douglas Johnson, on foreign policy issues; and Catherine Pradeilles, who contributes a striking criticism of the media, politicians, and political experts as interpreters of French public opinion.

A book with sixteen chapters and seventeen authors can be expected to have certain strengths and weaknesses. Among the former are the range of information and variety in interpretation that can be provided by various specialists. There also is no shortage of interesting vignettes and insightful comments. Gaffney, discussing the mainstream Right, tells us that "in French politics, a heavy premium is placed upon friendship and loyalty; treachery, therefore, is everywhere" (p. 99). Goldey's analysis is particularly interesting on the semantics of the Communist Party at the second ballot, when it chose to defeat the Right but not to support Jospin! Bell and Criddle remind us that the presidential system, "supposedly inimical to the left, had given the Socialists their longest ever tenure of power" (p. 132). Fieschi tells us how the unknown Jacques Cheminade managed to get the 500 supporting signatures required to stand as a presidential candidate. The Charlot team points out how long it took for the various polling firms to ascertain the actual line-ups of candidates for the two ballots for purposes of measuring preelectoral voting intentions (whose limitations the Charlots also clearly describe), and they point out that none of them got the results of the first round of voting in the right order. Much of the chapter by Pradeilles is devoted to demonstrating how the media, politicians, and experts got it all wrong: Nothing that was expected actually happened, and they all

"exalted the presidential function," while the reality was a "weakened, impotent, almost absent President" (p. 233).

There are some lacunae. Discussing the 1962 establishment by referendum of direct popular election of the president, Morris reports only de Gaulle's version of the reason for doing so: that it would be necessary for his successors to be legitimized by popular vote. But by 1962, it was not certain that de Gaulle could be reelected under the same electoral college system that had elected him in 1958. It might have seemed to him that he was more popular with the public at large than with the political class from which an electoral college would be drawn. It is possible, therefore, that the popular vote was designed to ensure de Gaulle's reelection.

The book contains more discussion of why Chirac won than it does of why Jospin, who entered the race only three months before the election, did as well as he did. How much of Jospin's good showing was due to his leftist affiliation, how much to his personal probity, and how much to disaffection with veteran insiders parading as harbingers of change?

Finally, I would have appreciated more than the very few comparative references that appear throughout the book, usually to good effect. Only by comparison with the United States, or Russia, or even parliamentary Britain can one gain confidence that some phenomenon is characteristic of a given national system rather than of some more general behavioral imperative. While reading Gaffney's question about whether Chirac had really changed (p. 281), I found myself thinking of "the new Nixon" and "the real Bob Dole."

The potential weakness of a collective work is the lack of a common inspiration. Gaffney tries to give unity to the book, mainly in the final chapter. I think he is on the right track when he emphasizes both that French parties "are in a state of perpetual organizational activity, constant change, and near-permanent campaigning for the presidency" (p. 277) and that "individual decisions, tactics, and timing" (p. 278) have inordinate significance in that context. But Gaffney does not have the opportunity in this volume to develop, qualify, nuance, and document such a broad interpretation. I hope he pursues the issue elsewhere.

The Crisis in Kashmir: Portents of War, Hopes of Peace. By Sumit Ganguly. New York and Washington, DC: Cambridge University Press, co-published with the Woodrow Wilson Center Press, 1997. 182p. $49.95.

Robert G. Wirsing, *University of South Carolina*

Heightened tension between India and Pakistan in recent years has rekindled scholarly interest in the now more than fifty-year-old territorial dispute over the Himalayan state of Jammu and Kashmir. This book focuses on the roots of the insurgency that has embroiled the Indian-administered portion of the state since 1989. Its author, an established and prolific writer on subcontinental political and security affairs, provides a detailed narration of the insurgency's evolution. He considers both its shorter- and longer-term causes. The discussion includes examination of what might be done to resolve the insurgency. Relatively detached in comparison with much that has been written on the subject, the book manages nevertheless to run aground here and there on the shoals of controversy.

In his examination of the sources of the insurgency in Kashmir, Ganguly begins with a discussion of four existing categories of argument—(1) that the insurgency is little more than state-sponsored (Pakistani) terrorism; (2) that the insurgency arises from India's brutal denial of the Kashmiris' right of self-determination; (3) that the insurgency bears witness to the rise in Kashmir of ethnic subnationalism, which destroyed the syncretistic Kashmiri cultural identity (called *kashmiriyat*) that had bound Hindus and Muslims together peacefully for many centuries; and (4) what Ganguly labels circumstantial and atheoretical accounts, that the insurgency arises from a mix of historical variables. Dismissing all of them as more or less inadequate, Ganguly proposes an alternative explanation, one that relates the insurgency in Kashmir to the theoretical literature on ethnonationalism, and, in particular, to the pioneering work of Samuel P. Huntington on political mobilization and institutional decay. Ganguly argues that rapidly paced modernization, in the form of more education, literacy, exposure to mass media, and social and physical mobility, exponentially enlarged consciousness among Kashmiris of political rights and privileges. As their awareness grew of the possibility of an alternative future, so did their disaffection from the increasingly ill-functioning and repressive political apparatus—the "institutional decay," in other words—that increasingly characterized political life in the state of Jammu and Kashmir. Ganguly points to a curious paradox here, namely, that it was India's considerable success in introducing the benefits of modernization to Kashmir that eventually produced a radicalized younger generation for whom violent and separatist sentiments had a natural appeal. Most of Ganguly's book is an effort to chronicle the gradual widening of the gap between the enlarged expectations of Kashmiris and the Indian government's declining ability and/or willingness to alter the political circumstances that frustrated their realization.

The Kashmir dispute's genesis in 1947, which has filled countless pages in numerous other works on Kashmir, gets very short shrift in this one. Ganguly, conceding that the present situation cannot be grasped sufficiently without some understanding of regional history, devotes exactly eight pages to it. Treading on ground that had already been raked over by others often and with meticulous care, Ganguly might have been better advised to skip it entirely. Quick to dismiss the "highly prejudicial inferences and little or no evidence" (p. 11) of a hugely controversial British historian (Alastair Lamb, *Kashmir: A Disputed Legacy, 1846–1990,* 1991), Ganguly himself furnishes an amazingly truncated and haphazard version of the Kashmir dispute's earliest phases. Particularly puzzling is his rendering of the weeks immediately preceding the princely ruler of Kashmir's request for Indian troops in late October 1947. He states, for instance, that "under the terms of the Indian Independence Act, none of the princely states would be allowed to declare independence." (p. 8). In fact, the act did no such thing, the departing British having deliberately decided not to mandate the automatic legal transfer of paramount power over the princely states to the new governments of India and Pakistan at independence. That British reticence in this regard resulted in an immediate political tussle between India and Pakistan, the latter committing itself at that stage wholeheartedly to the legal right of the princes to decide the matter, is dealt with at considerable length by an Indian historian whom Ganguly cites frequently throughout his book (Jyoti Bhusan Das Gupta, *Jammu and Kashmir*, 1968).

Equally puzzling is the distortion of events found in Ganguly's narrative of the invasion of Kashmir by Pashtun tribals—an invasion that lent moral legitimacy to India's airlifting of troops to the beleaguered ruler of the state. In Ganguly's rendering, the "invading column" (p. 9) of tribesmen, augmented by disguised Pakistani troops, appears to have materialized at the Kashmir State border on October 22 following its participation in a successful rebellion earlier in

the month slightly to the south in the Poonch subdivision of Jammu and Kashmir State. Indian and Pakistani accounts of the facts of this invasion disagree violently; but there is widespread agreement that a significant portion of the tribesmen were recruited for the invasion in Pakistan's Northwest Frontier Province, that they were transported to the Kashmir State border in trucks and busses through Pakistan's Hazara district, and that at the border they were joined by rebellious Muslims from Poonch and from the Jammu and Kashmir State forces. Also objectionable is Ganguly's suggestion that the ruler of Kashmir, "caught in a panic" by the tribal invasion of October 22, appealed at that time for military assistance to the princely ruler of Patiala State. In this claim, Ganguly is given no support either by Lamb, whose version he dismisses, or by Jha (Prem Shankar Jha, *Kashmir 1947: Rival Versions of History*, 1996), whom he cites approvingly. Jha, anxious to free the Indian government of responsibility for military assistance to the princely ruler of Kashmir prior to the tribal invasion, goes to great lengths, in fact, to demonstrate that the Patiala men were dispatched to Kashmir possibly even before India gained its independence on August 15 but in no case later than the early days of October.

Once Ganguly gets beyond the contentious matters pertaining to the year 1947, his discussion grows more solid, both factually and analytically. His treatment of India's post-independence political history is methodical and builds a convincing case for his mobilization cum deinstitutionalization thesis. Other recent works (e.g., see Sumantra Bose, *The Challenge in Kashmir*, 1997) lend at least parts of his thesis strong support. Some of his substantive assertions, however, deserve careful scrutiny. His fixing, for example, of "the beginnings of the wearing away of the special status of Jammu and Kashmir as a state within the Indian Union" (pp. 56–7) to the years between 1962 and 1965 does not square with most of what has been written by either Indian or foreign writers. The Kashmiri Muslim leader Sheikh Abdullah's arrest and incarceration in 1953; promulgation in 1954 by the president of India of the Constitutional (Application to Jammu and Kashmir) Order, which vastly extended the Indian government's legislative powers in Jammu and Kashmir; or the extension to Kashmir in 1960 of the Indian Supreme Court's jurisdiction would all seem to be better markers for the wearing away of Kashmiri autonomy.

Also to be questioned is Ganguly's argument that Kashmiri Muslim disaffection from India is of very recent origin—hardly antedating the 1980s. As evidence, he points to the fact that neither in the 1965 nor 1971 wars between India and Pakistan did the Kashmiris seize the opportunity to rise against their Indian masters. It is true, of course, that they did not rise up then. But Ganguly runs well beyond available empirical evidence with his claim that "the vast majority of Kashmir's Muslims had [thus] amply demonstrated their loyalty to India during both the 1965 and the 1971 wars" (p. 64).

For this reviewer, the most compelling part of the book is its final chapter, in which Ganguly provides an astute and balanced critical assessment of the various strategies and options that have been proposed in recent years for resolving the crisis. His own proposal retains Kashmir solidly within India and, since it recommends only token concessions to Pakistan and rules out independence for Kashmir, is unlikely to meet with favor, at least initially, either in Pakistan or among vast segments of the Kashmiri Muslim population. But especially welcome is its broad affirmation of the need to restore Kashmir's autonomy, including the suggestions that Kashmir be returned to its pre-1952 status (with New Delhi left in control solely of defense, foreign affairs, and communications) and that the Kashmiris be invited to write their own constitution.

In sum, while Ganguly's book makes a substantial contribution to the burgeoning literature on Kashmir, it falls well short of being either a definitive study of the insurgency or a major theoretical breakthrough. It is, moreover, seriously marred at a number of points by an all-too-casual approach to facts as well as by its author's apparent wish to score points over his scholarly opponents.

The Disenchantment of the World: A Political History of Religion. By Mark Gauchet. Princeton, NJ: Princeton University Press, 1997. 272p. $24.95.

Adam B. Seligman, *Boston University*

The subtitle of Peter Gay's seminal book on the Enlightenment was, for those who do not recall, "the rise of modern paganism." The full meaning of this, however, was brought home to me only a few years ago when, upon giving birth to our second child, my wife was given the option of taking her placenta home in a plastic bag along with a recipe for soup.

Marcel Gauchet's overriding concern is in describing and explaining the long parabolic movement from the binding of Isaac to such forms of modern if not postmodern paganism. His concern is with that process inherent to religion per se which, in explicating transcendence, sets up its very demise and with it a plethora of subjectivist responses to the overriding questions of meaning, ethics, and normative order.

The different forms of "the religious after religion" are, however, only of passing concern at the end of this intricately argued, dense, difficult yet thoroughly absorbing work. The truly radical thesis presented by the author pertains not to the variety of "postreligion" phenomena but to the origins and dynamics of what have been termed the great world historical religions, or what Karl Jaspers called the Axial Age. His thesis is one of Hesoidian decline. Rather than the apogee of religious instauration, Gauchet sees that Axial Age, that period when the consciousness of transcendence registered across the myriad religious traditions of Ancient Israel, Ancient Greece, Early Christianity, Zoroastrian Islam, early Imperial China as well as in the Hindu and Buddhist civilizations (and much later in Islam), as marking the first yet critical stage in religion's decline.

For Gauchet, the primordial essence of religion is in its permanence and what he terms "sacral dependence" on and indebtedness to an inviolable and external foundation. What takes an effort to assimilate is his claim that the radical heteronomy of Axial religions actually reduced this otherness in the very act of proclaiming a transcendent sphere vis-à-vis which the world was ordered. His claim is that the very act of establishing a hierarchy, of bringing the world into relation with the transcendent, even as that act relativizes the world, its meanings, statuses, and orders, it does the same to transcendent otherness as well; relativizing it vis-à-vis the world. Rather than a breakthrough to transcendence he therefore sees the Axial revolutions as the first stage in the decline of an otherness that, existing prior to *nomos*, was hence all the more absolute.

Needless to say this is both original and controversial. Not that the Axial moment does not do what Gauchet claims. Indeed, the idea that by bringing the world into relation with the transcendent other more is relativized than just the world is striking and brilliant. Rather, it is in what existed on the other side of the Axial revolution, the ontological structures of pre-Axial being, that must be questioned. What is lacking or obscure is precisely a phenomenology of that imminence, that sense of

being which for Gauchet presupposes the absolute distancing of the world from its foundations. Hence, for example, Gauchet speaks of Axial religion as reorienting the temporal dimension from past to present. Is not the real revolution, however, in temporality as such, in the very idea of time, of temporal change, of becoming, of a present, but through the idea of a created past and final future? Without a clear hold on matters such as these (and here I believe Gauchet is at his weakest) it is hard to grasp what preceded the Axial Age. Here, by the way, the thought of Eric Voeglin is most helpful and surprisingly absent from Gauchet's writing.

In the reflective stance that accompanies Axial religion the distance of the world from its other begins to shrink. Yet, here too, the foreshortening is not of a set. A created world and, for that matter, a Creator God, enter into a very different dynamic than what we find in, say, the Hindu tradition. Consequently, the world relativizes in very different ways as does transcendence leading to very different modes of disenchantment. Now the lack of a comparative perspective would not of itself be a legitimate critique of a work such as Gauchet's except that he repeats an oft-encountered *deformation professionnelle* in religious studies; that of writing, however unknowingly, from a Christian perspective, even a secular and disenchanted one. All too often the general characteristics of Axial religion are conflated with the particular solutions posited by Christianity to the soteriological drama that the Axial Age opened up. Again and again the analytic boundaries between what is unique to Christianity and what was expressed in all Axial religions is blurred.

This is especially so in Gauchet's central point—that of the emergence and eventual triumph of the individual and subjective inwardness as the key variable in the decline of otherness. Here, however, a much more precise delineation of what is uniquely Christian and what was an aspect of all Axial religion is needed. In a sense the Pauline (and later Augustinian) revolutions are almost marginalized in importance as the category of the individual is made an attribute of Axial religion as a whole. This I believe is mistaken. Just as there was no "sudden appearance of the personal god" (p. 64) but only the long torturous development of such an idea which involved an internal revolution in one particular Axial religion (Judaism), so there is no constitutive term of inwardness in all Axial religion. Or at very least, that sense of inwardness that emerges with transcendence must be analytically distinguished from that inward revolution in Jewish law that Paul introduced to make of Judaism a universal religion.

Concentrating more on the terms in which the Deity is posited and less in the modes of overcoming or bridging the Axial chasm, Gauchet tends to incorporate too much into the specificity of Christian soteriology. General components of the Axial revolution are presented as specifically Christian (p. 143) while specifically Christian innovations are presented as concomitant with the very idea of transcendence. This then becomes a problem in properly grasping the dynamics of secularization, the progress of reason, and the logic of political representation.

Having said this, we must stress the originality and cogency of Gauchet's analysis as well as its convincing questioning of received categories. It is a book to be read and studied more than once, not only for its general thesis, but for its many insights, not least his insight into the progress of modernity from the internal transformations of institutionalized Christian belief.

The Kurdish Question and Turkey: An Example of a Trans-State Ethnic Conflict. By Kemal Kirisci and Gareth M. Winrow. London and Portland, OR: Frank Cass, 1997. 216p. $45.00 cloth, $25.00 paper.

The Kurds and the Future of Turkey. By Michael M. Gunter. New York: St. Martin's, 1997. 184p. $45.00.

Frank Tachau, *University of Illinois at Chicago*

The Kurds present an intriguing case study of frustrated nationalism. They are often cited as the largest ethnic group, at least in the Middle East, that has failed to gain statehood in the late twentieth century. Numbering 20 million or more, Kurds occupy an area that straddles the borders of Turkey, Iran, Iraq, Syria, Armenia, and Azerbaijan. They are thus an outstanding example of transstate ethnicity. Yet, this nation in formation, and especially its Turkish segment, which apparently accounts for the largest numbers, has not received much scholarly attention until quite recently. (The most frequently cited study is Martin van Bruinesesn, *Aga, Shaikh, and State: The Social and Political Structures of Kurdistan,* 1992, 1st ed. 1978). Many books dealing with the Kurds take the form of advocacy, sometimes in impassioned terms. In part this reflects the effect of the Kurds on Western publics, especially since the Gulf War, in the aftermath of which hundreds of thousands of them fled from Iraq into Iran and Turkey. Their plight was vividly recorded by the news media and triggered a hasty visit by U.S. Secretary of State James Baker. (Thanks to the marvels of modern technology, the visit lasted only seven minutes [*The Kurds,* p. 160] but was long enough for a TV interview and graphically symbolized American concern.)

Dispassionate scholars familiar with the Kurds are hard to find. As an organized Kurdish insurrection within Turkey drags on into its fifteenth year, however, this is beginning to change. A refreshing example is the first of the works under review.

Kemal Kirisci and Gareth Winrow, both of Bosphorus University in Istanbul, have produced a detailed analysis of the Kurdish conflict in Turkey. They begin by setting this case in the context of the general scholarly literature on nationalism and ethnicity, as well as against the historical background of the multiethnic Ottoman Empire. They compare the Kurds in Turkey with the Basques in Spain, noting that the comparison is weakened by the fact that the Basques occupy a highly industrialized and prosperous region, while the Kurds have historically lived in a remote, backward, and poverty-stricken area. The authors are also discomfited by the fact that, despite the apparent lack of material deprivation among the Basques, violence continues to characterize the most radical militants among them; this obviously does not provide a reassuring model for the Kurds, at least from the Turkish point of view.

Kirisci and Winrow devote an entire chapter to the concepts of minority rights and self-determination. Following a general discussion, they apply these concepts to the Kurds in Turkey, indicating the political changes that would be necessary within the Turkish political system, as well as obstacles that mitigate against this option.

The bulk of the book (five of eight chapters) is devoted to a critical appraisal of the Kurdish question in Turkey as it has evolved during the twentieth century, beginning with the post–World War I negotiations that determined the disposition of the territories remaining under the control of the Ottoman Empire. They describe the shifting and conflicting interests of Britain and France, the prostrate Ottoman government in Istanbul, and the newly rising national resistance movement led by Mustafa Kemal Pasha based in Anatolia.

Their depiction of the latter emphasizes its multiethnic character, principally comprising Turks and Kurds. The movement initially claimed to speak for the Muslim subjects of the virtually defunct Ottoman Empire; only gradually did its terminology shift toward a specifically Turkish ethnic mode. This much has been commonly acknowledged by scholars for decades. The precise role of the Kurds, the degree of their participation in the national resistance, and their attitudes have been less clear. Kirisci and Winrow suggest that a number of factors divided the Kurds, among them the survival of traditional tribal structures, continued adherence to religious tradition, and, consequently, opposition to the modernizing, centralizing, and secularizing policies of the Kemalist regime. As has been argued by other scholars, the various Kurdish uprisings against the Turkish Republic in the 1920s and 1930s are accordingly depicted as arising from mixed motives, ethnic consciousness being only one component among several and not necessarily always the strongest one.

Over time, Kirisci and Winrow argue, the Republican regime succeeded in establishing a politically stable secular, centralized, and modernizing regime. At the same time, in the 1930s, the regime increasingly emphasized an ethnic variant of nationalism rather than a more moderate form of "civic integration." This tendency, they propose, quoting a recent study by a Turkish colleague, heightened awareness of a distinct ethnic identity among the Kurdish masses. In other words, Kurdish nationalism arose as a reaction against Turkish nationalism. The Turkish government continued to accept "individuals with a good command of the Turkish language as 'Turks.' Their ethnic background was not important. The system had aimed to integrate many of these 'Turks' into the ruling elite. However, any expression of ethnicity—other than Turkish ethnicity—was perceived by officials in Ankara as a threat to the existence of the state itself" (*Kurdish Question* p. 113). This sensitivity toward any suggestion of even minimal recognition of Kurdish ethnicity or identity has remained the hallmark of Turkish government policy to this day.

Kurds form 10–15% of the population of Turkey. The exact number is difficult to pin down; Kirisci and Winrow suggest an estimate of approximately 6 million in 1992, or 10.7% of the total population of Turkey (Gunter, p. 2, estimates 20–25%). Apart from language, Kurds and Turks have been difficult to distinguish from one another. Traditionally, intermarriage has been widespread. Unlike the Arabs, who are also predominantly Muslim, the Kurds were slow to develop separatist nationalism.

Kurds are the largest ethnic minority in Turkey. They have historically been concentrated in the eastern and southeastern provinces (although extensive out-migration has significantly modified that pattern in recent years). Inhabiting the poorest parts of the country, adjacent to the Iraqi and Iranian borders, they have been close to ethnic brothers across those borders, with whom many of them have kinship ties. These kinsmen are caught up in struggles to attain or maintain a tenuous autonomy of their own, especially in northern Iraq.

The current conflict between the Kurds and the Turkish government is dominated by the ultraleftist Workers' Party of Kurdistan, or PKK. Well over 20,000 lives have been lost since the inception of an open insurrection led by the PKK, several thousand Kurdish villages have been evacuated and destroyed, and thousands if not millions of Kurds have migrated to Turkish cities in the western part of the country or abroad to Europe. A special legal regime comparable to martial law has been imposed on Turkey's southeastern region, under which normal civil rights are suspended, and freedom of information and communication is virtually nonexistent. The Turkish army has on several occasions crossed the Iraqi border to destroy PKK bases. Kurds elected to the Turkish parliament have been deprived of parliamentary immunity and have been tried, convicted, and imprisoned on charges of advocating and abetting "separatism," a charge akin to treason in the Turkish legal and political lexicon. Tentative conciliatory suggestions by leading politicians have failed to restrain the use of sharply repressive measures by the military. The conflict has imposed itself on Turkish foreign relations as well, providing justification for keeping Turkey out of the European Union and triggering congressional barriers to the acquisition of military hardware in the United States by the Turkish government, including items bought and paid for.

Kirisci and Winrow review these developments with remarkable candor and even courage, considering the political sensitivity of this issue in contemporary Turkish politics. Their conclusion is straightforward: The most effective way to end the conflict is to seek a politically negotiated settlement, specifically providing for rights of cultural autonomy, including the right to use the Kurdish language in the media and education. The fear that such concessions will inexorably lead to the collapse of the Turkish state, they argue, is grossly exaggerated. They are not optimistic, however, that the Turkish political elite will soon prove willing to agree to such a solution, given the weakness of contemporary parties and their leaders and the belligerent attitude of the powerful military hierarchy. They bluntly assert that reliance on military measures "is unlikely to reap benefits in the longer term," since "PKK units will always be able to evade the Turkish armed forces by finding sanctuary in the remote mountains of the east or in the territory of neighbouring states" (*Kurdish Question* p. 183).

Michael Gunter's account consists of a compendium of facts on the Kurdish uprising in Turkey and its effect on Turkish politics, particularly in the realm of foreign policy. It is largely based on sources drawn from the Foreign Broadcast Information Service of the U.S. government. Gunter considers himself an "objective" scholar, but unlike Kirisci and Winrow, he does not hide his sympathy for the Kurds. There are a few stabs at comparative analysis, but, again unlike Kirisci and Winrow, these are not well integrated into the analytical framework. Suggestions for alternative policies designed to bring a peaceful end to this largely invisible war focus on greater democratization of Turkey and recognition of Kurdish cultural rights, paralleling Kirisci and Winrow. Gunter goes one step farther, however, suggesting that general local autonomy—indeed, federation—is the only feasible solution. Yet, he acknowledges that only a charismatic military leader like Ataturk, de Gaulle, Rabin, or George Marshall (p. 135) would be able to overcome the military's insistence on the use of force.

In elaborating and analyzing the case of the Kurds in Turkey, these two books, each in its own way, provide potentially useful comparative data for the further study of ethnicity and nationalism in the contemporary world.

The Captive Republic: A History of Republicanism in Australia 1788–1996. By Mark McKenna. Cambridge, UK: Cambridge University Press, 1996. 334p. $64.95.

From Subject to Citizen: Australian Citizenship in the Twentieth Century. By Alastair Davidson. Cambridge, UK: Cambridge University Press, 1997. 341p. $64.95.

Educating Australia: Government, Economy and Citizen since 1960. By Simon Marginson. Cambridge, UK: Cambridge University Press, 1997. 286p. $59.95.

James Jupp, *Australian National University*

Australia was a pioneer in the development of mass democracy, with manhood suffrage from the late 1850s, votes for women instituted second only to New Zealand, and a variety of electoral systems designed to represent public opinion as effectively as possible. Yet, it maintains the anomalous and puzzling headship of state by a monarch who lives 12,000 miles away and, for most of whose time, is the head of state of the United Kingdom. As with Canada and New Zealand, this strange arrangement stems from the historical connection with the original homeland of the majority of earlier immigrants. But the economic links between Britain and Australia have become increasingly tenuous, the great majority of current immigrants do not come from Britain, and governments over the past two decades have been urging with various degrees of enthusiasm that Australia is part of Asia. The impending centenary of Australian federation is concentrating political minds on the anomalous monarchy. For the first time in two centuries, a clear majority of Australians now favors a republic. Even the present conservative government seems ready to end the historical link in conformity with the decisions of a partly elected constitutional convention held in Canberra in 1998.

These developments have spawned an increasing number of books on the republic, citizenship, federalism, and the constitution in general. The idea that these concerns were confined to a few academics and lawyers was dispelled by the wide popular interest in the convention. Apart from the issue of the monarchy, the Australian constitution has none of the rhetoric and inspiration of the American. It was primarily the result of negotiations by British colonies, which accepted without much questioning the British system of government, although with a more populist tinge than was true for Britain. The constitution has proved very difficult to amend through the required referendum procedure, and most Australians do not have a clear idea of its relatively mundane contents. Some scholars, such as Alastair Davidson, would clearly prefer to start all over again on the basis of modern ideas of citizenship rather than somewhat archaic concepts of the relative powers of units within a federal system. Many would favor the inclusion of a bill of rights or a preamble stating the nature of the society and the rights and obligations of citizens. A clear majority of public opinion wants an elected head of state, which could produce a massive lurch away from the British and toward the American model.

Until recently the very idea of becoming a republic was usually outside the bounds of political debate. That was not always so, however, as Mark McKenna skillfully outlines in his history of Australian republicanism. Radicals who wanted to assert local autonomy within the British Empire frequently expressed their hostility to the British connection. When Queen Victoria was unpopular in England, as in the 1860s and 1870s, many Australians were willing to argue that she should cease to be the focus for loyalty and patriotism. But this was all swept away by the enthusiasm for the empire, which reached its height in the Boer War and was sustained by World War I, when Australian loss of life was proportionately very high, though suffered on far distant battlefields. From 1918 until the 1960s it was political death to advocate a republic, and few did so.

McKenna is clearly a republican, but his basic argument is that Australians have been very ambivalent and sometimes indifferent to constitutional issues. Other matters were more important, especially the relations between labor and capital and the desire to exclude non-European immigrants. Now that half the immigrants are non-European and trade union membership has declined, other issues have become more important, including the nature of the political system. The obsequious monarchism of the postwar Menzies government has for long been a subject of ridicule. McKenna provides some amusing illustrations from official briefings on how to behave during the royal visit of 1954. But he rejects the idea that republicanism was mainly of Irish-Australian inspiration: "In an Australian context the Irish have supported the maintenance of the connection with Britain" (p. 211). As he details, most earlier republicans were born either in England or Scotland.

The most important turning point was the dismissal of the Whitlam Labor government by the governor-general in 1975. Queen Elizabeth remained firmly in her London bed at the height of the crisis. That an elected government could be overthrown by her representative certainly sharpened debate, even though the governor-general was Australian-born and had been appointed by the very prime minister he dismissed. McKenna places the end of the "colonial" relationship as early as 1963, but many others would prefer 1975. Either way, it has been all downhill for the past twenty years, despite residual monarchism among the Liberal and National parties currently ruling Australia and five of its six states. McKenna concludes that "the more substantial and potentially invigorating republicanism is that which seeks to move the republican debate beyond the familiar framework of monarch or president to encompass the essence of 'respublica' and the commonwealth—government for the common good" (p. 267). Similar themes are taken up by Davidson and Marginson.

McKenna's study is excellent political and ideological history. It shows that some arguments have been in use for 150 years, a remarkable continuity in a system little more than two centuries old. Perhaps he underestimates lingering nostalgia for British traditions in a country with more than one million British-born immigrants. Perhaps he plays down the Irish contribution to Australian radicalism, although he is quite right in seeing most Irish Catholics as basically conservative. The real test of republicanism will come when constitutional change is put to a referendum, as it must be. Australians have a remarkable history of voting against such changes. The current prime minister probably hopes that they will do so again. McKenna shows that republicanism was rarely a mass popular sentiment. It may yet be defeated by popular apathy or suspicion. Unless the political parties can agree on details, which is currently unlikely, Australia may still be a "monarchy" into the next millenium. This will not prevent the broader debates on national identity and active citienship that McKenna, Davidson, and Marginson want, any more than it does in Britain, Canada, or Sweden. But it will be an odd, and to many a disappointing, outcome.

McKenna writes clear, chronological, and polemical history, and his arguments are certainly easier to follow than those of Davidson. This is partly because Davidson is trying to do something more difficult, partly because Davidson uses a less rigorous framework, and partly because he is strongly influenced by European models, which have historically had little influence on Australia compared with those derived

from Britain or the United States. The intellectual context within which Davidson writes is thus less familiar and would certainly prove to be so to most people active in Australian public life over the past century. Unlike McKenna, he comes out of a Marxist tradition, which still colors his arguments, despite the peripheral influence of Australian Marxism throughout this century. Thus, he stresses the dilemma of those excluded from the political system for various reasons, particularly aborigines and immigrants. As the organized working class has taken a major role in Australian politics, he is less concerned about them. But an "excluded" element to which much attention was paid in the 1996 election—uneducated blue-collar workers—gets less attention than it deserves. Australian political leadership now rests to a greater extent than ever before in the hands of university-educated professionals. If Australia was ever a workingman's paradise, as many believed at the turn of the century, then it no longer is, at least in terms of political power.

Apart from being a monarchy, another historical anomaly is that Australia did not have legal citizenship distinct from the British until 1949. Davidson traces the development of a distinct citizenship based on an agreed national identity and then argues that much remains to be done. Formal citizenship is not the same as an active democratic citizenship. Public apathy is masked by compulsory voting in Australia, so it is often hard to know how interested or informed the voters are about political affairs. Davidson suggests that many of the excluded have been consciously repressed by the holders of power and that the task is to rearrange that power through a bill of rights and by an actively engaged citizenry. He draws extensively on debates in France and Italy and on the formulations of the European Union, none of which have had much effect on Australia. He also believes that there is a largely frustrated urge for greater participation in government, a view institutionalized in the recent past by public consultation and the creation of a multitude of advisory councils. Yet, the public sense of alienation remained, and revenge was taken in 1996 on the very government which had so greatly extended participation, however artificially.

Marginson's study is ostensibly narrower in focus: developments in Australian education over the past 35 years. He is scathing (as are most professional educators) about the victory of so-called economic rationalism in public policy. By this is meant the primacy of free-market ideology and the consequent emphasis on education as vocational and market driven. He responds more directly to the 1996 election reversal than do McKenna and Davidson, but he wants the same active, educated citizenry. He traces the rise of the New Right, with its influences from Thatcher's Britain and Reagan's America running counter to the previous Keynesian consensus, which had dominated governments for the previous thirty years. In his view, "education's role in producing and reproducing social hierarchy would increasingly overshadow its role in the constitution of universal citizenship" (p. 144). Marginson knows what he is talking about when it comes to education policy. But one might ask whether the traditions of public education in the past were not even more hierarchical and vocational than they are today. From this perspective, the rapid expansion and liberalization of education in the 1960s and 1970s seems like not only a lost golden age but also a transient phase.

These three books reach rather similar conclusions from quite different directions. Australia is presented as searching for its identity and destiny as a pioneer of popular democracy; having lost its way, it now has an opportunity to find itself again. This presupposes that an informed democracy has been frustrated by conservative institutions and attitudes and can be liberated by suitable political and constitutional changes. As McKenna's study shows, something similar has been urged throughout Australian history. But the drive usually peters out, and the masses sink back into a self-satisfied slumber in a normally peaceful and relatively harmonious society. Certainly, Australians are far better educated than ever before, far better informed through the media, and far more in touch with global trends. Yet, they remain just as likely to view politics as a game, just as likely to be unsure of who they are, and just as unlikely to organize themselves into active citizens, despite ample opportunity to do so.

The Fragmentation of Yugoslavia: Nationalism in a Multinational State. By Aleksandar Pavković. New York: St. Martin's, 1997. 222p. $59.95.

Sabrina P. Ramet, *University of Washington*

The Preface promises "to avoid a discourse of moral indignation and condemnation" (p. x) in discussing the 1991–95 war in former Yugoslavia and its prelude. The author also asserts, on the same page, that he finds "nothing appealing in any national ideology, however romantic some of them may appear to intellectuals." Leaving aside the question of whether nationalism holds attraction only for intellectuals—a dubious implication that the author may not have intended—these prefatory remarks incline one to expect a balanced treatment in this book.

What follows is, however, disappointing. The first sixty pages are devoted to 1918–53. Although the author maintains a scholarly tone in these pages, there are already warning signs, such as the omission of any discussion of Chetnik leader Draža Mihailović's collaboration with the Nazis during World War II or of the collaborationist activities of Serb quisling Milan Nedić or Serb fascist Dimitrije Ljotić.

Another sixty pages are devoted to 1953–90. While the reader may obtain a handy summary of events from this section, it is probably not coincidental that Pavković, in referring to the respective aspirations of Croatian and Serbian nationalists in the early 1970s, writes that Croatian leaders Miko Tripalo and Savka Dabčević-Kučar "argued" certain positions (p. 67), while Belgrade legal theorist Djurić "noted" certain facts (p. 71). Implicitly, the reader is already given to understand that the latter's demands were reasonable, while those of the former evidently were not.

In the final section of the book, covering 1990–95, the author's agenda comes into clear focus. At the start of chapter 9 (p. 122), Pavković traces the emerging tensions in Yugoslavia to the alleged fact that "the new Croatian and Slovenian governments were committed to extricating their republics from the Yugoslav federation either by transforming the federation into an alliance of sovereign states, or by unilaterally declaring independence." Left out is the fact that the Croats and Slovenes made this "commitment" in desperation, after Milošević had extirpated the two autonomous provinces but pocketed their votes in the state presidency, overthrown the elected government of Montenegro in a putsch, and begun to demand that Slovenia and Croatia submit to a revision of the federal constitution already drafted in Belgrade. Thus, in Pavković's revisionist history, it was not Milošević who took the country to war, but Slovenia and Croatia!

The bias continues throughout the remainder of the book. Thus, the reader is told of the Croatian government's instructions to the Croatian media but not of the way in which

Milošević's control of television left many Serbian viewers suspecting, during the siege of Sarajevo, that it was Croatian "fascists" and Muslim "fundamentalists" who were shelling the innocent Serbs trapped inside the city. A commitment to balance could have been demonstrated by including this side of the story as well.

In the Bosnian chapter, the author claims that all three sides were arming in 1991–92 but says nothing of the fact that the Bosnian Serbs started arming first and by early 1992 were vastly better equipped than Bosnian Croats and Muslims combined. He implies (p. 167) that the UN declaration of "safe havens" was an anti-Serbian step. In his recollection of the war, moreover, there are only Serbian refugees, and the Serbian side engaged in "self-defense." No Croat or Muslim refugees are mentioned, but there are "Croatian mobs" (e.g., p. 186); I could find no references to "Serbian mobs." Furthermore, when the Serbs reject a peace plan, Pavković expresses his understanding for their concerns; when the Muslims reject a peace plan, he expresses outrage. He even advises the reader in connection with photos taken at the Serb detention camp at Trnopolje that "not all prisoners pictured were starving" (p. 171). Is the reader expected to conclude from this that there was no harm done at Trnopolje? The list goes on and on. In a nutshell, Pavković has produced an oddly one-sided book reflecting the standard Serb nationalist line.

The index omits the names of a number of persons mentioned in the book, such as Tito, although Tudjman, predictably, is amply represented there.

Through Corridors of Power: Institutions and Civil-Military Relations in Argentina. By David Pion-Berlin. University Park, PA: Penn State University Press, 1997. 241p. $45.00 cloth, $17.95 paper.

Incomplete Transition: Military Power and Democracy in Argentina. By J. Patrice McSherry. New York: St. Martin's, 1997. 408p. $45.00.

Paul W. Zagorski, *Pittsburgh State University*

At a certain level, *Through Corridors of Power* and *Incomplete Transition* might be considered complementary. They cover approximately the same period of Argentine civil-military relations (the 1980s and early 1990s) but, with one major exception, focus on quite different topics. While Pion-Berlin concentrates on the issues of rights trials, civilian control of military policy, and the military budget, McSherry is principally concerned with revanchist factions within the armed forces and their attempts to reestablish a domestic—specifically, an internal security—mission for the military. A well-rounded picture of recent civil-military relations in Argentina requires an understanding of the varying positions of the Alfonsin (1983–89) and Menem (1989–) governments on military issues, and this is Pion-Berlin's focus. Yet, the picture would not be complete without an understanding of military reactions to civilian attempts to control the armed forces or an appreciation of the reaction of civil society to the maneuvers of both politicians and military men. This is where McSherry presents the more detailed account.

The events surrounding the human rights trials in the mid-1980s are so obviously important that both books cover them extensively. The general outline of what happened during this period is not a matter of dispute. Although Raúl Alfonsin ran as the presidential candidate most likely to bring the armed forces to book for the up to 30,000 "disappearances" during the "dirty war," he never fulfilled popular expectations. Even his early moves were crafted to prod the military into self-purification and limited the number of those subject to prosecution. Yet, despite these limited objectives, the military delayed and resisted, and three open revolts in 1987–88 caused the government to backpeddle repeatedly. Eventually, under the Menem administration, the rights violators and those who had rebelled (except participants in the 1990 coup against Menem himself) were given amnesty. The difference in the accounts provided by Pion-Berlin and McSherry lies less in the general history or even the details of the revolts than in their implications and interpretations. The former concludes that the failure of the revolts ushered in a new period of civil-military relations: "The social connections that military conspirators had come to rely upon and had drawn sustenance from were now severed" (*Corridors*, p. 74). The latter describes the revolts as "low intensity coups" that succeeded in imposing "a sort of slow-motion pact or imposition from above" (*Transition*, pp. 226, 229).

It is not surprising, then, that the authors disagree on other important issues as well. Pion-Berlin sees the 1982–83 transition to democracy after the debacle of the Falklands/Malvinas War as a regime collapse (*Corridors*, p. 7), while McSherry argues that it lies somewhere between a "transition by extraction" (in which the armed forces abandon the rules of the military regime but negotiate the transfer of power) and "transition by reform" (in which the rules established by the military regime remain in place) (*Transition*, p. 9). "A central argument of this study," McSherry writes, "is that the volatile civil-military conflicts of the 1980s are best understood as clashes between democratizing sectors and recalcitrant elements of the military security apparatus . . . in a struggle to define the nature of Argentina's emerging democracy" (*Transition*, pp. 3–4). He goes on to paint a picture of a recrudescent military that successfully reclaimed its core prerogatives and regained political power (*Transition*, p. 7).

Pion-Berlin portrays the armed forces as severely factionalized. He describes the failure of both civilian reformers and uniformed officers to modernize the institution successfully, and he analyzes in detail the conditions that led to budget stringency for the armed forces under both Alfonsin and Menem. To simplify, under Alfonsin low military budgets were largely the government's reaction to persistent armed forces resistance over the rights trials. Under Menem, the military suffered along with other state-funded sectors. Surprisingly, neither the negative policy of "starving the elephant" nor efforts to rethink the mission of the Argentine armed forces in a changing world produced systematic military reform under either government. Units were shifted about; the intake of conscripts and overall strength fell; the purchasing power of officers' salaries declined; and combat readiness plummeted. Yet, old structures and habits of mind remained. While Pion-Berlin addresses the topic of military reform in detail, he pays little attention to a central topic of McSherry's account: military and civilian efforts to establish policies and procedures that would allow the armed forces to intervene in situations of domestic unrest. He does not view these efforts as indicative of a resurgent national security doctrine. Thus, the two books present quite different assessments of military politics in Argentina.

This difference is, in part, foreordained by methodological and philosophical differences. Pion-Berlin takes a self-described combination of a "new institutionalist" and "rational choice" approach (*Corridors*, p. 22). He argues that the national security doctrines—the military theories that sanctioned military coups and the Argentine military's "dirty war" against subversion—are in disrepute (*Corridors*, p. 18). One of his major concerns is for the consolidation of democracy, that is, giving the military's apparently "utilitari-

an" acceptance of subordination to civilian authorities an ethical foundation (*Corridors*, p. 16). Beyond this, he is concerned about building the institutions that will make civilian control truly effective. In contrast, McSherry takes an approach that is both "historical-structural" and "institutional." His overriding concern is the "quality of democracy, rather than its formal procedural elements" (*Transitions*, p. v). And the armed forces he describes are, in fact, not subordinate to civilians, even on utilitarian grounds. As a structuralist, McSherry is much more prone to assert a stronger connection between the most hardline sectors of the armed forces and the interests of the institution as a whole than Pion-Berlin would seem to admit. And, for a similar reason, McSherry tends to emphasize the link between the interests of the military and international capital beyond what the evidence warrants.

The strongest point in McSherry's brief is the united opposition of the armed forces to rights trials. Yet, this unity can be explained on other than structuralist grounds. Given the widespread participation of officers in rights violations, almost the entire officer corps had legal and psychological reasons for opposing the trials, but their unity was both limited and fragile. Even the three military revolts under Alfonsín were directed against the prosecution of officers and did not represent attempts to seize political power. As an Argentine journalist once remarked to me, the revolts represented "trade union actions." Moreover, these rebellions split the armed forces themselves and resonated favorably only with the most extreme and marginal sectors of the Argentine right. And the coup attempt of 1990, which did have grand political objectives, was easily put down.

The armed forces of Argentina are notoriously difficult for outsiders to penetrate and read accurately in any detail. Future historians may well discover clear evidence of the sort of connections among the military rebels, the institution as a whole, and the designs of international and big domestic capital that play a prominent role in McSherry's account. Until then, it is probably prudent to remain cautious about the true scope of such conspiracies. Thus, while both Pion-Berlin and McSherry contribute to our knowledge of Argentina's civil-military relations, the more nuanced presentation of the former is less likely to mislead those who are unfamiliar with the subject.

Redefining Mexican "Security": Society, State, and Region under NAFTA. By James F. Rochlin. Boulder, CO: Lynne Rienner, 1997. 217p. $49.95.

Maria Lorena Cook, *Cornell University*

Observers of Mexican politics may have longed for a study that joins under a single explanatory framework the diverse problems that have afflicted Mexico since NAFTA's passage: peasant uprisings, heightened crime, economic crisis, political assassinations, militarization, and drug trafficking. James Rochlin's brief but ambitious book tries to do just that by redefining these and other developments as dimensions of Mexican "security" that have emerged in a new, post-NAFTA environment. The author's innovative treatment of security concerns as those that transcend traditional conceptions of military surveillance of national borders or domestic counterinsurgency is certainly welcome. It is also intriguing to explore how new notions of regionalism under NAFTA may redefine security issues. But despite the promise of Rochlin's idea, the cluttered presentation and confused analysis left this reader disappointed rather than enlightened.

The key argument in *Redefining Mexican "Security"* is that the rise of "neo-liberalism" after 1982 contributed to the erosion of the legitimating myth of Mexican politics, revolutionary nationalism, which in turn forced the state to adopt more coercive measures against its own population. This loss of hegemony of the Mexican state is presented alongside the new disruptive forces exacerbated by regional economic and military integration under NAFTA, such as immigration, drug trafficking, increased militarization, the Chiapas peasant uprising, environmental disasters, and increased human rights violations. The author acknowledges that positive forces, such as the emergence of nongovernmental organizations and transnational networks, are also part of this new regional setting. The net effect on Mexican civil society, however, is to render it fragmented and incapable of articulating a counterhegemonic alternative of politics and economic integration. Rochlin argues that state definitions of security problems and societal definitions, which view security more in terms of the satisfaction of basic needs and quality of life, have become increasingly counterposed. Successful economic integration, he concludes, will only come about once these two conceptions are fused.

Unfortunately, the story lacks focus and conceptual clarity. The category of "security" is so varied and broad as to render the term meaningless. For instance, environmental degradation, human rights violations, neoliberalism, illegal immigration, drug trafficking, the plight of Mexico's indigenous minority, and an incomplete transition to democracy are all discussed here as if they were manifestations of the same phenomenon. Yet, their precise causes and interrelationships are left undefined. At the very least, illegal immigration, environmental decay, human rights abuses, and the impoverishment of Indians predate 1982, the year Rochlin identifies as the beginning of the decline of Mexican political stability. Even if these developments have been made worse by NAFTA, the book sorely needs a more nuanced analysis of how and why this is so.

The mostly descriptive chapters contain oversimplifications and some annoying mistakes. For example, *maquila* is misspelled "maquilla" throughout the book, and the decline of the PRI is directly attributed to a rather abstract "advent of neoliberalism" after 1982 (p. 24), whereas a more accurate and actor-centered view might have given political opposition forces some credit. The author gives a great deal of weight to the role of "legitimating myths" in the erosion of political stability, at the cost of explaining how specific economic and political developments have undermined support for the PRI and contributed to an increase in security problems. Indeed, the too-frequent references to the decline of revolutionary nationalism make the author seem nostalgic for the "good old days" of import substitution and PRI hegemony. Never mind that those days were marked by electoral fraud, limited citizen participation, and restricted public debate.

Despite the author's claim to offer a novel perspective, reflected in part by the postmodern phrases that litter the book (e.g., "the time/space compression" in reference to technologies such as the Internet), his analysis is surprisingly conventional. Rochlin attributes much of the current crises to the decline of PRI hegemony and the compensating increase of coercive state power, a traditional top-down view of Mexican politics. By comparison, societal actors play a derivative role. Rochlin downplays the important achievements of Mexican civil society in recent years: the growth of a unified leftist party; electoral reforms, including independent oversight of the electoral process; the emergence of a more active and independent print and electronic media; a vast network of nongovernmental organizations; the role of citizens in seeking a peaceful solution in Chiapas; and so on. We are left

without a better understanding of how these more positive developments affect and coexist with the very real problems Rochlin has identified.

We are also left without a clear sense of what is needed to ameliorate these problems in Mexico and of how that change might come about. The conclusion is not only vague but also remarkably moderate. The "fusion between state and societal security" can be achieved with "a functional economic system paired with legitimate political structures" (p. 187). There is no reason to believe that the author is referring to anything other than a capitalist democracy. It is puzzling, therefore, that Rochlin should call this a "fairy-tale scenario of major structural reform" (p. 187), given the modesty of the prescription as well as recent advances in Mexican economic growth and democratization. What is needed, Rochlin says in the final paragraph of the book, is "good government and . . . policies that generate social consent" (p. 190). Unfortunately, the book does not move us beyond this self-evident conclusion to a better appreciation of what such policies should be.

Democracy in Asia. Edited by Michèle Schmiegelow. Frankfurt: Campus Verlag; New York: St. Martin's, 1997. 555p. $55.00.

James Cotton, *University of New South Wales*

The views of Michèle Schmiegelow on the way democracy in Asia should be understood are clear. In the lengthy Introduction she argues that the trend to democratization is a major phenomenon of our time, and while economic development may have been one cause, cultural forces have also been important. The power and example of Western democracy have played their parts, but the compatibility of democratic values with elements of the traditional cultures of Asia has helped spread democratic standards across the region. This latter point seems to indicate the way out of the impasse of cultural relativism, which Schmiegelow finds implicit in the "Asian values" debate. While the relativism presupposed in the clash of civilizations scenario of Samuel Huntington seems to be reinforced by the rise of fundamentalist movements of various kinds, Schmiegelow is of the view that critical rationalism, informed by insights to be derived from across all cultures, can nevertheless provide a convincing grounding for a preference for democracy. Democracy should be understood not merely in the limited manner of Schumpeter, that is, as a method of appointing governing elites, but also should incorporate normative perspectives.

This book originated in a conference convened in Louvain in 1994. The authors of the 23 chapters are drawn from the international scholarly community and were evidently given some latitude in addressing the general theme. The contributions are arranged in two broad sections, one on religious and philosophical foundations for democracy, one on the historical record of democratization in a range of Asian political systems. Although many authors address some or other of the editor's concerns, for the most part they do not seem to follow the single research program that Schmiegelow's views might have proscribed; some have been content to follow their own interests, and others have disagreed explicitly with some or all of her propositions. There are some gems of historical and institutional analysis—Gilbert Rozman on Confucianism in China and Japan and Greg Barton on trends in Indonesian Islamic thought are especially noteworthy—but there are also some examples of obscurity and even irrelevance.

The general criticism that might be made of the material is that there is far too much historical detail. Brigitte Piquard offers some interesting observations on the inapplicability of such antinomies as "tradition and modernity" and "religion and secularism" in the political evolution of Pakistan, but much of her 34 pages are concerned with a political chronology. Philippe Regnier's comparison of South Korea and Taiwan devotes most attention to the period prior to the last ten years and fails, perhaps for that reason, to capture the considerable differences in their respective patterns of democratization, a point stressed by Huntington and other students of the third wave. The chapter by Junko Kato on party factionalism in Japan and Italy, though workmanlike, would have been more at home in a collection on comparative party systems.

There are some cases of judgments that are eccentric if not obtuse. Thus, in the only contribution that ranges over the whole terrain, Seizaburo Sato offers the extraordinary ad hoc hypothesis that Singapore's advanced level of development has not fostered further democratization because "it must be that Singapore happens to be of a size which is small enough to permit an effective control of the whole society by one leader as long as he provides an exceptionally capable leadership" (p. 89). And the editor herself sees Malaysia as most emphatically a participant in democracy's third wave as a result of the outcome of the 1995 national elections (p. 38), whereas many commentators have offered quite a different assessment in the light of the decimation of the parliamentary opposition at that election. A little less controversial is the claim, made by Raoul Jennar, that Cambodia's unhappy experience with political competition following the UN-sponsored elections has been the outcome of the practice, by the "Anglo-Saxons," of "a sort of puritanism or democratic fundamentalism" (p. 518) that made no allowance for Cambodia's past or existing state of political consciousness. Finally, though Kingsley de Silva presents a defensible argument on the nature of the continuing communal conflict in Sri Lanka, the 34 notes include 20 with references to other works by the same author, which must be a record in self-citation.

Thanks to the generosity of the Japan Foundation, this diverse material has been made available to the academic community in a single volume. In general it conforms to the standard set by most of its genre, but what makes it somewhat unusual is the format of the editor's concluding chapter. This incorporates sixteen "propositions" that summarize the themes of the book and represent "plausible paths of cognition and action" (p. 534). These range widely over issues in the philosophy of the social sciences but generally defend a multicausal account of democratization that rejects cultural relativism and affirms the normative understanding of democratic practices. Although the propositions are the editor's responsibility, they apparently follow an extensive postconference correspondence with the contributors and thus represent something of a consensus. They are illustrated in a chart (Figure 1, p. 538) "consciously designed to resemble a circuit diagram of a fuzzy logic rice cooker" (p. 533), which may induce puzzlement in some readers.

The Soviet Experiment: Russia, the USSR, and the Successor States. By Ronald Grigor Suny. New York: Oxford University Press, 1998. 540p. $35.00.

Eugene Huskey, *Stetson University*

"Stalinist culture established what was acceptable or possible, but within that permissible range art could provide emotional and aesthetic pleasure, bring people to tears or laughter, and make them feel good about the lives they lived" (p. 274). One could ban saxophones—as the Soviet Union did in the late

1940s—but not the spirit that leads men and women to make music. In these and other observations in *The Soviet Experiment*, Ronald Grigor Suny reveals the humanity of a population subjected to a century of lawlessness and indignity. To be sure, episodes in Soviet history, such as those recounted in Varlam Shalamov's chilling tales of the Kolyma death camps, could destroy the spirit as well as the lives of individuals. But Russian society exhibited a remarkable resilience and instinct for survival in the face of the relentless campaigns of a messianic state.

If the Soviet experiment bequeathed at least some "usable history" with which to build a new, postcommunist culture and society, it could contribute little of value to a revival of politics in the 1990s. Unlike in Latin America and southern Europe, the transition from authoritarianism in Russia and the other post-Soviet states requires the razing of the entire edifice of political and economic relations. During the last decade, reform-minded Russian leaders have found it immensely difficult to dispose of a history conditioned by a command economy, by the Communist Party's monopoly of political organization and discourse, and by habits of the heart that discourage trust and personal responsibility. Although the author does not explicitly link the past to the present—this is, in that sense, a traditional history text—there is in *The Soviet Experiment* a wealth of material to be mined by those who seek to understand how the burden of Soviet history weighs on transition politics in Russia.

Writing in the tradition of historians such as Moshe Lenin, Suny presents an interpretation of the Soviet legacy that gives as much emphasis to circumstances and human and economic resources as to ideology and elite tactics. It is not history from above or below but history from all over. Making excellent use of the secondary literature on the Soviet era, including some works grounded in recently released archival material, Suny weaves a lucid and convincing narrative across topics as diverse as high politics, agricultural policy, diplomacy, and film. It is a comprehensive, in places almost encyclopedic, history of the USSR. For some, the treatment of Lenin may appear insufficiently critical. Suny takes seriously the prerevolutionary writings of the Bolshevik leader that anticipate the establishment of an egalitarian commune-state. But once in power, Suny writes, "Lenin pulled away from his vision of radical democracy" (p. 124), in part because of the circumstances of a deepening domestic crisis and the receding prospects for a revolution in Europe. Whatever one's view of the relationship between Lenin as writer and Lenin as ruler, there is here a strikingly even-handed presentation of the competing Western interpretations of Soviet political and economic development.

Where the author exhibits a clear preference is in the disproportionate attention accorded the early decades of Soviet rule. The book devotes 350 pages to the first 36 years of the Soviet era—from the revolutions of 1917 to the death of Stalin in 1953—and only 60 pages to the following 32 years, which takes the story to Gorbachev's accession to power. Without question, the years under Lenin and Stalin laid the foundations of the Soviet order. But in a book of more than 500 pages, one would expect a more extended analysis of the Khrushchev and Brezhnev eras, when the Soviet Union came of age. It was during this period that Soviet leaders struggled to satisfy the demands of a more sophisticated society without abandoning the principles of state socialism. A similarly brief treatment of the Yeltsin years leads the author at times into oversimplified and hurried accounts of key events, such as the violent confrontation between parliament and president in October 1993. To correct a minor error in the final section of the book, the Duma has 450, not 550, members (p. 502).

One reason for the underweighting of the last half of the Soviet era is undoubtedly the relative paucity of serious historical research on Soviet rule under Khrushchev and Brezhnev. While graduate students in the last two decades have flocked to the interwar years, encouraged by advisors who are themselves specialists in this period, few have tackled subjects in the postwar era. Indeed, the years between 1945 and 1985 represent a kind of academic no-man's land, falling between the interests of political science and history. For historians, the problem lies as much in the poverty of sources as in the chronological proximity. For all the horrors and brutality of the decades between the wars in the USSR, there were still lively and revealing debates on the pages of contemporary journals. The range of ideas and emotion contained in published materials were far narrower in the decades after World War II. Thus, conducting serious research on most postwar topics requires access to the archives, which have only recently been opened to foreigners.

The Soviet Experiment is a lucid and accessible introduction to the staggering complexity and drama of communist rule in the Soviet Union. Even specialists in the field will find here new detail and insights, especially on long-neglected ethnic issues and on Soviet rule in the Caucasus, the region on which the author is the West's leading scholar. But one may ask whether the end of the twentieth century is, as the author argues, "the right moment to tell the story of the Soviet Union as a whole." Yes, the Soviet Union now has "a beginning, a middle, and an end" (p. xiii). However, it will almost certainly be another generation before we have a mature literature on the last decades of Soviet rule and before we begin to understand how the Soviet experiment fits into the longer sweep of Russian and world history. When and how Russia emerges from its most recent time of troubles will shape our perceptions of Soviet history and our sense of the questions that should be asked about it.

Economic Strategy and the Labour Party: Politics and Policy-Making, 1970–83. By Mark Wickham-Jones. New York: St. Martin's, 1996. 294p. $65.00.

Mark D. Harmon, *University of California–Santa Cruz*

From 1973 until 1983, the British Labour Party articulated and advocated a series of radical economic policy proposals that together became known as the Alternative Economic Strategy (AES). The AES consisted of six interrelated policy planks that were presented "as an economic strategy aimed at regenerating the British economy and providing the basis for a society built on socialist values" (p. 53). These proposals included the reflation of demand, a substantial expansion of public ownership, formal and compulsory economic planning with privately owned firms, price controls, industrial democracy as part of a strategy to transfer power from private firms, and (after 1975) import controls to prevent a recurrence of balance-of-payments crises and to protect particular industrial sectors.

Wickham-Jones provides the first book-length treatment of the AES—its intellectual origins, the politics within the Labour Party that surrounded it, its consequences for the party, and more broadly what the AES can tell us about the prospects for social democracy. While a good deal of excellent work has been done in recent years on Labour Party politics in the 1970s and 1980s, on constitutional reform, on factionalism, on the rise and eclipse of the Labour Left, and so on, this book is distinct in its focus on the AES as the primary object of study. For a variety of reasons, Wickham-Jones argues, the AES is important: "It marked a departure

for the Labour Party [and]... represented a fundamentally different economic strategy based on a new theory with fresh objectives distinct from its predecessors" (p. 6). It is relevant to any analysis of the Labour Party's social democratic commitments, it is indicative of the kind of party organization that developed in Labour after 1970, and it is important because its adoption had significant consequences for Labour (pp. 6–7).

The overall argument has several parts. After demonstrating that the AES constituted "a qualitative break with Labour's Revisionist past" (p. 7), Wickham-Jones argues that "whatever the formal content of party policy documents, most of Labour's leaders did not reconcile themselves to the AES... I demonstrate that the institutional character of Labour and its pluralist nature were important in allowing this divergence to occur... [and] that the disunity which became endemic in Labour with the failure of its leaders to accept party policy was the chief cause of the left's demands to reform the constitutional nature of the party [after 1979]... I conclude that any analysis of Labour's decline must examine the internal determinants of the party's crisis as well [as] the external causes" (pp. 7–8).

The Labour Party's policymaking structure during this period is crucial to Wickham-Jones's account of how the AES emerged. Because of the generally acknowledged economic policy disappointments of the 1964–70 Labour government, led by Harold Wilson, the party when it went into opposition took on the task of rethinking its revisionist/Croslandite policy orientation of the 1950s and 1960s (see Anthony Crosland, *The Future of Socialism*, 1956). Policymaking subcommittees were established that "became a direct part of the policy process" (p. 120). The Labour Left was able to dominate these committees and to pull party policies in a more radical direction. While Tony Benn became the most outspoken public advocate for the AES from the mid-1970s onward, it was most notably Stuart Holland, an economist at Sussex University, who provided the theoretical work on Labour's policymaking committees that underpinned the AES, especially on the public ownership and economic planning proposals (see Stuart Holland, *The Socialist Challenge*, 1975). The Labour Right was outnumbered and, in any event, tended to be marginalized because of its support for Britain's Common Market membership (the official Labour position in the early 1970s was to oppose joining Europe).

Also important to Wickham-Jones's argument is the divergence in views between the Left-dominated policy committees in the party and the more revisionist Labour leadership in the Parliamentary Labour Party (PLP). Because of Labour's organizational structure and broader trends within the party, the PLP was unable to dominate the development of party policy. Given that the PLP was generally disapproving of the leftward drift of policy during this period and that party activists were intensely suspicious and distrustful of Labour's leadership in Parliament, the two were frequently at each other's throats. (Labour Chancellor of the Exchequer Denis Healey once quipped that party leftists were "out of their tiny Chinese minds.") There was sharp conflict over the 1973 commitment in *Labour's Programme 1973* to nationalize "around 25 of the top 100 firms" (p. 63). When Labour was in power from 1974–79, there were repeated party conference resolutions critical of the government for its failure to carry out party policies (pp. 150–4). When Labour went into opposition again in 1979, the sense of betrayal felt by the party vis-à-vis the PLP was a primary cause of the bitter and fratricidal internal debate that followed over the party's organizational structure; "the overriding reason for reform, leftwingers argued, was the need to render the party leadership accountable" (p. 158). Wickham-Jones argues that this internal party conflict "did great electoral damage to Labour.... The bitterness and factionalism meant that Labour ceased to present a credible image to voters" (p. 204). The most damaging split of all occurred in 1981, when prominent right-of-center Labour MPs and former ministers left to form the Social Democratic Party and Labour's position as the main opposition party to the Conservatives was threatened.

Wickham-Jones has conducted interviews, made ample use of the relevant memoirs and secondary literature, and carefully sifted through the research papers and minutes of committee meetings in the Labour Party's files to put together his argument. His judgments about the development and consequences of the AES are usually fair and astute. That said, I found two weaknesses that undermine the overall effort.

First, Wickham-Jones accepts the point that the AES on its own merits was a viable economic strategy for a modern industrial economy and that Labour's failure to implement the AES while in power was primarily because the party was disunited (p. 206) rather than because the AES was impractical or perhaps even unworkable. He argues that "the AES would have required adaptation, but a reformed yet radical version might have been the basis for a feasible economic policy" (p. 197). I am more skeptical regarding the potential of the main elements of the AES—expansion of public ownership, compulsory planning agreements, import and price controls, and so on—to foster dynamic conditions of economic growth in an increasingly globalized economy. Wickham-Jones is overly generous with respect to the AES when he concludes that the question of whether it met its objectives remains "unanswered" (p. 197).

Second, because of the organizational structure of the argument, the broader economic environment confronted by the Wilson/Callaghan governments in the 1970s is usually discussed apart from the AES and seems underemphasized within the narrative. For example, in Wickham-Jones's discussion of the party proposal that was passed by the 1976 Labour Party conference to nationalize the big four clearing banks, seven large insurance companies, and one merchant bank (p. 65), no mention is made that the government at the same time was announcing its application for a conditional IMF load in order to cope with the sustained loss of investor confidence made manifest in the steady slide in the sterling exchange rate. The broader economic environment was extraordinarily hostile, and it is important to keep in mind when discussing the 1974–79 Labour government's failure to implement the AES that this was a period of high inflation (more than 20% in early 1975), repeated and heavy selling pressure on sterling, a difficult and painful conditional loan from the IMF, problematic annual negotiations with the trade unions over incomes policies, and finally an explosion of industrial relations conflict that shattered the government's assertion that it could effectively manage the unions. Attempting to implement AES policies would have been difficult under the best of economic conditions, and the 1970s were far from the best.

These more critical points notwithstanding, this book should be of interest to specialists in British politics and in social democracy and, more generally, to anyone interested in the politics of the Left in advanced industrial societies. It is carefully researched, engagingly argued, and concerns an important period in Labour Party history that we are unlikely to see again.

International Relations

War and the Red Cross: The Unspoken Mission. By Nicholas O. Berry. New York: St. Martin's, 1997. 159p. $35.00 cloth.

Larry Minear, *Brown University*

The central issue in today's widely ranging but still jumbled debate about humanitarianism involves its relationship to politics. The debate engages the UN Security Council and General Assembly, UN member states, parliaments and administrations of donor and recipient governments, the concerned international public and the media, and, of course, humanitarian agencies themselves.

Some aid organizations, with the International Committee of the Red Cross (ICRC) in the vanguard, embrace the classical principles of neutrality and impartiality. Life-saving assistance and protection, in their view, should be provided to persons in ways thoroughly divorced from any and all political objectives and conditionalities and solely in proportion to the severity of need. The ICRC acknowledges that its aid work on the ground and its advocacy of humanitarian imperatives at the local and international political levels have political effects and implications, yet it does not see itself as a political actor as such. It keeps its distance from efforts to use humanitarian aid to achieve political objectives, however supportable they may be in their own right. If emergency aid can be an instrument for healing tensions, then well and good, but that should not be its purpose, and it should not be withheld in exchange for better behavior among belligerents.

Other humanitarian organizations are, by their own terms of reference and/or by their scripts, directly engaged in the political sphere. Although working to provide assistance in proportion to need, they are not neutral with respect to belligerents, some of which have more legitimate claims than others, or with respect to war, a plague against humanity. Indeed, such groups, including some nongovernmental organizations (NGOs) and some UN agencies, affirm an obligation that entails explicitly political tasks. They seek not only to ameliorate but also to prevent suffering by addressing its underlying causes: ethnic tensions, racism, conflicts over resources, warlordism, criminality, and, above all, war and injustice. Such groups, with increasing support and pressure from the UN and governments, situate their emergency relief and protection activities squarely within the broader ambit of conflict prevention and conflict resolution. Their work is part and parcel of the UN's peace agenda and of international efforts to bring violators of international law to justice and to rebuild war-torn societies along democratic and participatory lines.

In *War and the Red Cross: The Unspoken Mission,* Nicholas Berry positions the ICRC, the most omnipresent source of succor in today's complex emergencies, with a foot in both camps. Berry portrays the ICRC as continuing to embrace, as it has since its inception, impartiality and neutrality in assisting the victims of war without regard to ideological considerations. At the same time, however, he is convinced that in recent years the ICRC has been engaged in a secret and highly political mission: the abolition of war. He bases his contention on ICRC documents and numerous interviews with ICRC officials, quoted at great length. He finds further evidence of the secret mission in his review of the organization's activities in conflicts in former Yugoslavia, Rwanda, the Sudan, Afghanistan, and Guatemala.

While presented with the flair of a mystery writer, Berry's thesis and documentation are flawed. His central hypothesis fails to distinguish between political objectives, which the ICRC disavows, and political effects, which it acknowledges. Berry is correct in observing that all humanitarian actors and activities in today's complex emergencies have political effects, although these are often soft-pedaled by the aid agencies themselves. These include, as he enumerates them, monitoring—and thereby deterring—the levels of violence, helping to stalemate wars, serving as neutral intermediaries between the parties, and pressing for intervention by the UN and outside governments. Yet, to construe such activities as evidence of an "unspoken new mission [that] is nothing less than the sabotage of war" is not borne out by either ICRC theory or practice.

Berry is surely also correct in delineating the close connection between the institution of war and the work of the ICRC in assisting and protecting its victims. "Humanitarian intervention," he rightly notes, "upsets the natural flow of war," although he goes too far in suggesting that such intervention takes war "out of the control of the warring parties," as those who manage Operation Lifeline Sudan or other such interventions will attest. The sum total of activities by the ICRC and other international humanitarian agencies does indeed make it harder to wage no-holds-barred war. As custodian of the Geneva Conventions and Protocols and promoter and disseminator of international humanitarian law, the ICRC is itself at the forefront of global efforts to humanize—that is, to delimit—war making. "War was the making of the Red Cross," observes Michael Ignatieff in his own study of these issues, "and it is at the heart of its complexity as an institution that war remains its reason for being" (*The Warrior's Honor: Ethnic War and the Modern Conscience* [New York: Metropolitan Books, 1997], p. 119).

Yet, however much ICRC activities may constrain belligerents and however strongly individual ICRC staffers may wish to end today's uncivil civil wars, the organization itself is not committed, even implicitly, to the abolition of war itself. Were that the case, its humanitarian activities within armed conflicts would at once become politically suspect. The relationship between war and humanitarian action is captured more faithfully in the more generic observation of analyst Hugo Slim that, "as it has developed next to war through the centuries, humanitarian assistance is much more a priestly rite of cleansing and boundary setting in war than a prophetic movement of political and social challenge to war itself" ("Sharing a Universal Ethic: Spreading the Principle of Humanity Beyond Humanitarianism," address to a conference sponsored by the European Community Humanitarian Office (ECHO) and the Overseas Development Institute, "Principled Aid in an Unprincipled World," April 7, 1998, p. 2). Moreover, available evidence hardly supports Berry's contention that "war is becoming obsolete." The ICRC itself has even been criticized for the opposite result: that humanizing war, rather than abolishing it, renders war more acceptable.

Precision, which is of the essence in traversing the humanitarian and political interface, is quite absent in this volume. Berry's presentation and analysis of data lack rigor and discipline. The ICRC is introduced as one NGO among many, ignoring its special status. Yet, the ICRC is not "the only NGO accredited to the UN, allowing it to voice its opinion directly and giving it access to meetings of the General Assembly and committees." Lengthy excerpts from ICRC documents and interviews, off-putting in their intrusiveness, do have the value of allowing the reader to render independent judgment about the author's conclusions. Berry's thesis leads to other proof-texting distortions as well: for example, the treatment of the NGO Code of Conduct as an antiwar tract rather than a set of principles for humanitarian operations. Although the ICRC did play a role in interna-

tional efforts to end the conflict in former Yugoslavia, an Oscar for best supporting actor would be inappropriate.

In sum, the book highlights the complex interaction between humanitarian action and the political realm. Yet, its treatment of that interaction reflects the confusion in the current debate about humanitarianism and politics rather than contributing to a long overdue understanding of the key issues and options.

Africa and the International System: The Politics of State Survival. By Christopher Clapham. New York: Cambridge University Press, 1996. 340p. $59.95 cloth, $19.95 paper.

William J. Foltz, *Yale University*

In this major contribution both to our understanding of African politics and to our understanding of the interplay between the international system and its weakest members, Clapham gives us a view of international politics from the bottom up. His focus is on how African leaders have sought to extract resources from the international environment in the attempt to survive. Their task has never been easy, and in Clapham's reckoning not only the leaders but also their states face even harsher struggles for survival in the post–Cold War era.

Clapham builds from Robert Jackson's (*Quasi-States,* 1990) view of African states as "juridical states," demarcated and defined by international convention and dependent on the Westphalian order for the sovereignty that gives their rulers standing to bargain in the international system. Their legitimacy and sovereignty are externally vouchsafed, whatever may be their relations with the peoples living within their official borders. That sovereignty in turn provides a "bargaining counter" with which African leaders hope to extract economic aid, political support, and weapons with which to enhance their ability to hold onto power. Such support could consort with a neopatrimonial internal political and economic order that could give the regime monopoly status without ever effectively penetrating or mobilizing the energies of ordinary peoples through a serious process of nation-building.

The Cold War provided an ideal environment in which African leaders could seek external resources without embarrassing questions being asked or encumbering conditions being imposed. The regimes most confident of their domestic base and regional security could abjure the extra protection and play the nonaligned game; most African leaders found it expedient to tilt one way or the other, depending on opportunity and need. (The West was a better source of money, the Soviets of arms.) The 1977 reversal of alliances when Ethiopia and Somalia exchanged superpower patrons demonstrated how labile ideological commitment could be. In turn, the Cold War's end—"overwhelmingly greeted by African rulers with regret" (p. 159)—took away much of the leverage African states possessed and left the field clear for the state-transcending forces of globalization.

The international community's continued support for the juridical states was overmatched by disaggregative pressures. The international financial institutions insisted on shrinking the state while subjecting what was left to external supervision and "conditionalities." Bilateral economic aid was increasingly funneled through NGOs, many opposed to the regime in power. What had been state services, such as health and education, were privatized or devolved to regional or local bodies. International organizations, the UN High Commissioner for Refugees most notably, came to exercise de facto responsibility for the survival of millions of people. Insurgencies multiplied. For the African states' inability to withstand these pressures, Clapham faults their dismal economic performance, which has driven economic activity underground and out of the state's grasp, and which has curtailed, petroleum aside, whatever international investment interest there might be. Clapham puts "corruption" at the center of his explanation for economic failure, although well-meaning incompetence plays its role, ranging from the "awe-inspiring stupidity" (p. 170) of Tanzania's development schemes to the Organization of African Unity's "economically illiterate" (p. 176) Lagos Plan of Action for an African Common Market.

African leaders' maneuvers to stay in power provide the entry point of Clapham's analysis, and their responsibility for current woes gets pride of place. None of the significant actors comes off well, however. Among the former colonial powers, France is faulted for its support of corrupt francophone tyrants and its aggravation of civil wars in Nigeria, Liberia, and Rwanda. If France has acted like an interfering parent, then Britain has been a neglectful one, seeing Africa "as a source of trouble rather than opportunity" (p. 88), an attitude that British officialdom extended to Lonrho, the one British firm closely involved with a number of African governments. Yet, as Clapham points out, the legacies of the lesser colonial powers are worse. Whereas several of the former French and British colonies have had serious troubles, every single African country not colonized by Britain or France has had a "dramatically greater level of insecurity" (p. 85). As for the superpowers, the best that might be said is that during the Cold War the African leaders found them easier to manipulate than the former colonial powers.

African insurgencies are dealt with in a particularly acute chapter. For Clapham, these are the "ultimate proof of the failure of monopoly statehood" (p. 209) and are part and parcel of the failure of the African state system. While insurgencies have domestic roots (only Mozambique's Renamo was evidently external in origin), they are dependent on external support. It is neighboring states, not outside powers, that provide the key support. Few of these insurgencies live up to Mao's romantic image of fish swimming in a sea of peasant support; most have victimized whatever local population was unlucky enough to find itself in an operational area. (The successful Ethiopian and Eritrean insurgencies may be exceptions to Clapham's strictures.) The norms and executive weakness of the OAU and other structures of the African state system have rendered them nearly incapable of dealing with the challenge of insurgency, Clapham argues. The recent, and perhaps transient, success of the Nigerian-led interventions in Liberia and Sierra Leone may open room for a slightly more hopeful judgment, as may the unprecedented Franco-Anglo-American cooperation in equipping, training, and providing logistical support for units of selected African armies for eventual peacekeeping duties. Still, one can hardly dispute Clapham's judgment that just as the insurgencies overwhelmingly resulted from internal factors, so they likewise must be settled internally.

Clapham's book is not written in the language of international relations theory, but theorists will have much to learn from it. State-centric neorealism has little relevance to a world in which the interests served by political action are narrowly those of the leader. Leadership, seeking to maximize and eternalize its grip on power, serves the state only coincidentally; statehood, however, is the rapacious leader's prime resource. Clapham urges greater attention to, in effect, "second image reversed" analysis of the domestic political effects of international structure. He also calls for focusing on the struggle for control of resources flowing across state

boundaries. A new field beckons to the theoretical and empirical researcher alike: the international relations of statelessness.

New Thinking in International Relations Theory. Edited by Michael W. Doyle and G. John Ikenberry. Boulder, CO: Westview, 1997. 296p. $69.00 cloth $25.00 paper.

Robert W. Cox, *York University*

This book implicitly reaffirms what Stanley Hoffmann wrote over two decades ago, that international relations (IR) is an *American* social science [*Daedalus* 106(1997):41–60]. Not only are the contributing authors all Americans, teaching in American universities, but the frame of reference of discussion concerning IR theory is also specifically American. Among the names of noncontributors prominent in the index are (in no particular order) Keohane, Gilpin, Ruggie, Katzenstein, Krasner, and Waltz. The issues for debate have been defined by these scholars; and the newer generation of contributors to this book have joined in the debate with the aim perhaps of broadening its range, considering alternative answers to the questions raised and, in particular, introducing a concern for understanding "change."

Within the limits of this intellectual framework, the book has considerable merit. Miles Kahler's excellent chapter on the postwar history of IR theorizing in the United States takes reflective distance from its object in discerning the forces shaping this theory: the demand from government and foundations for knowledge to match the new global interests of the United States (p. 28), which made influencing the minority that shapes public opinion the primary goal of theorizing (p. 29). He links the individualist rationalist models that became a feature of this theory with a traditionally American ahistorical liberal exceptionalism (p. 39).

The book signals a late entrance to the American debate by postmodernism and feminism. The ironic self-deprecating notes struck occasionally in James Der Derian's chapter suggest a lingering uncertainty about postmodernism's acceptance as a "dialogical" partner, the concept of "dialogism" being derived from the Russian literary theorist Mikhail Bakhtin. The monologue, for Bakhtin, is the single acceptable framework of discussion to which all participants must conform if they are to be admitted to the debate (what the French call *la pensée unique*); dialogism accepts a plurality of discourses each continually generating new meanings and new identities. Der Derian thus potentially undermines the *American* character of IR as a social science by postulating a plurality of realisms for a multipolar and multicultural IR (p. 57).

Jean Bethke Elshtain continues this theme with a critical review of the classic foundations of the dominant IR discourse. She argues that the contemporary neorealist readings of Thucydides and Machiavelli are one-sided whereas these classics of theory are open to alternative and more complex readings that, in the case of Thucydides, see the relations of states as shaped by forms of knowledge and civic identity rather than by some essential quality of "stateness" and, in the case of Machiavelli, which bridge the dichotomy of public and private. The gender factor for her is not a question of "adding women" but of discarding the genderless rational actor model in favor of greater awareness of modes of social organization and the perspectives of subjectivity.

From here on only Matthew Evangelista looks seriously within the encasement of the state to examine the link between domestic structures and international behavior in a very clearly written chapter. Daniel Deudney seeks to revive interest in geopolitics. While acknowledging German sources, he assimilates geopolitics to the vocabulary of American social science by defining it as a "structural-functional security materialism" (p. 93). He would bring "nature" into the calculations of IR theory—nature considered not only as factors like climate and access to the sea but also nature as modified by technology. "Materialism" for this author is the interaction of geography and technology (p. 100). James De Nardo makes a fairly complex attempt to explain why mathematical modeling does not yield definitive conclusions for policy. His answer is that models rely on the assumption that both sides in a conflict perceive the world in the same way (p. 136), an assumption (we may add) that anyone taking a dialogical position (Der Derian, above) would have rejected at the outset. He concludes with the rather minimalist justification that the benefit of modeling is to underline the complexity and uncertainty of things (p. 156).

Joseph Grieco seeks to reaffirm realism—although what he calls realism seems more like American neorealism than the realism of, for instance, an E. H. Carr—while acknowledging that there are some problems in explaining why states invest time and resources in international institutions like the European Union, and in knowing whether, given the posited condition of anarchy, states will seek to maximize either security or power (or whether perhaps they amount to the same thing). Steven Weber argues, beyond Grieco's realism, that states pursue not just interests but purposes that are infused by ideas and values, and this is what leads them to join with other states in creating international institutions, institutions which are thus endowed with a distinct ontological status, no longer to be considered as just part of the environment in which rational-actor states pursue their particular goals.

Since this book is intended to be assigned in international relations theory courses it is important to consider its implications for the understanding of the world by a coming generation of Americans who will have something to say about America's role. In this regard some serious questions arise. Does it focus attention on the problems the world faces and what the United States can and should (yes, there is a moral question that cannot be evaded by a commitment to "science") do about them? Does it lay down an approach to knowledge that would be conducive to confronting these problems?

The "American social science" of IR has had these features: (1) an aim to constitute an "objective" science that would include testable generalizations of universal application about politics beyond the domestic level; (2) a separation of political and economic spheres, which means that social conflict and cleavages arising in the economic sphere are excluded in favor of "interests" that are an acknowledged part of the political; and (3) a definition of "reality" in world politics as something that is knowable in the same way by all rational observers. This set of assumptions, viewed historically, may be seen as characteristic of the eighteenth-century Enlightenment thought in which American political institutions and the tradition of American political thought were conceived. They have been accentuated by recent decades of Cold War in which the European alternative of Marxism has been anathematized, effectively excluding that rival form of knowledge.

Even though the United States has emerged as the greatest single state and societal power in the world, that power is probably not great enough to organize the whole world in its image. The question is, then, whether the coming generation of American thinkers about world affairs does not in the first instance require a deep awareness of the different modes of

thought about the nature of world problems to be found in other coexisting and developing cultures and civilizations—in other words of the different "realities" that confront one another.

Taking this approach would also yield a different sense of problem priorities from those discussed in this book. The polarization of rich and poor, not just among but within countries, developed and less developed, would loom as a principal problem of world politics. Elshtain, in this book, points to the effects of the separation of public and private but none of the authors discuss the consequence of the separation of the economic from the political in allowing free rein to economic power to dominate and deprive, thereby creating conditions of political instability.

Deudney introduces "nature" but neither he nor any other contributor to the book considers the problem of the biosphere as the ultimate material constraint on politics and economics. Perhaps the explanation of this gap is embedded in ideology. Just as social cleavage may have been marginalized in IR thought by the anti-Marxism of the Cold War, so ecology may be set aside for economic reasons as posing a threat that it could revive claims analogous to those of the aborted New International Economic Order of the 1970s—cost-raising constraints upon global economic expansion.

The editors, in conclusion, suggest that "it is necessary to look more deeply into the character of societies and states to find out how they construct goals and expectations" (p. 278). Unfortunately, the book does not go very far in this direction toward an emphasis on the development of civil society as the foundation for new political authorities from local to global levels.

Perhaps what is most needed for a radical understanding of "change" in the condition of world order is a retreat from the constraints of the persistent attempt to emulate the physical sciences and a new openness to a more historically based interpretive or hermeneutic understanding which would broaden considerably the range of disciplines that have a contribution to make. History (e.g., Fernand Braudel), anthropology (e.g., Karl Polanyi), economic history (e.g., Harold Innis), and the classics of social and political theory (e.g., Max Weber and Karl Marx, going back even to Ibn Khaldun, Machiavelli, and Gianbattista Vico) still have much to offer as guidelines for understanding historical change in which the level of state interactions cannot be meaningfully separated from the total process. One would hope that alongside books like the one reviewed here students of IR would be exposed to readings from outside the specifically American theorizing of IR. As examples, see Susan Strange, *The Retreat of the State* (1996), and Bertrand Badie and Marie-Claude Smouts, *Le retournement du monde* (1992), both of which in different ways expand the scope of power relations beyond the interstate sphere and focus on vital problems that are largely obscured in the narrower lens of conventional IR theory.

Strategic Assessment in War. By Scott Sigmund Gartner. New Haven, CT: Yale University Press, 1997. 256p. $30.00.

James J. Wirtz, *Naval Postgraduate School*

Although operations research often shapes how militaries conduct war, scholars have not paid much attention to the way military establishments employ quantitative analysis to evaluate strategy and policy. This state of affairs is perplexing, given the statistical and analytical dexterity often displayed by political scientists, for instance, on the pages of the *APSR*. Maybe "systems analysis," Robert McNamara's effort to use statistical indicators (body counts, aircraft sortie rates) to gauge progress in the Vietnam War, was so politically and strategically disastrous, that it discredited study of the effect of military operations analysis on organizational and state behavior. Alternatively, those interested in explaining the behavior of military organizations generally eschew quantitative methods. Not surprisingly, their analyses of the way organizations develop and modify strategy often gloss over the effect of statistical indicators as an independent variable influencing organizational behavior. Realism, bureaucratic politics, and strategic culture are preferred explanations in accounting for the origins and implementation of military doctrine and operations.

Strategic Assessment in War thus represents an important and overdue contribution to the extensive literature on the way military organizations select and assess strategy in wartime. In Scott Gartner's view, organizations do not judge their performance by surveying the universe of available data; instead, they select "dominant indicators" (measures of effectiveness) to assess progress toward achieving organizational objectives. These measures of key aspects of the operating environment are often unique to specific institutions because they reflect organizational cultures, missions, and goals. Gartner also suggests that decision makers do not use dominant indicators as a sort of cumulative index to judge the success and failure of their policies. Officials estimate the prospects for success by monitoring positive or negative trends in dominant indicators and the direction and acceleration of these trends. In other words, bureaucrats could believe that a deceleration of a positive trend is more alarming than a negative trend that does not appear to be worsening.

To demonstrate the usefulness of his decision-making theory, Gartner describes four instances in which organizations reassessed their preferred strategy in light of battlefield developments: British anti-submarine warfare in World War I and World War II; the debate about U.S. policy in Vietnam following the 1968 Tet offensive; and the Carter administration's deliberations during the hostage ordeal in Iran. Gartner's theory works best when applied to the way a particular institution interprets events. For example, in the aftermath of the Tet offensive, army officers were encouraged because Tet produced a sharp jump in the rate allied forces were inflicting casualties on North Vietnamese and Viet Cong units. This represented an acceleration of a positive trend in the dominant indicator that the army was using to measure progress in the war. Others in the Johnson administration and, for that matter, the American public were more concerned about the sharp increase in the rate American units suffered casualties during Tet. As a result, the army's claims of victory in spring 1968 were difficult to comprehend for many Americans both in and out of government.

The policy debate following the Tet offensive also demonstrates the limits of Gartner's approach. What broke the back of the Johnson administration's commitment to Vietnam was General William Westmoreland's post-Tet request for about 200,000 additional troops. General Earle Wheeler, Chairman of the Joint Chiefs of Staff, apparently encouraged Westmoreland, who commanded American forces in Vietnam, to make the request. Wheeler was concerned because the army lacked reserves for additional contingencies; all available forces had been sent to Vietnam. The troop request would have forced President Johnson to call up the reserves, a politically charged decision that Johnson had sought to avoid. The army's claims of victory, the call for additional forces, and the prospect of a reserve call-up, created a political fire storm, leading Johnson to announce that he would not seek another term as president. Although Gartner's theory of

decision making helps to identify the context in which various players viewed events in Vietnam, it cannot capture the rich interplay of competing political positions and policy preferences that shape governmentwide policy outcomes.

Gartner's work represents an important theoretical contribution to the literature on bureaucratic policymaking for both political scientists and systems analysts. In terms of operations research, it suggests that organizational culture influences the way people select and use measures of effectiveness to evaluate institutional performance. For political scientists, it can explain when organizations will identify some event as a problem or opportunity, and it helps to explain the context used by organizations to view their domain. Gartner also offers a key methodological insight in warning against placing too much analytical emphasis on the cumulative effect of battlefield activity, a measure of effectiveness which appears persuasive in hindsight but is of less significance to officials struggling to overcome an ongoing problem. Gartner probably asks too much of his theory to explain outcomes that reflect many competing political currents and interests, but his approach does identify the "statistical" context of political debates. Indeed, the selection of measures of effectiveness can only take on increasing importance in an information age, a time when the cost of collecting and analyzing quantitative data is diminishing.

China's Security: The New Roles of the Military. By Mel Gurtov and Byong-Moo Hwang. Boulder, CO: Lynne Rienner, 1998. 349p. $59.95.

David Lai, *University of Colorado*

China is determined to regain its national strength, national unity, and international prestige. This book is about the People's Liberation Army's (PLA) role in this mission. Gurtov and Hwang have provided an excellent account of the Chinese military establishment, its relation to the Chinese Communist Party (CCP) and China's foreign policy. They have also shown how economic and technological development and domestic and international factors have substantially reshaped Chinese military thinking and behavior. This is one of the best books about China's military published in the 1990s. It is a must-read for students of China politics.

The authors begin with an insightful analysis of the Chinese civil-military relations. In China, the ruling party commands the gun. Leaders of the party and the military are so interconnected, however, that the party and the military are in essence an organic whole with no separate and competing interests. As such, Gurtov and Hwang are correct to point out that conflicts between high-level civilian and military leaders are intraparty elite conflicts. Accordingly, occasional PLA intervention in domestic politics results not from party-military conflict but from intraparty debate and decisions. In this sense, military involvement in domestic politics is a stabilizing action. Furthermore, the PLA is much more than a fighting force. It has served a whole range of domestic, political, social, and economic functions. It is the party's important instrument for Chinese politics.

With a clear delineation of the party-military relationship, Gurtov and Hwang proceed to discuss Chinese military thinking and the approaches in national defense (the authors call them doctrines). Since the founding of the people's republic, China has had a national defense policy of active defense. The people's war doctrine lays the foundation for this policy. It emphasizes the following principles: primacy of politics, primacy of people over weapons, the strategy of using a weak force against a strong force, the mobilization of the masses to fight a protracted war against invasion, and multiple roles of the PLA.

The people's war doctrine came out of the Chinese experience against foreign invasions. In the past, the Red Army was weak and could not prevent its enemy from invading China; however, the Chinese people had a great capacity to endure the initial suffering from the invasions. The CCP eventually led them to defeat their enemies through protracted guerrilla warfare. Times have changed. The Chinese military today is much stronger. China now finds it possible to stop potential enemies at the borders and even deter them from contemplating an invasion. Deng Xiaoping and other top leaders in 1981 modified this old doctrine, dropping the part about luring enemies deep into the Chinese territory and then waging a people's guerrilla warfare against them. Today, active defense means confronting enemies in the outer stretches and stopping any advance before an incursion in China's territory.

Gurtov and Hwang have carefully documented the evolution of the new military thinking and the modifications made to the people's war doctrine in the last two decades. They have shown that Chinese military leaders have expanded their strategic thinking from the traditional geographical territory (primarily the land) to strategic boundaries of the so-called three-dimensional frontier, with more attention given to the ocean outstretch and outer space. The Chinese navy, for instance, has advanced an offshore active defense approach, which allows it to exert greater control over a vast ocean front surrounded by Korea and Japan in the northeast and the ASEAN (Association of Southeast Asian Nations) nations in the south. This approach has allowed the Chinese military to put new pressure on Taiwan. It has also put a backbone in China's diplomatic dealings with the ASEAN nations over the South China Sea islands.

The Taiwan issue and the disputes over the South China Sea islands are in fact test cases for China's new military doctrines. Gurtov and Hwang include an excellent discussion of these two cases in chapter 8. The authors point out that the ways in which the Chinese deal with the Taiwan problem and island disputes are embedded in a multitude of other issues: China's national unity and sovereignty, Chinese domestic politics, China–U.S. relations, and China's regional relations. The military's role is especially significant in these areas. Yet, while Chinese military has made some headway in its offshore active defense, its experience in these areas has also exposed the weaknesses and limitations in the PLA's offshore fighting capabilities. It is believed that a full-fledged offshore active defense is still decades away.

In addition to the fundamental issues mentioned above, Gurtov and Hwang have three interesting chapters on issues that are of interest to outside observers. These are the Chinese military's involvement in China's economic reform. Through these three chapters (5, 6, and 7), Gurtov and Hwang discuss the issues and problems in China's defense industry and armament production and sales. The authors have shown that the effect of China's economic reform on the military behavior is mixed. While the military has derived some limited income from its economic activities and made some headway in military-civilian production conversion, the PLA has been drawn into a money-making business. This erodes the discipline and morale of the PLA. As I am writing this review, news from China reports that Jiang Zemin and the top leaders have launched a campaign to crack down on corruption in the military's economic dealings and scale down the military's business ventures.

The rise of China and its implication for world affairs have been a subject of debate in the last several years. At issue is

whether this rising China will be a militant one. Critiques in the West are quick to put forward a China-threat thesis. They charge that as its power base expands, China is bound to be more aggressive on a whole range of issues such as Tibet, Taiwan, territorial disputes over the South China Sea islands, military modernization, armament development and sales, and military competition in open seas and outer space. Led by a cover article in *The Economist* in August 1995, advocates of the China-threat thesis have been calling for containment. They charge that if not stopped or at least constrained, a rising China is bound to pose a great security threat to its neighbors and ultimately come into conflict with the United States.

Gurtov and Hwang did not write this book to join the debate. Their findings, however, compelled them to reject the China-threat thesis. They point to the fact that instead of menacing its neighbors, China has normalized relations with each of them. In the coming decades, China's priority is economic development. It needs a peaceful international environment. China's military will be more essential in stabilizing the mounting domestic political and social problems than external adventures. The fundamental national-security function of the PLA is to safeguard China's territory. Defense of the country's lengthy borders and protection of China's sovereignty are therefore the PLA's key missions. Furthermore, the PLA has numerous serious weaknesses acting as constraints on its external ambitions. China is going to be more assertive on international issues. This is normal for such a big power. However, a more assertive China is not necessarily a militant one. The two authors warn against the trap of Cold War thinking.

Sovereigns, Quasi Sovereigns, and Africans: Race and Self-Determination in International Law. By Siba N'Zatioula Grovogui. Minneapolis: University of Minnesota Press, 1996. 282p. $54.95 cloth, $21.95 paper.

Leonardo A. Villalón, *University of Kansas and Université Cheikh Anta Diop de Dakar*

The ambiguities in the notion of sovereignty and the limitations on the independence of the former colonies which acceded to the status of autonomous states in the post–World War II wave of decolonization have posed interesting analytical problems for political scientists attempting to understand the fate of Africa. While earlier theorists of "dependency" and of "neocolonialism" described the limitations on African independence in terms of the historical insertion of the continent into the international capitalist economy, more recently others have explored these limitations in terms of the inherent contradictions in the notion of national sovereignty that was elaborated by the major international actors following World War II. The most significant of these recent works is Robert Jackson's (1991) book, *Quasi-States: Sovereignty, International Relations, and the Third World.* Following on Jackson, but breaking with him in terms both of his analysis and his methodology (or perhaps more accurately, his epistemology), Siba N'Zatioula Grovogui undertakes in this book to study the role of international law in the "shortcomings" and "disappointing outcomes" of decolonization in Africa. (pp. 1–2)

The author's point of departure is the premise that there are "connections between historical philosophical systems and the structures of the international order" (p. x). As a result, his approach is based on the analysis of "the structures of the discourses of both international law and politics" (p. 2). In successive historical chapters Grovogui explores the evolution of the major international legal regimes: the justifications for imperialism and the colonial order; the post–World War I League of Nations mandate system; the United Nations' trusteeship system, and the logic of decolonization. In each of these he explores first the "discursive structures," which characterize the period, and then discusses the effect of this discourse on Africa, and most particularly on the case of Namibia, the former South-West Africa. The account of the legal discussion surrounding the elaboration of these regimes is often interesting; one might note in particular his discussion of the 1884–85 Berlin Conference's understanding of natives "right to dispose of themselves," meaning primarily the right to give up their sovereignty.

It is, however, not always clear—at least to this reviewer—what in fact is meant by certain supposed elements of that discourse. Take, for example, the argument that the "requirements of the dominant Western political economies" have been "legitimized by systems of thought and operation that have anchored the discourses of international law and relations in the Western ego" (p. 3). Or even more opaquely: "[The] fixation on the self and on self-interest can be an obstrusive [sic] self-love (*amour propre*) or an abstruse yet recognizable self-love (*amour de soi*). In the narcissistic moment between the Middle Ages and the Reformation, Christian theologians and other theorists posited the European ego as the sole locus of intercommunal relations" (p. 24).

In addition, the Namibia case itself is unfortunately not always as well presented as it might be. The crucial steps in the moves toward independence from 1978 to 1989, for example, are dealt with in one paragraph (p. 168), leaving the impression that one is not getting the full story. This may be in part because the case does not appear to fully support the argument. After insisting that the Western powers mediating Namibia's independence refused to acknowledge the supremacy of SWAPO (South-West Africa People's Organization) in the independence struggle because it did not serve their interests, the awkward fact that SWAPO emerged as the major force at independence is dealt with by the claim that it only did so by selling out its principles to the West. Indeed, in what sense Namibia's rather recent decolonization can already be said to be "disappointing" is not made clear.

These difficulties would seem to be rooted in a fundamental ambiguity in the argument of the book. The focus on the discursive and philosophical origins of international law suggests a reading of world history, and in particular of imperialism, in which major trends are driven by ideas rather than interests. This is emphasized in the detailed examination of the alleged Western mentality and the conception of "the other" which it entails. This argument is problematic, however, for two reasons. First, the attempt to find the ideas driving major and complex historical processes leads to an unfortunate but pervasive tendency to caricature those ideas. Note, for example, his list of four "authoritative claims and propositions" that he says dominate Western discourse on Africa: (1) "an emotive Western claim to postcolonial redemption and moral rectitude"; (2) "that African and other Third World crises are *solely* the result of inadequate domestic leadership"; (3) "that Western models ultimately offer the *only* reasonable alternative to the current state of international affairs"; and (4) "that Western philosophy, science, and culture remain the *sole* guarantors of international stability and prosperity" (p. 180, emphasis added).

More significantly, as international relations theorists have often noted, a consideration of the behavior of international actors would most frequently suggest the predominance of interests in explaining their actions, with the discourse of

ideas serving primarily as justification for the pursuit of those interests. Indeed, Grovogui's own account of events consistently supports such a reading, insisting as he does on how the West's encounter with Africa has been driven mainly by the effort to ensure its continued economic hegemony.

There is no doubt that centuries of Africa's relations with the international system have not been kind to Africa or Africans, and that much suffering has been the result. This is no less true in the postcolonial period, and the meaning, extent, and effect of the transfer of legal sovereignty to African governments is an important and timely topic of research. Moreover, it makes intuitive sense that, as Grovogui points out throughout this book, the fact that decolonization took place within the framework and rules of international conduct elaborated by the colonizing powers has had a significant and negative effect on the former colonial entities now recognized as independent juridical entities. Scholars hoping to understand the dynamics by which this has occurred might usefully be directed to Robert Jackson's work cited above. Grovogui's book, by contrast, might best be read as an impassioned essay calling for a recognition of the injustices of what he characterizes throughout as a "morally deficient" world order (p. 43). This book ultimately does a better job of decrying those injustices than of explaining how this state of affairs came to be.

The Future's Back: Nuclear Rivalry, Deterrence Theory, and Crisis Stability after the Cold War. By Frank P. Harvey. Montreal and Buffalo: McGill-Queen's University Press, 1997. 193p. $55.00 cloth, $22.95 paper.

Walter W. Hill, *St. Mary's College of Maryland*

The immediate reaction of many analysts to the collapse of the Soviet Union in 1991 was that the international system was entering a new era. In this new world order, the lessons gleaned since the end of World War II would become irrelevant. Harvey presents the provocative counterargument. He says that the end of the Cold War is likely to renew and exacerbate questions of nuclear proliferation and deterrence. Our understanding of various types of nuclear deterrence, as was developed in the last forty or so years, is likely to retain its applicability in the current era. Harvey reviews deterrence theory as it appeared and was developed in the Cold War and proposes a more extended form of analysis from that tradition. Despite claims that the collapse of the Soviet Union resulted in a new international system, he argues that events in the future will be partially understood by looking back at the use of deterrence during the Cold War.

The work proceeds by identifying relevant crises within the Cold War and analyzing these cases. Harvey is quite sensitive to the rules employed in the selection of cases. Although he believes that several fine reviews have generated a suitable list (p. 19), there is no consensus on what cases should be included. The lists of different authors do not match. He thinks that continuing to debate methods and case listings has become counterproductive. This position is understandable but probably not consistent with a positivist research program. He critiques critical questions involving coding rules.

One interesting problem is that deterrence may be extremely effective in many unobserved cases. Consider the instance in which the provoking country is deterred from even making the initial threat (p. 136). In a game theoretic sense, the nonequilibrium path is never seen, even though it constitutes the deterred threat. Such a case will not appear in any list, even though deterrence was clearly effective.

Even with a given case, coding presents further problems. The author lists (p. 49) 28 superpower rivalry crises from 1948 to 1988 and identifies the provoking country as the first superpower to become involved in the crisis. To my amusement, with these rules, the Korean War is seen as provoked by the United States in June 1950. Yet other rules present different anomalies.

Other problems exist when deciding whether a situation belongs as a case. For example, it is difficult to determine the short-range objective of a potential aggressor, particularly in the absence of an attack. To my mind Harvey persuasively argues that clarity need not come even with full access to diplomatic records. Given that uncertainty, it is difficult to evaluate the effectiveness of a given strategy. This is true in cases of general deterrence or immediate deterrence. He examines some cases, such as the 1956 Hungarian crisis and the 1962 Cuban missile crisis to clarify this point.

As should be expected in a book dealing with deterrence, several sections are informed by work in game theory. The writing is such that the technical aspects of the analysis can be easily understood by the general reader. In chapter 4, Harvey reviews various game theoretic strategies as applied to cases. As is typical in the deterrence literature, the games of chicken and prisoners' dilemma are commonly discussed. He favorably cites authors (p. 67) who argue that the traditional structure of the two-by-two game is too simple because perceptions, which are not easily coded in the payoff structure, were critical in the superpower rivalry. For example, credibility of a specific threat in a game might be evaluated outside the game. He reviews and uses material on iterative games, and this interesting reformulation is used in a later chapter. Recent material from complexity theory is not used and could perhaps be fruitfully applied to deterrence theory. The applications might be of particular interest in light of the author's concern with nuclear proliferation in the multiactor post–Cold War world.

In chapter 6 there is a discussion of horizontal nuclear proliferation. Harvey cites the pessimist's argument as compelling, that other states are likely to obtain nuclear weapons, and he notes the weapons program of a number of countries, including Iraq and North Korea, as well as the motivation of poorly paid Russian scientists to spread arms. My sense is that one can find alarms of nuclear proliferation going back at least to the Kennedy administration, and so I have the sense of rereading "the boy who cried wolf" in this section. Of course, the wolf may eventually come, and if weapons extensively proliferate, then decision makers have to worry about the use of the weapons. Toward the end of the chapter (p. 120) Harvey notes that "other factors further undermine the assumption that reduced direct confrontation between the superpowers will necessarily result in harmonious relations at a more general level." He cites Western indifference to post–Cold War international disputes and claims that this myopic viewpoint could be disastrous in the long run.

An alternative proposal for analyzing crises appears in the final chapter. The method begins with a game-theory perspective. Harvey treats what most authors view as a single crisis as a sequence of events in a protracted crisis. He examines events in the former Yugoslavia from April 1993 to September 1995 as a series of encounters rather than a single exchange. He tests to see if there was compliance or deterrence failure at each exchange. He finds this view of the crisis more compelling than a single round of a game. In the final section there is a concise critique, from a realist perspective, of several postmodern schools. Given the continuing debate on the relevance of realism, the interesting ideas in this short subsection could have been extended.

What If They Gave a Crisis and Nobody Came?: Interpreting International Crises. By Ron Hirschbein. Westport, CT: Praeger, 1997. 223p. $59.95.

Michael J. Shapiro, *University of Hawaii*

According to Ron Hirschbein, most of those seeking to make sense of "international reality" have yet to hear the good news about interpretive theory. In *What If They Gave a Crisis and Nobody Came*, Hirschbein both brings that news and shows how to make use of it. And despite backsliding into realist/empiricist language from time to time, he has been edified enough by his news to recognize that "international reality" is a product of interpretive imposition. Hirschbein's treatment also contains a lot of old news—for example, elaborate exegeses of various contemporary approaches to crisis decision making—to which he applies critiques based on his notions of what constitutes both interpretive and explanatory adequacy.

Happily, unlike the bulk of practitioners in the international relations field, Hirschbein is a post-Kantian. In a refreshingly lively set of discussions and analyses, he portrays crises as "meaning-making projects" (p. 16). This perspective, amplified with extended discussions of interpretative practices in various social science disciplines and with applications to Kennedy's and Nixon's Cuban crises, turns the reader's attention away from a world of unmediated facticity and toward the constructive imaginations of subjects. Kantian epistemology impugned approaches to meaning that relied on a world of things in themselves, and Hirschbein shows that the condition of possibility for the emergence of "international crises" is not the world of raw matter but the interpretive culture of subject/actors who make decisions.

The interpretive culture of Hirschbein's subject/actors articulates itself through metaphors and narratives. These symbolic equivalences and storylines combine to invent what are perceived as urgent situations. Enduring action stories (among others) reach out from the past and continue to impose meanings for those who must respond to the exigencies of the present. In developing this perspective, Hirschbein acknowledges an impressive variety of thinkers—philosophers and practitioners of interpretation, theorists of international decision making, cultural anthropoligists, biographers, and media analysts, among others. His book is an ambitious, comprehensive, and focused attempt to improve our understanding of crisis decision making, as it is understood by those who function within a state-oriented, geopolitical imaginary.

I add that last phrase because Hirschbein's project—an attempt to introduce a thoroughgoing hermeneutic mentality into a field dominated by a empiricist-realist perspective—fails on one of the most crucial dimensions of critical hermeneutics; it is not self-reflective about the predicates of the world it imposes. Hirschbein supplies no critical thinking about the practices of space (the geopolitical imaginaries) within which his "crises" emerge; he uncritically adopts the locus of enunciation of state officials as his own. This most serious shortcoming of *What If They Gave a Crisis* is previewed when Hirschbein evokes and then dismisses (a caricature of) a postmodernist perspective because, he says, he does not want to succumb to "the joys of postmodern sects by abandoning the search for meaning" (p. 48).

Apart from the way Hirschbein's remark effectively abandons his own position—that meaning is imposed not found—had he heeded postmodernist insights, he would have found perspectives that challenge his approach to subject/actors. While he succumbs to the joys of abandoning fixed objects and situations, he joylessly locks his subject into a spatially fixed and psychologized mentality. Despite his reference to the "symbolic turn in international relations" (p. 6), his subject is psychologically rather than discursively constructed. Moreover, his only subjects are U.S. public officials, whose intelligible world is a function of the reigning system of state sovereignties.

A critical approach to "crises" would raise issues about the discourses—the dominant structures of intelligibility—within which decision makers constitute the world of danger. Hirschbein's approach to hermeneutics strengthens the hold of those structures of intelligibility rather than analyzing and questioning them. In addition, granting that Hirschbein's project is to provide a better account of the terra cognita he shares with state officials rather than allowing other worlds to presence, his analyses fail even on their own terms. While he evokes the concept of an interpretive culture, his case studies rely primarily on psychobiographical insights into the mentalities of individuals. And while, at a discursive level, he privileges interpretation over explanation, he frequently lapses into empiricist commitments to unbiased explanations (even misreading the Gadamerian idea of prejudice as bias) (p. 97).

Nevertheless, despite its shortcomings, the good news about interpretation in *What If They Gave a Crisis* has redemptive qualities. Hirschbein's text frequently knows more than what it argues. In his construction of crisis decision making as a "meaning-making project" (p. 16). Hirschbein shows himself to be attuned to the limits of a reliance on mentality or structures of apprehension and the "contradictory dimensions of subjectivity" (p. 71). Were he to pursue the implications of a conceptually enriched notion of projects, he would be showing how the worlds of danger emerge not simply because of how people think within their decision-making cultures but because of how those cultures are constituted as a set of involvements or practices with considerable historical depth.

Hirschbein's promising notion of the project, along with some of his other insights into the ways in which the organization of decision-making cultures involves more than mere cognitive styles, should encourage more investigations into aspects of the habitus within which danger emerges. And his attention to the narrative dimension of state actors' projects, which situate danger in an unstable mythos rather than a fixed logos, should encourage an attention to various other narratives. A recognition of other narratives, for example, those of indigenous peoples and other cultures within states who fail to achieve a presence in the discourses articulating state actors' meaning-making projects, would reveal a world of danger for various peoples victimized by the frames of meaning surrounding crisis decision making.

Anarchy and Order: The Interplay of Politics and Law in International Relations. By James C. Hsiung. Boulder, CO: Lynne Rienner, 1997. 245p. $55.00.

Stephen A. Kocs, *College of the Holy Cross*

There is a wide gap in international politics between states' everyday foreign policy concerns, which revolve to a large extent around the negotiation and implementation of international legal agreements, and the theoretical analysis of international structure, which (at least in the United States) tends to treat international law as though it were irrelevant or nonexistent. Although James C. Hsiung is not the first person to take note of this gap, his book is one of the few to attempt

to bridge it. Hsiung's main target is neorealism, which he castigates for failing to recognize the role of international legal constraints in ordering and regulating relations among states. His avowed goal, however, is not to refute neorealism but to improve and revitalize it by incorporating international law into its explanatory matrix.

Legal order, Hsiung argues, is a necessary—even inevitable—derivative of an anarchic international realm. As sovereign entities, states confront the uncomfortable fact of coexistence with sovereign others. Lacking direct control over other states' actions, they crave legal rules that will impart a degree of order, predictability, and security to their external environment. The emergence of international law is thus dictated by states' self-interest. In the international realm, according to Hsiung, anarchy and legal order are not mutually exclusive; they are complementary and mutually reinforcing. Hsiung repeatedly criticizes neorealism's "fixation" on power to the exclusion of other important factors in international politics such as institutions and legal values (pp. 21, 33, 54). He agrees with Kenneth Waltz and other neorealists on the importance of power and self-help in ensuring states' survival under anarchy, but disagrees with neorealism's tendency to define self-help exclusively in terms of coercive acts and capabilities. States' willingness to accept reciprocal legal constraints on their freedom of action, he argues, is itself a form of rational self-help under anarchy (pp. 9, 183). In the end, the distribution of power matters in determining the fates of individual states, but international legal order plays an equally important role.

Hsiung's most interesting theoretical arguments are found in chapter 2, which treats the relationship between international law and the security dilemma. In neorealist theory, the security dilemma leads directly to the problem of relative gains: Neorealists expect states to have difficulty cooperating under anarchy because of the risk that an unequal distribution of gains from cooperation will lead to a loss of relative capability. But in practice, cooperation involves more than just questions of relative gain. It also involves agreement on a set of "rules of the game" under which cooperative exchange will take place. To thrive under anarchy, states require predictability and transparency in other states' behavior. Securing such transparency is highly valuable to states irrespective of other considerations. Thus, as Hsiung demonstrates, negotiating the legal rules governing cooperation often takes precedence over questions of the distribution of resulting gains. And since transparency has strongly positive-sum characteristics (in contrast to the zero-sum character of relative gains), a focus on rules rather than outcomes enables states to circumvent substantially the relative-gains problem. Neorealism, whose monofactor emphasis on the distribution of power causes it to overlook the value of predictability and transparency in an anarchic environment, fundamentally misapprehends the structural dynamics of cooperation among states in the real world.

In subsequent chapters, Hsiung analyzes the ways in which international legal norms constrain and regulate state behavior in such areas as human rights, decolonization, environmental protection, the resort to force, and the exercise of sovereign jurisdiction. While these chapters do not break much new ground theoretically or empirically, they do perform two quite useful services. First, they provide a concise overview of the substance of contemporary international law in terminology accessible to the general reader. Second, they describe the functioning of international law from a political rather than legalistic standpoint. Hsiung has succeeded in his attempt to produce a book on international law with which political scientists can be comfortable.

In his concluding assessment of Waltzian neorealism, Hsiung is remarkably forgiving. He praises neorealism's "structural emphasis and logic based on the anarchic premise," which, he believes, provide the theory with "the potential of self-renewal" (p. 215). He acknowledges that neorealism in its current form, which ignores the importance of legal norms in structuring international relations, "does not hold up to the real world at large" (p. 178). As Hsiung sees it, however, the solution is simple. Neorealism should treat power and law as equal and complementary pillars of international structure. Doing so would remedy neorealism's most glaring deficiency and broaden the theory so that it could account for international reality as it is actually experienced by states.

In my view, it is doubtful whether neorealism can be renovated along the lines Hsiung proposes. International relations theorists today fall into two quite distinct groups. One group, which includes the neorealists, consists of those who regard anarchy as the decisive causal force in international politics. For these theorists, the dictates of anarchy are inescapable and ultimately absolute, defining the very terms of states' existence or extinction. In the other group are those who view anarchy as a secondary factor in international life—one whose implications can be shaped and limited by self-interested states through a combination of concrete bargains and diffuse legal and normative constraints. Hsiung seems to be proposing a modified version of neorealism that would straddle these positions; but given the contradictory nature of the two camps' initial premises, it is far from clear that they can be reconciled in a single theoretical framework. Either anarchy is the decisive structural constraint in international politics, or it is not. If it is, then neorealists are right to view international legal agreements as ultimately irrelevant, and it is difficult to see how neorealism would gain additional explanatory leverage by adopting Hsiung's suggested revisions. But what if anarchy does not in itself determine the character of international structure? In that case it would seem that the neorealist paradigm collapses, leaving nothing to salvage. In contrast to traditional realism, neorealism is not a thickly descriptive and historically informed interpretation of international politics, in which selected explanatory variables can be emphasized or deemphasized while leaving the overall analytical perspective intact. It is instead a spare, deduction-based model whose predictions all proceed directly from its a priori premise that the consequences of anarchy are immutable and decisive. For neorealists to hedge on this assumption would amount to abandoning the paradigm altogether.

The Clash of Civilizations and the Remaking of World Order. By Samuel P. Huntington. New York: Simon and Schuster, 1996. 368p. $25.00.

Richard Rosecrance, *University of California, Los Angeles*

Samuel Huntington has made many important contributions to the fields of international relations and comparative politics. He has called to our attention the distinction between quantitative and qualitative arms races, advanced our understanding of modernization and institutionalization processes in the Third World, and contributed to our knowledge of "transnationalism in world politics" and of the effect of democratization. One important measure of creativity in the field is the number of new and important variables a scholar

introduces. Huntington's previous work leaps this hurdle with room to spare. *The Clash of Civilizations* dramatically continues this process, uniting both comparative and international analyses in a forcefully argued analytic tract. Though admittedly not a work of social science, Huntington's new essay offers powerful food for thought for scholars, governments, and revolutionaries: Will differences between "civilizations" agitate world politics? What can be done to reduce conflict and allow Western civilization to survive?

Huntington's broad subject follows in the path of Arnold J. Toynbee and Karl W. Deutsch, who understood that religious, linguistic, and ethnic differences could evoke and even determine social conflict, domestically and internationally. Toynbee observed the response of civilizations to challenge and graded their performances accordingly. Deutsch knew that as latent ethnic populations were mobilized to political activity, the very definition of the nation could undergo change. Toynbee observed that Sumerian, Egyptian, and Classical civilizations ultimately failed to respond to multiform challenges. In like manner, Deutsch witnessed the transformation of nation-states from one nationality to another. To take but one of his examples, Finland was initially dominated by its mobilized Swedish population, then ruled by Russians. The latent Finnish population became fully politically participant only in the twentieth century.

Unlike Toynbee and Deutsch, however, Huntington offers a theoretical approach to what has happened since the end of the Cold War and what will happen in the future. The division of the world into two blocs, which prevailed during the Cold War, no longer exists. But Huntington does not believe that the world has thereby degenerated into a realist anarchic system of nation-states in which the only protection is nuclear deterrence. The "one-world" theory of politics and economics, he thinks, is also false. The processes of "globalization" ultimately lead to cultural rejection and self-assertion. Instead, Huntington believes major fault lines are being drawn between civilizations: Orthodox vs. Western Christian; Orthodox vs. Moslem; Moslem vs. Hindu; Sinic vs. Hindu; Moslem vs. Western Christian. Sub-Saharan Africa and Latin America remain on the sidelines of these oppositions. There is a link between Sinic and Moslem civilizations which may grow in strength. Western Christian civilization is becoming marginalized. Not only that, it may be weakening in face of the challenge of other cultures. Increasingly, Huntington believes, fault lines between civilizations breed wars, particularly between Muslims and nonMuslims. He contends: "Fault line wars are intermittent; fault line conflicts are interminable" (p. 291). Such conflicts can be briefly interrupted by papal intercession, the intervention of the World Council of Churches, the OSCE, or by Dayton-style accords. But such mediations will not endure.

Huntington distinguishes between civilizations that have major core states (or in which the civilization is a state) and civilizations which have no cores. Sinic civilization has the core of China. Japan has its own civilization. Western civilization has linked cores in the United States, France-Germany, and the United Kingdom. Russia is the core state of Orthodox Christianity. Unlike these cultures, Islam has no core states, only feuding rivals for influence.

Yet, Islam represents, he believes, the future challenge to the West. Its birthrate is higher than that of other civilizations, and Huntington contends that the "Islamic Resurgence" is as historically significant a phenomenon as the European Renaissance, the Reformation, and the Industrial Revolution. The United States and Western Europe are involved in a "quasi-war" with Islam, and not just with Islamic Fundamentalists. Moslems of all political and religious corporations reject Western civilization and American influence. Even though direct conventional war only occurs from time to time, Islamic terrorism carries weapons into the citadels of Western power. Thus, the vital reference point for the future is the hostility between Islam and the West.

Huntington also forecasts the emergence of an alliance between China and Islamic nations. China will sooner or later assert its dominance of East Asia. If the United States resists this pressure, Beijing will seek support from Islam. Indeed, it is already doing so. The outcome of the conflict between the West and the United States on the one side and China and Islam on the other therefore depends upon the policy of "swing states"—Japan, Russia, and India. Japan will probably "bandwagon" with China unless the United States adopts an extremely strong line and builds up its military presence in East Asia. Russia will not adopt a pro-Chinese position. A long-term reapprochement between India and China, the world's two most populous nations, is impossible. In one Huntington scenario, "the United States, Europe, Russia and India . . . become engaged in a truly global struggle against China, Japan, and most of Islam" (p. 315). That conflict occurs because one core state intervened in conflicts within another civilization. To avoid war in the future, core states will have to keep out of each other's intra-civilizational quarrels. Will Great Powers heed such limitations? Huntington is pessimistic. As a result, conflict will likely increase between civilizations.

How plausible is such a world? How dominant are the civilizational forces which Huntington describes? As many critics like Edward Said and Bernard Lewis have argued, cultural differences cannot simply be ignored. However, Huntington's synthesis neglects three key points. The first is microanalytic in character. Civilizations have a tendency to break up into contending factions. Subunits emerge which dilute the integrity of the whole. Christianity was riven by the struggle between Protestant and Catholic camps and remains divided today. Buddhism scarcely remains a unified religious movement. In Islam, Sunni and Shi'ite wings vie for influence. There are in fact as many conflicts within cultures and civilizations as exist between them. Indeed, from Huntington's point of view, the Cold War was a struggle between two wings of an essentially Western and Christian civilization. Huntington admits that "the conflicts within Islam were also more numerous than those in any other civilization" (p. 257) about 42% of the total in 1993. When the prominent Arabist, Malcolm Kerr, wrote *The Arab Cold War* in 1965, he observed that rivalries among Islamic nations were just as characteristic as their common hostility toward Israel. Nothing which has occurred since alters this judgment. Taiwan and China, North and South Korea, Afghanistan, and Tajikistan exist within the same civilization yet have signal differences.

And one might ask, if civilization is the true independent variable, why did it give way to power relationships during the Cold War? If civilizations bowed before military power then, aren't they likely to be reshaped by analogous forces now? In the final scenarios of the book Huntington aligns a powerful China with Islam to oppose Western nations. Japan bandwagons in favor of Beijing, leaving India and Russia to join the West. These alignments explicitly cross civilizational boundaries and reflect the interests of powerful states, not overarching cultures. One might then be tempted to conclude that military power and bipolarity can dwarf the influence of cultural determinants.

But state power is not the only relevant factor. Today the "clash of civilizations" is overshadowed even more by global economic power (which is both more seductive and harder to resist) than military strength. Judged by their recent behav-

ior, both China and Iran do not appear to overlook the economic benefits of a closer relationship with Western countries. Thus, macroanalytic forces provide a second basis of criticism. If divisive forces within civilizations are still powerful, so are forces that unite them, militarily and economically. In East Asia, the members of ASEAN are Buddhist, Christian, and Moslem. They include Vietnamese and Thai cultures that are distinct from China's, APEC brings into this group Western nations, China, and Japan. The economic benefits of association appear to transcend civilizational limitations. OECD includes several cultures; G-8 and IMF still more.

In fact, the influence of the world economy is the one variable essentially excluded from Huntington's analysis. Some claim that economic interdependence among states was never higher than in 1913, but this did not prevent the outbreak of World War I. In fact this claim is largely specious. Though foreign investment then was a larger proportion of GDP than it is today, it was overwhelmingly liquid portfolio investment which could be quickly disposed of in a crisis. To the extent that direct investment in physical facilities was involved, it was largely confined to a metropolitan power's empire. European Great Powers did not have a significant stake in the economies of brother nations. This has dynamically changed today. In fact, most developed countries are decentralizing their manufacturing production to other nations and to each other. Most new German production, for example, will be lodged outside of Germany, in the United States, China, Eastern Europe, or East Asia. The United States and Japan are also sending much of their production overseas, with Japanese production dynamically increasing in the American economy, while U.S. production goes to Europe and Latin America as well as China. Largely excluded from this productive relationship is the contemporary Middle East. In that region, most Islamic nations continue to discharge their futile energies in regional rivalries and wasteful military spending, undermining their prospects of economic growth. Culturally, there may be an Islamic Resurgence, but there is no economic or military Islamic challenger to Western leadership and none is likely to arise.

Finally, Huntington's analysis contains methodological problems. Independent variables have to be correlated with dependent outcomes. Civilizational differences have not generally been correlated with the outbreak of strife, either domestically or internationally. If this had been true, multinational or multilinguistic states could not exist. There are more than 8,000 separate cultural dialects spoken in the world today. If the cultural groups which each of these represent were to become a nation-state, the present world of 200 countries would be smashed to smithereens. In fact, most present states easily transcend or confine cultural conflicts within their borders.

Equally, if civilizational difference is not correlated with conflict, neither is war the product of such differences. The enormous numbers of wars within a single civilization (look at Western European history) makes it clear that the equation does not work whether one starts from the dependent or from the independent variable. And important power and economic dimensions are entirely left out of the equation, even though they centrally bear on issues of war and peace.

This does not mean that Huntington's warning is not a useful corrective to "end of history" optimism that has characterized some recent thought. The rise and decline of civilizations sometimes lead to disturbances which cause conflict. It appears that Chinese though not Islamic civilization is rising. Huntington is right to suggest that Western states will have to find means of accommodating it within a more pluralistic society of nations.

Gatekeepers of Growth: The International Political Economy of Central Banking in Developing Countries. By Sylvia Maxfield. Princeton, NJ: Princeton University Press, 1997. 192p. $35.00.

Sofia A. Pérez, *Boston University*

The role of central banks in the political economy has been a subject of theoretical debate and empirical investigation by political scientists for some time now. Taking normative work by economists as its basis, most work by political scientists on the subject has focused on the effects of central bank status (i.e., whether the central bank is subordinated to executive authorities or enjoys some form of statutory autonomy or operative independence in setting monetary policy) on various measures of economic performance (inflation, growth, public deficits, and employment) or on the incidence of electoral business cycles. In recent years, there has also been a growing literature on the political determinants of central bank status, or as it is commonly referred to, central bank independence (CBI). Most work on the causes and consequences of CBI by political scientists, however, has been based on assumptions that fit the political and institutional context of advanced industrialized democracies. *Gatekeepers of Growth*, by Sylvia Maxfield, addresses what thus remains an important void in this literature. It develops a general argument about the determinants of central bank independence in middle-income developing countries.

Maxfield begins by pointing out that most explanations of why governments delegate authority to central banks focus on internal political causes while neglecting external sources. This closed-economy approach, she suggests, is particularly inadequate when it comes to understanding why governments opt in favor of CBI in developing countries. Maxfield acknowledges that some of the domestic variables highlighted in the literature on CBI in advanced industrialized countries (the time horizons of governments and the degree of political competition) may also be relevant in explaining variation in central bank status in developing countries, while dismissing others (wage bargaining institutions, leadership, and ideology). The single most important factor that is overlooked in closed economy models, she argues, however, is the need for international credit and investment faced by developing country governments. She proposes an "investor signaling" model of CBI according to which governments cede authority to central banks in order to signal their country's creditworthiness to international investors. The principal empirical implication of this model is that "actual central bank independence in middle-income developing countries [should vary] with a country's need for credit and investment, as perceived by government politicians" (p. 35).

The book is devoted to supporting this hypothesis through a combination of quantitative data and case study material. Maxfield draws a connection between recent moves toward greater central bank autonomy in Latin America, Eastern and Western Europe, and Asia and the rise in international portfolio finance over the last decade. She also looks at the relationship between CBI and need for international creditworthiness (as measured by foreign reserve coverage of import requirements) over time in four case study chapters on Thailand, Mexico, South Korea, and Brazil. In addition, she seeks to test her signaling hypothesis indirectly by regressing private investment in middle-income developing

countries against a set of variables that includes an index of central bank independence. This eclectic methodological approach allows Maxfield to present a more contextualized account of the subject than is offered in the quantitative literature on CBI, much of which has failed to produce consistent results. It also allows her to move the discussion beyond the problem of finding a valid, quantifiable indicator of CBI, one of the principal points of contention in the literature.

Maxfield is up against considerable data limitations in the regression analysis. She is not able to test her hypothesis directly due to the lack of temporal variation in measures of de jure central bank independence. However, she is able to draw a compelling connection between the changing external position of developing countries and changes in the role that central banks play in economic policy in the case study chapters. Indeed, the case study material represents the principal strength of the book. But it also raises some important conceptual issues. Maxfield chooses to measure central bank autonomy not in terms of legal or statutory features, but in terms of a qualitative evaluation of whether economic policy conforms to the preferences of central bankers. She offers a persuasive justification for this choice: There are important changes in the role that central banks are afforded over time that are not reflected in legal statutes. Yet, if such nonformalized central bank authority is a function of a government's need for external creditworthiness (as Maxfield argues) and can be withdrawn as easily as it is granted (as the case studies seem to suggest), then does it represent anything more than a change in government policy preferences? Maxfield does offer one measure of central bank authority that is independent of policy outcomes (governor turnover). But again, as her discussion of Mexico reveals, governor turnover is high when a government's need for creditworthiness is low. It declines when a government's need for creditworthiness rises. Is this central bank autonomy or a convergence in the preferences of governments and central bankers?

The problem is hardly unique to Maxfield's analysis. It is a conceptual ambiguity inherent in any institutional arrangement that involves a delegation of authority. Indeed, Maxfield seems to spell out that ambiguity in arguing that governments grant authority to central banks in order to signal "their" creditworthiness. But why should such a commitment be credible or effective in influencing investors. The answer, the literature suggests, seems to lie in the manner in which formal central bank autonomy alters the rules of the domestic political economy. Thus, while Maxfield persuasively establishes a connection in time between changing external constraints and central bank authority in developing countries, a real understanding of the dynamic here at play requires a more developed model of the domestic politics of economic policy in these countries.

In spite of these conceptual issues, *Gatekeepers of Growth* represents an important contribution to the literature on central banks, and beyond it, on the politics of economic adjustment in developing countries. It establishes a link between changes in the financial needs of developing countries and domestic institutional developments and thereby provides a framework for future research. One of the tasks of this future research will be to explain how central bank independence alters the political dynamics of economic policy in middle-income developing countries, and how this in turn may help us understand the choices that governments in these countries make in response to international financial pressures.

Crucible of Beliefs: Learning, Alliances, and World Wars. By Dan Reiter. Ithaca, NY: Cornell University Press, 1996. 232p. $35.00.

Patricia A. Weitsman, *Ohio University*

It is often said that military strategists are constantly preparing to fight the last war. They draw on lessons of the past, while neglecting key technological innovations. This leads to critical errors in the prosecution of war. In a similar vein, Dan Reiter develops a theory of learning that facilitates our understanding of how and why small states make the alliance choices they do. In *Crucible of Beliefs*, learning theory is developed and tested against balance-of-threat theory to determine which is the better predictor of small state alliance choices. In the quantitative section of the work, Reiter finds strong support for both of the key learning hypotheses, although especially for the individual learning proposition. Small states will learn from alliance successes and failures—they will maintain a course of action (alliance or neutrality) if they experience success or will change their course (alliance or neutrality) if they experience failure. In contrast, the balance-of-threat propositions do not fare nearly so well (pp. 71–2, 92–6).

The subsequent two chapters explore in greater detail a number of the cases. Chapter 6 analyzes two "clusters" of similar cases, Belgium/Netherlands/Switzerland and Sweden/Norway, that supported the individual learning proposition; chapter 7 closely examines the 16 cases (out of 127) that were not supported by the individual learning hypothesis. The combining of rich qualitative detail to supplement the quantitative findings is an important strength of the work. Reiter supplements learning theory by developing parameters within which learning will translate into policy. He argues that a state's political system will have an important effect on whether or not it acts on the lessons learned from formative events. He proposes and finds empirical support for the argument that democracies will be more apt to heed the lessons of history than will nondemocracies within the foreign policy domain of alliance behavior (pp. 183–9, 195–202).

In all, *Crucible of Beliefs* is a provocative, well-crafted, insightful, and informative work. Reiter is meticulous in developing and testing his theory; he is attentive to potential problems that would inhibit his findings. Furthermore, the prose is crisp and clear, chock full of illustrative anecdotes and examples. An additional strength of the work is its focus on small states—the most numerous members of the international system, yet the least studied. There are, however, a few questions that remain unanswered by the book.

A first issue is the unit of analysis. Reiter's analysis of learning suggests that it is the state that learns lessons from formative experiences. This necessarily begs the question of what happens when decision makers within a state learn divergent lessons from a formative event. Since Reiter borrows from social psychology studies and decision-making theory—theories that facilitate our understanding of the minds and thought processes of individuals—it seems there should be some mechanism through which we better understand how those processes are aggregated. It is not satisfying to equate learning with outcomes; the first is a process, the second, an action.

In addition, learning theory is only applicable to states whose goals do not fundamentally change. Lessons are heeded by status quo but not by revisionist states. For instance, in the brief discussion of the great powers and the applicability of the framework to these states in chapter 5, Reiter examines Germany. He writes that instead of learning how to avert war from the World War I experience,

Hitler accepted war as an important means to attain his objectives; and instead of learning the dangers of alliances, the lesson drawn would be only the importance of autonomy (pp. 114–5). This reveals the ambiguity surrounding the kinds of lessons that can be learned from any one experience. Reiter also writes that the central objectives of Hitler were so different from the largely defensive aims of the postwar Germanies that the "German leaders would be unlikely to draw on lessons from the Nazi experience to guide decisions on alliances" (p. 114). Preferences appear to be exogenous to the theory—learning theory is not really a theory of preference formation as Reiter suggests. The difficulties associated with aggregating the learning process are also clear.

Finally, a formative event is a "small set of high impact, vivid events that spur the drawing of new lessons" (p. 35). Yet, how can we know, ex ante, what will be a formative event? This problem is illuminated when Reiter discusses the states that did not learn from the World War I or World War II experience. For some of these cases, "the World War experience was simply not as significant as other political factors, such as the emergence of nationalism" (pp. 181–2). This implies that we only know post hoc what is a formative event to any specific state.

A related point is how we know, ex ante, when a situation will be analogous to a formative event. Reiter examines World War I and World War II. These are, however, exceptional cases—two systemic wars in the space of 25 years. It is unlikely in the era following the nuclear revolution that conflagrations of this type will take place. How do we extend the insights of Reiter's work into the contemporary system? What would make for analogous situations? In addition, since the focus of the book is on small states, would experiences such as wars of independence or colonial histories be formative events and more appropriate sources of analogy? Furthermore, to what extent do nonevents or crises averted—formative nonevents—affect learning? In answering these questions, Reiter's theory may help us understand the perpetuation of the NATO alliance following the end of the Cold War: Member states may have drawn lessons concerning the utility of membership in the organization. It must be emphasized, however, that the fact that these questions arise is testimony to the strength of Reiter's work, the many directions of research his book has revealed, and the promise of learning theory. They do not detract from the importance of his contribution.

Dan Reiter's *Crucible of Beliefs* is an outstanding addition to the alliance literature and the literature on international relations theory in general. It is a refreshingly new and innovative advance in the field. The book is a must-read for anyone interested in alternative avenues of inquiry about state behavior that go beyond the rationalist, realist, or institutionalist explanations that have dominated the field for so long.

Winning the Peace: America and the World Order in the New Era. By John G. Ruggie. New York: Columbia University Press, a Twentieth Century Fund book, 1996. 237p. $27.95.

War or Peace? Nationalism, Democracy, and American Foreign Policy in Post-Communist Europe. By Steven L. Burg. New York: New York University Press, a Twentieth Century Fund book, 1996. 258p. $35.00.

Tony Smith, *Tufts University*

For the purpose of charting the course of American foreign policy after the Cold War, the Twentieth Century Fund has commissioned a series of at least a dozen books to date—including volumes by Michael Mandelbaum, James Chace, Janet Heininger, Robert Art, Jonathan Dean, Rosemary Righter, Richard Ullman, and the author of this review—books that either reflect on the overall structure of the international system, as John Ruggie's work does, or that deal with specific issue areas, as Steven Burg's volume does. Under the direction of Richard Leone, the Fund's aim is to sponsor books based on an intimate understanding of world affairs that may be of interest not only to academic audiences but also to the informed public as well as to policymakers in Washington.

John Ruggie is a political science professor at Columbia and former dean of the School of International and Public Affairs there, a man known for stressing the importance of multilateralism in American foreign policy. In *Winning the Peace: America and World Order in the New Era*, Ruggie aims to make three important points. First, multilateral cooperation in economic and security affairs has been one of the most distinctive contributions of American foreign policy to world order since 1945. While the tradition dates back to Woodrow Wilson, it was only with the creation of the Bretton Woods System in 1944, the United Nations in 1945, and the North Atlantic Treaty Organization in 1949 that multilateralism became a central component in Washington's efforts to create an international system assuring this country's security.

Ruggie's second key point is that the contribution of liberal international multilateralism to America's success during the Cold War has gone underappreciated. Realism in the Academy, and unilateralism (not so much isolationism) in the Congress and the public, have failed to give credit where it is due to the enormous benefits the United States has reaped from a world order many of whose leading institutions have been inspired by American thinking on multilateralism.

Ruggie's final major concern is that a combination today of protectionism in economic affairs and unilateralism in security matters may lead the United States to turn its back on one of the most important set of policies that has contributed to the current Pax Americana. To counter these trends, he calls for a commitment to social welfare and job training programs at home large enough to insure a domestic consensus in support of international economic openness and for a better understanding of the enormous security advantages that the United States gains with its leadership in international organizations like the UN and NATO. If America would "win the peace" after winning the wars, then it would best sustain those practices which contributed to its current international preeminence.

While I have no quarrel at all with Ruggie's essential argument, I find the way he makes his case wanting. The overall logic of his book is sound: that past thinking created institutions that made for present greatness in ways that should be respected in future policies. But the amount of space given to the potted history that sets up the argument for the importance of multilateralism in today's world (surely 80% of this short volume of just 173 pages of text) is totally disproportionate to what should presumably be the work's major focus: what multilateralism means today.

Three cheers for multilateralism, to be sure, but what does this portend for the way forward? Should NATO be expanded? Ruggie seems unsure. How might the UN better serve peacekeeping functions? Ruggie describes the problem but offers no solution. How might the World Trade Organization respond to China's bid for membership or the International Monetary Fund more effectively monitor foreign political (and not only economic) performance? Here not a word.

Nor is even Ruggie's potted history of U.S. support for multilateralism totally satisfying as a basic introduction. He

makes no reference to America's efforts to foster multilateralism by sponsoring democratic governments in a host of countries, certainly a key ingredient of U.S. success. Promoting liberal democratic government in Japan and Germany—with the latter making the European Union possible and contributing importantly to later efforts to democratize Eastern Europe and Latin America, while the former has perhaps had some effect on political developments in South Korea and Taiwan—has been so fundamental to the unprecedented vitality of U.S.-sponsored multilateral economic and security institutions, that it simply cannot be ignored. Surely it is far from a coincidence that multilateralism works best where democracy (and not just capitalism) is most consolidated. I have no idea how a theme of such obvious and basic importance could have been neglected given the vast literature on the subject that has appeared since the early 1990s and, if this body of work has any point at all for today's world, the presumed dependence of the future of multilateralism on the strength of democratic governments worldwide.

Ultimately, then, the book fails to deliver on its potential. The strength America has drawn to date from its involvement in international multilateralism is not fully enough presented to make as rousing a case for it as the author (and I) would like, while the lessons from the experience of the last half-century are not clearly enough drawn to inspire the enthusiasm the author (as well as I) would like to see take hold in official Washington and with the public at large for a continuation of U.S. commitment. A worthwhile inspiration to make a case for American liberal internationalism has not satisfactorily materialized as a book.

Brandeis political science professor Steven Burg's *War or Peace? Nationalism, Democracy, and American Foreign Policy in Post-Communist Europe* is like Ruggie's in its ambition to help us better make important political decisions in challenging new times. Also like Ruggie, Burg is what I like to call a "national security liberal," a person who sees the American national interest linked to promoting a particular kind of world order first outlined by Woodrow Wilson, one where democratic governments unite institutionally to promote openness in international trade and investment and cooperation for security. Both authors also agree that the realist paradigms for analysis and prescription are fundamentally flawed by their inability to take seriously a liberal agenda for world order—the promotion of democratic government abroad (Burg's principal concern) or the sponsorship of multilateral organizations for security and prosperity (Ruggie's main focus). Whereas Ruggie is interested in international organizations, however, Burg's arena of analysis is the challenge of consolidating democratic governments in Eastern Europe, especially in the former Yugoslavia, in the aftermath of the Cold War, although he explicitly endorses multilateralism as an appropriate way for the United States to proceed.

Burg's mastery of the intricacies of the problems confronting liberal American statesmanship in the task of promoting democratic government in postcommunist Europe is quite impressive, for he manages to interrelate three distinct narratives: the evolution of ethnic political forces in the countries of Eastern Europe and the Caucases in the aftermath of the Soviet implosion; the story of Western and Russian efforts thereafter to promote stable governments in the region; and the collected wisdom of political theorists as to why we in the West should care to ease the transition from authoritarian rule to democratic government so as to avoid having this region descend into murderous anarchy unleashed by ethnic fears and hatreds manipulated by demagogic leaders.

Burg is already known as an expert on the Yugoslav crisis, but in this book he takes pains to make comparative references to analagous situations in the Baltic region, Czechoslovakia, and the Caucases (especially Armenia and Azerbaijan)—all situations where communist rule collapsed and ethnic politics came to the fore. While events have already moved beyond the histories he recounts, Burg's analysis nonetheless can stand as useful historical case studies. The same is true of his account of international efforts to contain Balkan wars. He completed his book before the Dayton Accords, so much has transpired recently, yet the clarity of his presentation of the deliberations of international actors is such that his analytic framework has not been left behind by events.

Ultimately, then, it is Burg's argument for the importance of democracy in postcommunist Europe, and his confidence that we know enough to make at least some important contributions to bringing a stable peace to the region by promoting such governments there, that emerges as the strongest, because the most enduring, element of his book. Indeed, because the theoretical sections of his book stand so well on their own, someone interested in ethnic conflict in Africa, South Asia, or the Middle East might also well profit from this study.

The essential message of Burg's book is liberal Wilsonianism: that the United States and its allies need to promote democratic governments in postcommunist Europe if they hope to serve their concrete national interests in promoting stable governments in the region. Whereas others (especially realists) tend to assume that there is little that outsiders can do to dampen conflict in the region, and that there is little reason for us to be concerned in any case because of our minimal interests in the region, Burg points out the danger of expanding wars in the region, the challenge of refugee and other humanitarian problems, and the world order need for the United States to affirm its leadership by responding to these crises by developing new multilateral solutions for ethnic conflict in areas important to Western security. As a result, Burg argues against the conventional wisdom that force should be projected only as a last resort into ethnic conflicts of the Yugoslav kind, just as he maintains that outsiders can participate in "nation building" and "state building" schemes despite the concerns of those in the military, the Congress, or the ranks of the realists who favor clear "exit strategies" that avoid "mission creep" (to use some of the terms bandied about today) and so oppose the political and miliarty agendas of liberal interventionists.

If the book has weaknesses, they are those of the liberal argument in general. While personally I would like strongly to endorse Burg's argument, candor requires facing its difficulties. One that Burg seems reluctant to admit is the extraordinary intractability of ethnic relations once they have become polarized. For example, despite enormous outside pressure, only now do the Troubles in Northern Ireland finally seem to be coming to an end. At the same time, the peace accord between Arabs and Israelis today seems to be unravelling, while there are mounting fears of renewed Hindu Muslim clashes in South Asia. In short, much as we may want the international community to make a difference, and compelling as the suggestions Burg makes as to what Washington and its allies might do to foster democracy abroad, how confident can we be that we have anything like the requisite influence to bring about transition to multiethnic democracies in regions like Eastern Europe, the Balkans, and the Caucases (not to speak of Africa, South Asia, or the Middle East)?

A second limitation is that Burg seems to me to inflate the

dangers to us of our failure to be successful in the liberal enterprise. He is surely correct that it would serve Western interests were democratic regimes to become widely accepted in postcommunist Europe; we should be glad that such seems to be the case in the Czech Republic, Poland, Hungary, and Slovenia. And Burg is right to stress that the United States should promote democratic governments abroad multilaterally—in the case of Eastern Europe, in conjunction with the European Union and Russia especially. However, we must know where not to risk allied unity in pursuit of quixotic undertakings. The likelihood of escalating wars and streams of refugees he warns us against must be weighed against the real costs of the protracted military interventions he seems to envision.

In short, both the inherent difficulties of influencing local circumstances and the vagaries of multilateral cooperation dictate prudence when calling for efforts to promote multiethnic liberal democracies abroad. Where the means to ends are not readily available, then however desirable the ends may be, we should be cautious as to the course we take. Burg has written an excellent book, one well worth reading by those interested in the stake the United States has in democratic transitions today in many parts of the world. But he surely knows how difficult the outcomes he hopes for will be to achieve, and he might have been more candid in informing the general public and our policymakers on the problems inherent in achieving the democratic future we all wish for these troubled regions.

Sovereignty over Natural Resources: Balancing Rights and Duties. By Nico Schrijver. Cambridge, UK: Cambridge University Press, 1997. 452p. $89.95.

Elizabeth R. DeSombre, *Colby College*

In this impeccably documented volume Nico Schrijver paints a thorough picture of the evolution of the principle of international law that he calls "permanent sovereignty over natural resources." This concept, tangentially related to broader discussions of what constitutes sovereignty or to issues of resource conservation, is in practice a legal doctrine of mostly symbolic effect. He traces the development of the idea primarily through examination of General Assembly resolutions, although he also examines its use in arbitration and in writings of legal scholars. The pinnacle of its development came in the 1962 declaration of the United Nations Commission on Permanent Sovereignty over Natural Resources, which affirmed the "rights of peoples and nations to sovereignty over their natural wealth and resources" (General Assembly Resolution 1803 [XVII]). This principle, originally intended to give developing countries the ability to control their own economic development, was expanded beginning in the 1970s to include state jurisdiction over areas of the ocean, and to ensure rights of states within broader principles of environmental protection.

The genesis of this principle can be traced to two different sources. First, newly independent developing countries (primarily in Latin America) pushed the idea of permanent sovereignty as a way to gain control over their own resources, often exploited by multinational corporations in vestiges of colonial influence. Initial struggles over the extent to which states had rights to their own resources began with the nationalization of industries, and the ensuing legal battles about whether expropriation of foreign-owned companies should be subject to domestic or international guidelines. This development also played into Cold War rivalries, as the Eastern bloc supported the rights of these newly independent states against those of the industrialized West.

Second, the principle developed in the context of nonself-governing peoples, to refer to the situation of those under foreign domination and the idea that their territory should not be exploited for the benefit of its occupiers. Schrijver explores this development through examination of the reactions to South Africa's control over what later became Namibia, Israel's occupation of Arab territories, and U.S. control of the Panama Canal Zone. This issue developed concurrently with international human rights law and the acceptance of the idea that people have the right to self-determination. Although it is possible to see in instances of foreign occupation an effort to develop the principle, Schrijver fails to show that this principle has been applied in a meaningful way in these cases. He points to resolutions that call for the rights of peoples in foreign-controlled areas over their own resources, and UN studies of the resource costs to occupied territories, but little evidence of behavior change can be seen coming from these discussions. If present at all, this influence came indirectly. For instance, Schrijver tells of a Dutch uranium reprocessing facility that was taken to task in the UN Council for Namibia for countervening the "rights of that people with respect to the ownership and exploitation of the natural resources of Namibia" (p. 150) because some of the uranium it reprocessed came from South African controlled Namibia. It is not clear that the finding produced a change in behavior, or that Dutch behavior in uranium reprocessing would have much of an effect on Namibian permanent sovereignty whatever the outcome.

It is at times difficult to distinguish whether the positions taken by states in the development of the idea of permanent sovereignty were due to actual concern over the validity of uses of legal principles (as they and sometimes Schrijver suggest) or to political maneuvering. It often seems that intricate legal arguments were made based simply on what various actors wanted the result of a given debate to be. But the fact that actors are playing politics with law does not prevent the legal arguments from having influence. It may be important to remember that politically expedient arguments can ultimately result in legal principles that have effects outside the issues on which they were formed.

The two main areas in which permanent sovereignty has established itself as an accepted principle are international investment law and laws pertaining to the environment and the international conservation of resources. Within investment law the acceptance of rights to nationalization and expropriation, within some parameters, are an embodiment of this principle. Most recent laws about environmental protection contain some statement about a state's sovereignty over its own resources and its ability to use them as it wishes for its own development.

Interestingly, Schrijver suggests that the emergence of this norm at the intersection of international laws both about how states conduct their economic affairs and how they address protection of the environment will allow for its use in the issue of sustainable development. Perhaps. But national control over resources seems ill-suited to provide guidance in an age of environmental problems that relate to transboundary resources or the global commons, and it does not take into account the importance of geographically fixed resources to world ecosystems. Schrijver does address the analogously developed principle of the Common Heritage of (Hu)mankind, which also arose out of concerns of developing countries hoping for influence over resources (in the common heritage case it was of resources beyond national boundaries). Because of their similar origin he does not see these

two principles as contradictory, but if you take each to its logical conclusion they may not be compatible.

Ultimately, though, Schrijver asserts that, along with the rights that can be derived from permanent sovereignty over natural resources come obligations. These obligations include some duty to exercise these rights in a way that ensures that the population of the state benefits from the use of these resources, to have "due care for the environment" (p. 391), to recognize rights of other states to resources that are shared or that cross boundaries, and to fulfill international obligations. In part he is making an empirical claim that obligations have tended to follow the development of rights of permanent sovereignty, as a way to restore the shifting balance of influence gained by developing countries. And in part he is making a plea, that we remember that, as the Permanent Court of International Justice ruled in a 1925 case, "all rights of an international character involve international responsibility" (p. 29), and act accordingly.

Rogue Regimes: Terrorism, and Proliferation. By Raymond Tanter. New York: St. Martin's, 1997. 260p. $29.95.

Beau Grosscup, *California State University, Chico*

Raymond Tanter's *Rogue Regimes*, using a case study approach, offers a few sound insights into how and why American policymakers view the post–Cold War context. First, he contends that they tend to personalize their enemies and demonize those they find most troubling. This has led them to identify those leaders and nations who appear to threaten the new American-led world order, label them "rogue or outlaw regimes," and attempt to contain or diminish the perceived threat. For Tanter, the tendency to demonize provokes policy whose centerpiece is the imposition of sanctions on the rogues for reasons of retribution. This contention helps us understand why and how sanctions have come to dominate the U.S. approach to perceived enemies. Second, Tanter informs us that the policy of the United States toward rogue regimes is often at odds with that of its industrial allies. This important discussion helps to sort out why European nations often oppose or give lukewarm support to U.S. calls for sanctions. It also highlights the fact that the notion of rogue regimes is fundamentally an American creation that principally serves U.S. domestic and foreign interests. Finally, in his case studies of Iraq, Iran, Syria, Cuba, and North Korea, Tanter argues for different policy options depending on the factors involved in their threat, the arena of U.S. interest served, and the status of the various regimes. This is useful because it focuses attention on the complexity of the policy context even in dealing with states and leaders lumped together under a general ideological construct (rogue regime).

Unfortunately, the book has three major shortcomings that severely limit its scholarly contribution. First, there is no critical assessment, or even recognition of such an assessment (as is provided in Michael Klare, *Rogue States and Nuclear Outlaws*, 1995), as to the political nature of the rogue regime label. Instead, Tanter operates within a highly questionable parent-child framework, in which the motives and behavior of the parent are never addressed, and accepts that the criteria by which rogues are designated are useful in separating out these states from the rest of the world. Yet, brief evaluation of the criteria suggests that many states, most notably Israel, Great Britain, France, Russia, and the United States itself, easily qualify as rogue regimes.

Second, Tanter relies wholeheartedly on the U.S. State Department definition and designation of states who sponsor terrorism to bolster his case for the usefulness of the rogue designation. Yet, the political nature of that terrorist list is well documented (e.g., see Edward Herman and Gerry O'Sullivan, *The Terrorism Industry*, 1989; Beau Grosscup, *The New Explosion of Terrorism*, 1991; and Noam Chomsky, *World Orders Old and New*, 1994). Thus, the reader receives another partial and biased analysis of state-sponsored terrorism that now dominates the Western literature (e.g., see Cindy C. Combs, *Terrorism in the Twenty-first Century*, 1997).

Third, in the analysis of the rogue leaders, Tanter engages in psychological analysis to explain their strange behavior that is by any measure amateurish, if not wrong. In any case, it needs much more depth and documentation in order to pass muster.

The book ends with no conclusion, so there is no effort to summarize or build on the analysis, a weakness only partially overcome by rereading the Introduction.

Risk Taking and Decisionmaking: Foreign Military Intervention Decisions. By Yaacov Y. I. Vertzberger. Stanford, CA: Stanford University Press, 1998. 519p. $69.50 cloth, $24.95 paper.

Edward Rhodes, *Rutgers University*

As critics of rational-choice modeling have long observed, when confronted with risk and uncertainty decision makers typically do not make rational calculations and choices. The challenge facing these critics is twofold: first, to develop a general understanding of the process of decision making under risk—one that recognizes the role of affective, cognitive, social, cultural, and organizational influences in determining perceptions of and attitudes toward risk—and, second, to draw upon this understanding to develop predictive models that are sufficiently parsimonious to be useful. Yaacov Vertzberger takes up the first of these challenges in this study of military intervention. He provides a wonderfully rich, highly textured examination of how decision makers actually grapple with risk, exploring the interplay of factors which encourages different assessments of dangers, different tastes in risk-taking, different tolerances for risk, and different behavior in risky situations. This is a welcome addition to the literature, bluntly challenging the narrower, more mechanistic microeconomic conceptions of risk that have increasingly informed political science discussions.

Specifically, Vertzberger is interested in explaining decisions to undertake and persevere in major military interventions. The subject is of more than purely theoretical interest: There are dark mysteries here worthy of resolution, nightmares of ill-judged policymaking that still haunt American and Israeli leaders and scholars traumatized by the Vietnam War and the 1982 Lebanon invasion and that may again come to haunt the creators of today's new global order. First, and perhaps most important, how do decision makers considering military intervention assess risk, how does this process of risk-assessment vary between leaders and between states, and how is this process shaped and reshaped across time? How is the identification and measurement of risk influenced by the context of decision and problem framing; by culture; by personality, individual cognitive structure, the demands of ego preservation, and decision-making heuristics; by group dynamics and composition; and by organizational forces? Second, how can we explain variance in decision makers' willingness to accept, or reject, particular types of risks and to roll the dice on a military solution to foreign policy problems?

The paths to military intervention are both complex and

multiple, and Vertzberger eschews easy and simplistic answers to these questions. As he frankly notes, "the description and explanation of risk assessment and the formation of a risk-taking preference defy parsimony.... No single cause effectively explains either risk assessment or the formation of a risk-taking preference; instead, explanations require a broad socio-cognitive approach..." (p. 111).

This unembarrassed acknowledgement of complexity is simultaneously the volume's great strength and greatest liability. Vertzberger's emphasis on understanding the full range of influences that shape risk behavior is arguably warrented given the state of the field and casts exciting new light on a set of puzzling intervention decisions. This "everything matters" approach to explanation, though, necessarily limits the volume's contribution to the development of a parsimonious predictive model, one able to compete with the rational-choice models Vertzberger dismisses. This will frustrate those readers interested in applying sociocognitive insights to other decision problems. The step from explanation to prediction is critical if sociocognitive approaches like Vertzberger's are to improve the quality of decision making or to challenge rational-choice explanations of foreign policy. This step is not an impossible one. Indeed, one plausible reading of Vertzberger's analysis and evidence is that decision framing, cognitive belief structures, and simplifying heuristics have an extraordinary effect on risk behavior. Most interesting, although Vertzberger addresses recent work on prospect theory only briefly, the effect of decision framing emerges in high relief from this research—decision makers display a heightened willingness to gamble and decreased inclination to face up to the full scope of risks when they see themselves as acting to avoid loss.

In part, the volume's substantial length and intimidating heft are dictated by the complexity of the process being studied and by Vertzberger's wide-angle examination. In part, however, they also reflect Vertzberger's decision to combine what are essentially three logically distinct studies within a single cover.

The first, densest, and most exciting is an analytical exploration of the concept of risk and the sociocognitive factors influencing risk assessments and preferences. Vertzberger eschews classical microeconomic approaches to risk and regrounds the concept in common usage. He conceives of risk as a situation involving both the danger of an adverse outcome and informational uncertainty—about possible outcomes, the probability of those outcomes, or the validity of estimates of outcomes and probabilities. Decision makers vary in their attitude toward risk, both in their tolerance for risk and in their taste for different "textures" of risks—risks of similar magnitude but possessing different transparency, certainty, horizons, complexity, reversibility, controllability, and accountability. Risk attitudes, Vertzberger argues, must be regarded as created (and revised) through ongoing sociocognitive processes, not as given or fixed.

This approach to risk is intellectually liberating, opening new analytical doors by defining the term in a way that captures the essence of real-life decision processes and problems. At the same time, however, the imprecision of Vertzberger's definition raises significant difficulties, particularly since he remains interested in comparing the behavior of "risk averse" and "risk acceptant" decision makers. Given the multiple dimensions of risk implied by his definition and the absence of any advice on how to combine these dimensions in a single function, it is impossible to ascertain which of any two policies is objectively "riskier" and which a risk-averse decision maker would prefer.

The second portion of the book, coexisting in some ways only uncomfortably with the first, is an examination of the factors that are likely to affect the success of military interventions and that should therefore be included in decision makers' risk assessments. That decision makers *should* pay attention to military capability, economic power, domestic and external political legitimacy, the nature of the target state, the effect of time on other variables, linkages between military and political outcomes, structural and nonstructural attributes of the international system, and possible responses by third parties when they gauge the risks of military intervention is reasonable enough. Given the logic of Vertzberger's sociocognitive conception of risk assessment and attitude-formation, however, it is much less clear that they *can*. As Vertzberger notes, decision makers are likely to overattend to the military balance and their appraisal of their own situation and to underweigh other risks.

The third component of the book is a focused comparison of decision making in five cases of military intervention—the Americans in Vietnam, Grenada, and Panama; the Israelis in Lebanon; and the Soviets in Czechoslovakia. Vertzberger's treatment is highly effective and the cases prove quite revealing, demonstrating the strikingly different paths leading decision makers to accept the risks of military intervention. His comparison of Grenada and Panama, for example, underscores how two presidents with dramatically different cognitive styles and initial risk preferences could approach risk-assessment and decision in very different ways but in the end both embrace military intervention. In two respects, however, case selection is arguably unfortunate. First, given Vertzberger's interest in advising foreign policy decision makers regarding the dangers and opportunities of military intervention, it is regrettable that he considers only cases from the Cold War: This limits Vertzberger's ability to address questions about the effect of globalization on the risks of intervention and the risk propensities of decision makers. Second, given Vertzberger's interest in exploring the factors that lead to risk acceptance, it is unfortunate that he has chosen not to examine cases in which military intervention was considered but rejected as too risky.

None of these complaints, however, should obscure Vertzberger's achievement and contribution. This volume should be regarded as mandatory reading for scholars grappling with the problem of explaining foreign policy decision making as well as for those wrestling with the issue of military intervention. Vertzberger's insights into the combined operation of sociocognitive influences on decision making under risk promise to spur and refocus theory-building efforts.

The Implementation and Effectiveness of International Environmental Commitments: Theory and Practice. Edited by David G. Victor, Kal Raustiala, and Eugene B. Skolnikoff. Cambridge: MIT Press, 1998. 737p. $27.50.

Juliann Emmons Allison, *University of California, Riverside*

The process of implementing international agreements is a highly complex and not yet fully understood process. This is nowhere more true than in the area of environmental protection, which is plagued by scientific uncertainty and complicated by the increasing participation of subnational governments and nongovernmental actors. Yet, a better understanding of just how international agreements are put into practice is key to the negotiation of successful international environmental accords, such as those intended to protect rare and endangered species, reduce stratospheric ozone depletion, and regulate trade in hazardous materials. After 25 years and 170 multilateral environmental agree-

ments, what do we know about implementing such accords? In response to this question, the contributors to *The Implementation and Effectiveness of International Environmental Commitments* provide fourteen interrelated case studies evaluating the effectiveness of international environmental accords, or the degree to which nations' commitments are fulfilled. The product of a three-year research project conducted at the International Institute for Applied Systems Analysis, the volume is a welcome complement to prior research on international environmental cooperation focused primarily on the formation of international institutions and the negotiation of legally binding commitments.

The fourteen case studies assess implementation at both the international and domestic levels of analysis in eight substantive issue areas. International systems for implementation review (SIRs) are emphasized in three of these issue areas: the protection of flora and fauna, stratospheric ozone pollution, and trade in hazardous substances. John Lanchbery provides a competent examination of trends in the history of international efforts to collect and disseminate information on wildlife protection, and reports that there have been an increasing number of consistently improving SIRs concerned with the protection of flora and fauna. In the first of two chapters on the implementation of the Montreal Protocol on Substances that Deplete the Ozone Layer, Owen Greene describes in detail the mechanisms dedicated to implementing that accord, as well as the formal and informal activities of complementary institutions, such as the Technology and Economic Assessment Panel (TEAP), that have evolved over time. Greene's study suggests that the increased redundancy and access to data and review procedures associated with multiple, overlapping SIR's have improved the effectiveness of the ozone regime's implementation review process. David G. Victor's excellent analysis of the Montreal Protocol's provisions for managing noncompliance distinguishes between ad hoc and formal procedures for achieving signatories' compliance with not only the accord's regulatory commitments but also with its programmatic requirements, i.e., data reporting. Victor's finding that soft law, or discussion and diplomacy, has proved more effective and easier to implement than hard law, or sanctions, confirms the intuition of casual observers that compliance has been smoothest among nations that are motivated independently to implement the Montreal Protocol. Victor also provides a fine discussion of the prior informed consent (PIC) norm, which has evolved over the last decade to protect the mostly developing importers of hazardous chemicals and pesticides from potentially exploitative trade with their more industrialized exporters.

The domestic politics of implementing international environmental agreements is stressed in three other issue areas: pollution in the North Sea and Atlantic Ocean, commercial whaling, and the dumping of nuclear waste at sea. Jon Birger Skærseth's analysis of international efforts to reduce pollution in the North Sea and northeast Atlantic Ocean focuses on the institutional transition from targeting the reduction of dumping at sea to controlling pollution on land, which has required much greater domestic regulatory activity. According to Skærseth, the emergence of a balance between polluters and environmentalists within a given nation has contributed to the successful implementation of international standards for the quality of ocean waters. Steiner Andresen argues that a lack of restrictions on membership in the International Whaling Commission (IWC) effectively permitted environmental nongovernmental organizations (ENGOs) to pack the commission with nonwhaling nations in favor of a moratorium on whaling. He further explains how ENGOs, particularly Greenpeace, also influenced national decisions to implement the IWC's preservationist policies. Olav Schram Stokke's study of the London Convention on the Prevention of Marine Pollution by Dumping of Wastes and Other Matter suggests that despite the accord's general failure, it nonetheless fostered the reduction of barriers to public participation in Soviet (and later Russian) decisions pertaining to nuclear waste disposal.

The remaining two issue areas—transboundary air pollution in Europe and the prevention of pollution in the Baltic Sea—include both chapters devoted to the functioning of SIRs and chapters focused on domestic implementation. With respect to transboundary air pollution, Juan Carlos di Primio provides a succinct description of the system for collecting and collating data on air pollution that preceded and continues to underlie the Convention on Long Range Transboundary Pollution's (LRTAP's) SIR. Jørgen Wettestad's complementary comparative study of the implementation of the LRTAP's nitrogen protocol in the Netherlands, the United Kingdom, and Norway suggests that the success of the LRTAP's SIR has coincided with the simultaneous decline in environmental and national ambitiousness with respect to reducing emissions of nitrogen oxides. His explanation for this coincidence is that the inherent difficulty of reducing nitrogen emissions has dampened interest in controlling this source of transboundary air pollution. Vladimir Kotov and Elena Nikitina's linked chapters on the implementation of the LRTAP's sulfur and nitrogen protocols in Russia suggest that the accord's success has been due largely to the Soviet Union's initial interpretation of transboundary air pollution control as a means of cooperating with the West, and centralized regulation of just the transboundary fluxes of sulfur dioxide and nitrogen oxides. According to Kotov and Nikitina, Russia's incomplete transition to a decentralized system of environmental regulation governed by market incentives for air pollution control has made it understandably much more difficult for that nation to continue implementing the LRTAP.

The volume's chapters on the implementation of the Convention on the Protection of the Marine Environment of the Baltic Sea (the Helsinki Convention) provide a provocative look at the role of public participation in the regulatory processes of democratic and democratizing nations. Contributors suggest that participation by environmentalists and the representatives of industries targeted by domestic regulations intended to reduce pollution of the Baltic Sea has mostly improved implementation of the Helsinki Convention in democratic nations; however, in democratizing nations, where environmentalism is weak and the administrative and financial resources necessary to regulate Baltic Sea pollution limited, public participation has arguably hindered it. Owen Greene's excellent description of the Baltic Sea regime's SIR suggests that despite the multiplicity of institutions and other parties dedicated to supporting its implementation, newly democratic members have found it administratively and fiscally difficult to comply with the Helsinki Convention's programmatic provisions, such as data collection and reporting. Alexei Roginko and Ronnie Hjorth both echo this concern in their accounts of Russian, Baltic, and Polish implementation of the Helsinki Convention, respectively. Originally interpreted as a security measure and means of establishing "green credentials," implementation by these nations was administered by the Soviet Union. The general lack of public concern for the environment and limited capacity of subnational governments to regulate environmental quality in these nations since the end of the Cold War support Roginko's and Hjorth's association of implementa-

tion with economic decline, rather than successful regulatory initiatives.

Analyses of the relationship between the political and economic transition of former Soviet nations and the implementation of international environmental agreements, such as the Helsinki Convention, support one of the volume's key conclusions. That is, eliciting international environmental commitments from democratizing and other nonliberal nations will likely require economic sanctions and other disincentives intended to alter their perception of the costs and benefits associated with international environmental cooperation. A greater reliance on nonbinding agreements is another possibility, considering some contributions, such as Victor's discussion of the PIC norms, relate higher levels of cooperation and more stringent regulatory programs to these less formal instruments. Unfortunately, the volume's length and scope may discourage all but the most diligent and interested readers from appreciating these and other significant conclusions. A less descriptive and more balanced theoretical treatment of selected cases would make it far more accessible and useful to international relations and comparative scholars alike. *The Implementation and Effectiveness of International Environmental Commitments* would, nonetheless, make a nice addition to the library of anyone seriously interested in the practicalities of international environmental cooperation.

APSR EXTERNAL REVIEWERS, 1997–98

The peer review process on which scientific publication relies depends on the intellectual contributions and generosity of those scholars who give of their time to read and evaluate the work of their peers. The Editor and the Editorial Board thank the following scholars, who served as manuscripts reviewers from August 15, 1997, through August 14, 1998.

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Required by Act of October 23, 1962: Section 4369, Title 39, United States Code

1. Date of Filing: September 28, 1998.
2. Title of Publication: *American Political Science Review.*
3. Frequency of Issue: Quarterly in March, June, September, December.
4. Location of Known Office of Publication:
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
5. Location of the Headquarters or General Business Office of the Publishers:
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
6. Names and Addresses of Publisher and Managing Editor:
   Publisher: American Political Science Association
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
   Manager Editor: Ada W. Finifter, Editor, *American Political Science Review*,
   Department of Political Science, 303 S. Kedzie Hall, Michigan State University, East Lansing, MI 48824.
7. Owner: American Political Science Association
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.
8. Known Bondholders, Mortgagees, etc.: None.
9. The purpose, function and nonprofit status of this organization and the exempt status for federal income tax purposes has not changed during the preceding 12 months.
10. Extent and Nature of Circulation:

	Average No. Copies Each Issue During Preceding 12 Months	Average No. Copies Single Issue Nearest to Filing Date
A. Total Number Copies Printed	16,681	16,605
B. Paid Circulation		
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2. Mail Subscriptions	15,695	15,624
C. Total Paid Circulation	15,695	15,624
D. Free Distribution by Mail, Carrier or Other Means	121	130
E. Total Distribution (C + D)	15,816	15,754
F. Office Use, Left-Over, Unaccounted, Spoiled after Printing	864	851
G. Total	16,681	16,605

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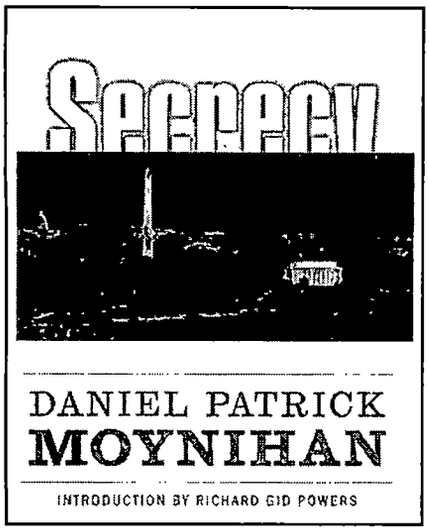

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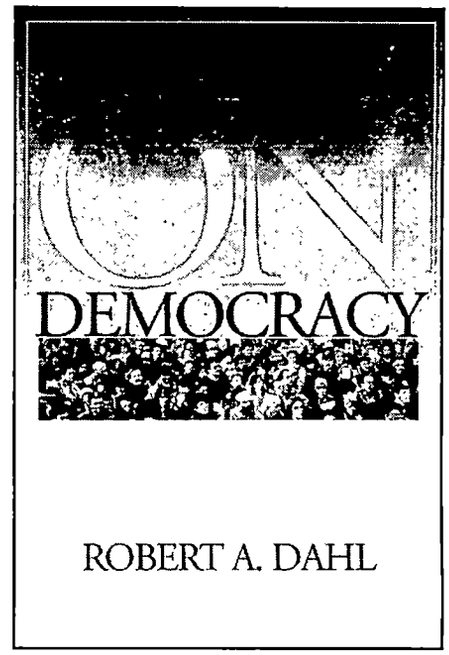

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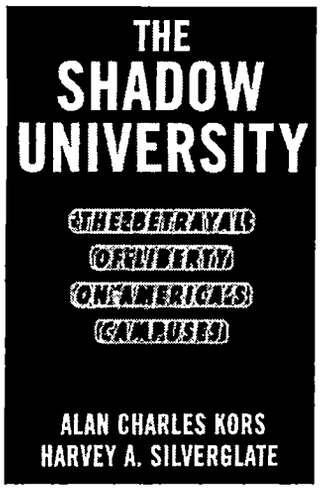

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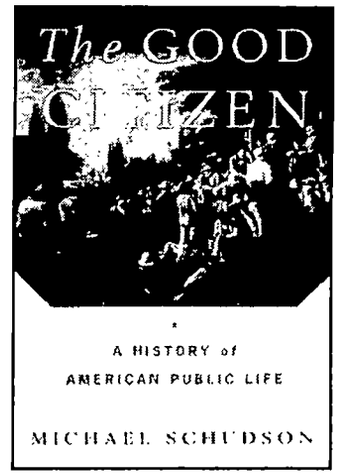

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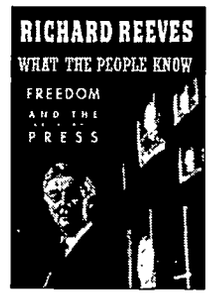

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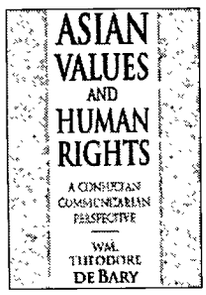

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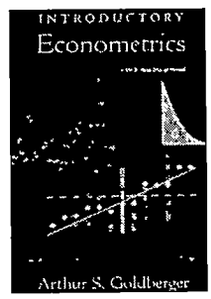

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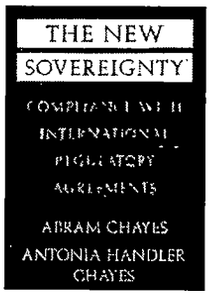

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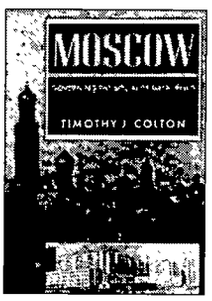

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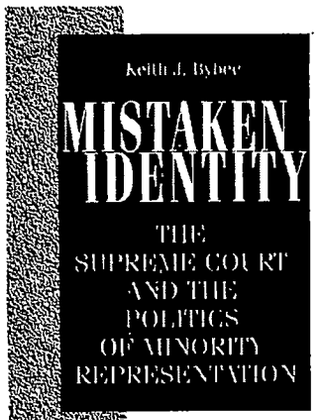

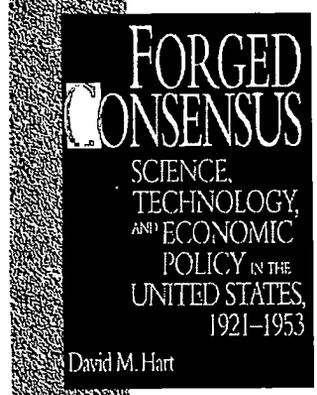

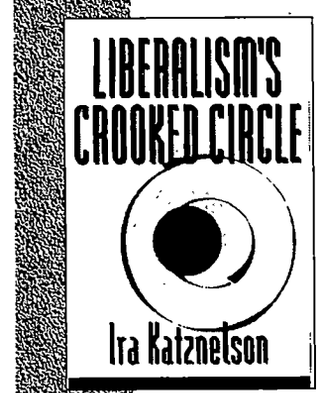

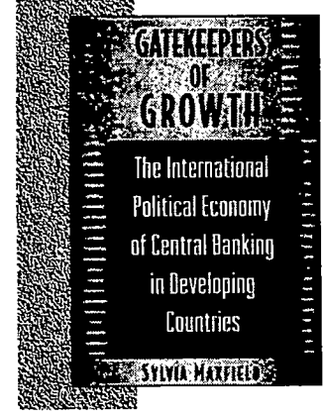

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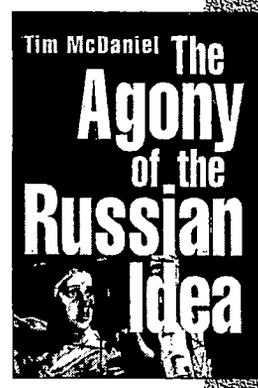

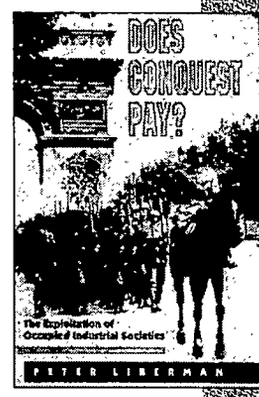

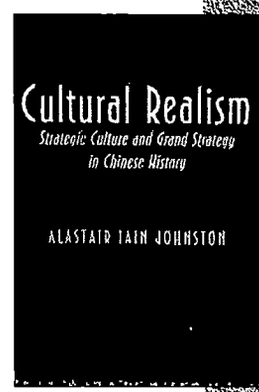

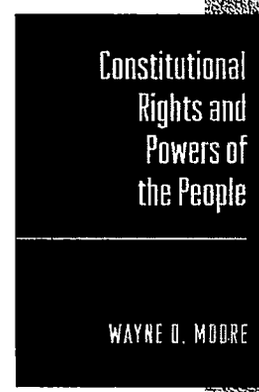

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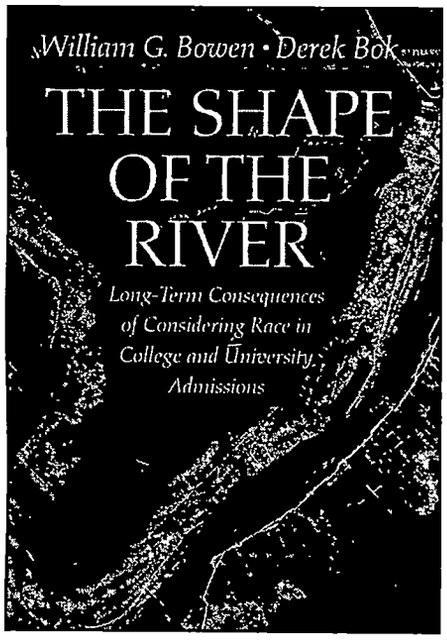

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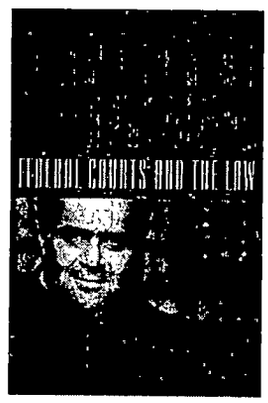

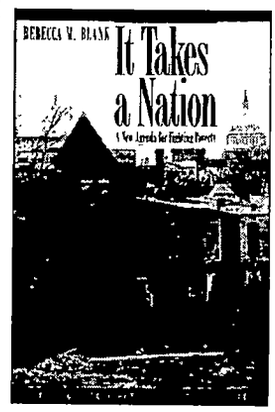

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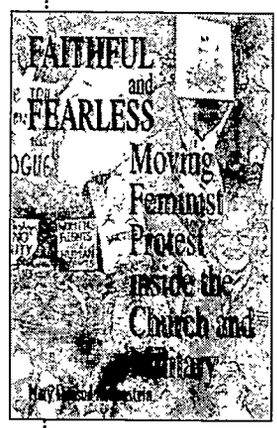

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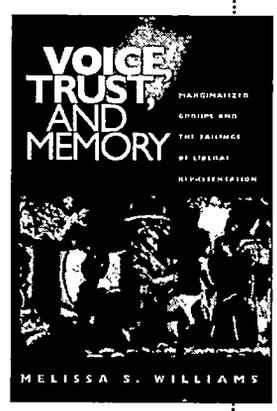

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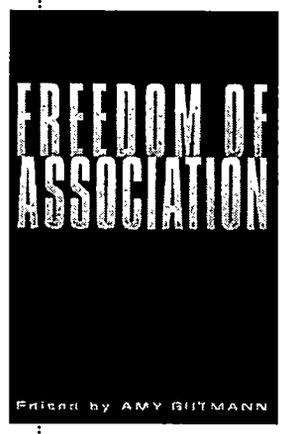

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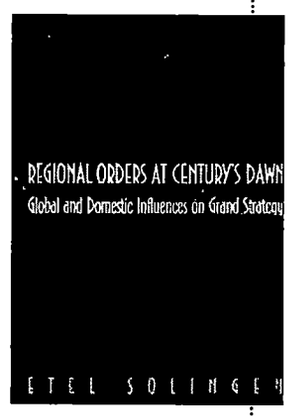

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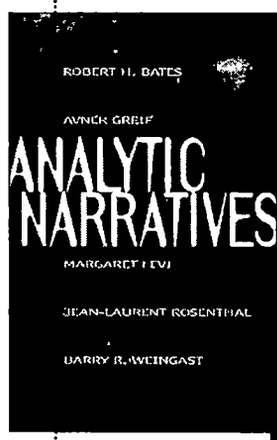

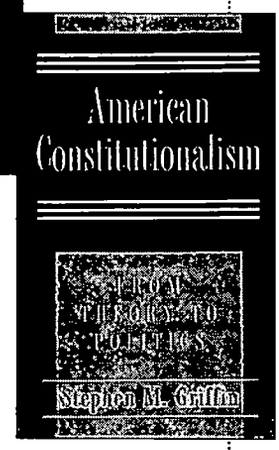

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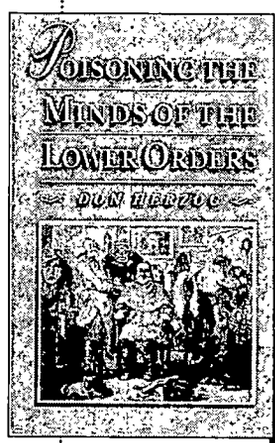

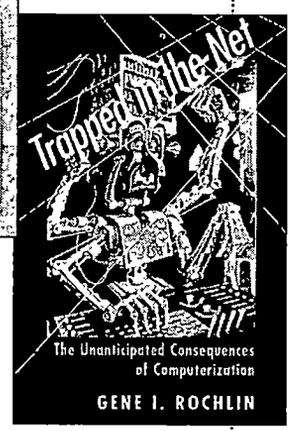

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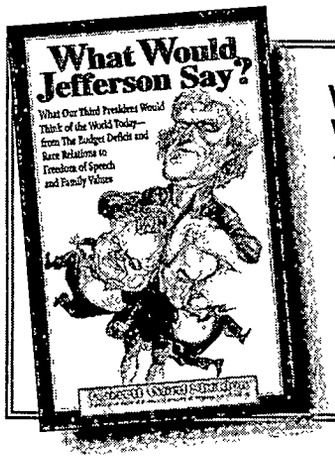

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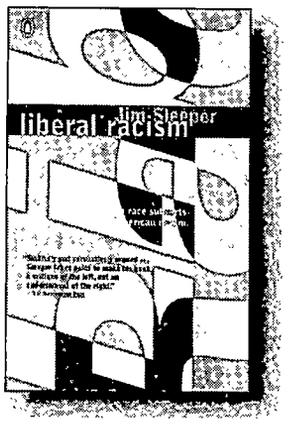

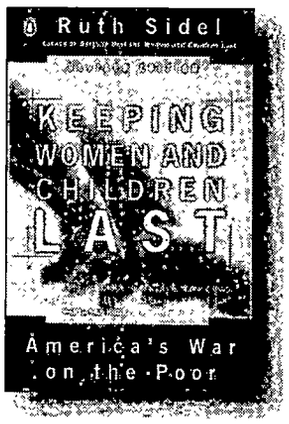

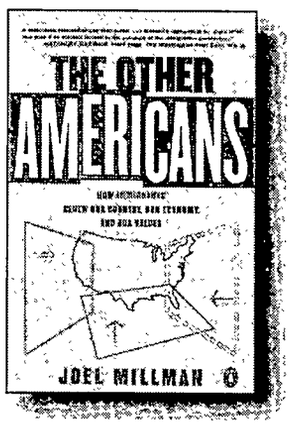

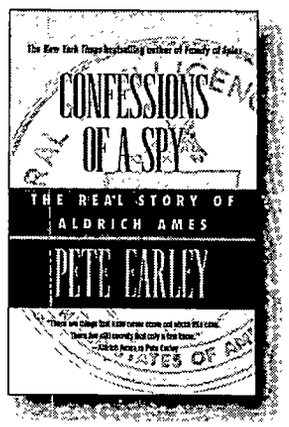

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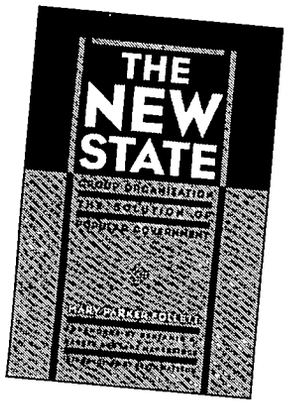

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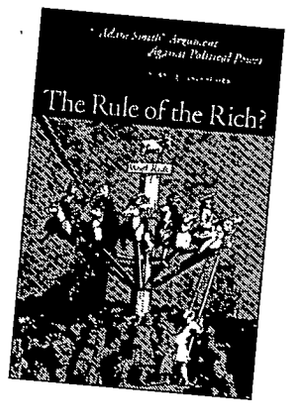

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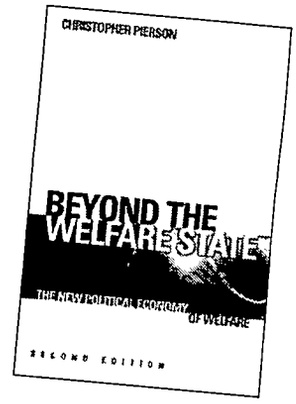

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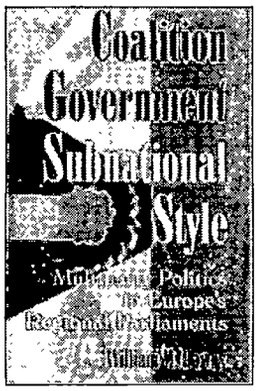

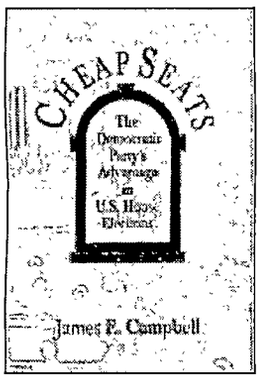

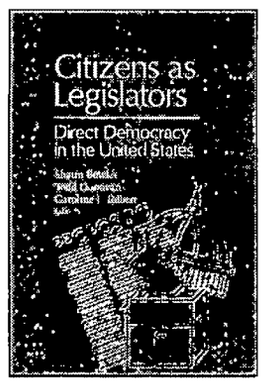

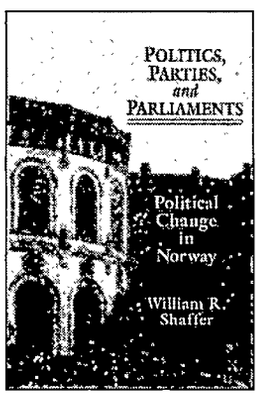

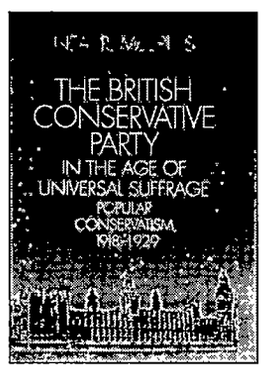

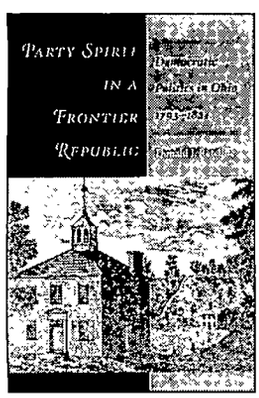

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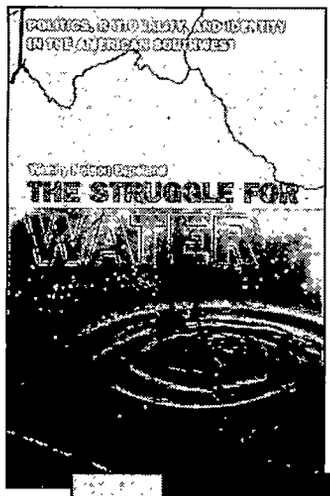

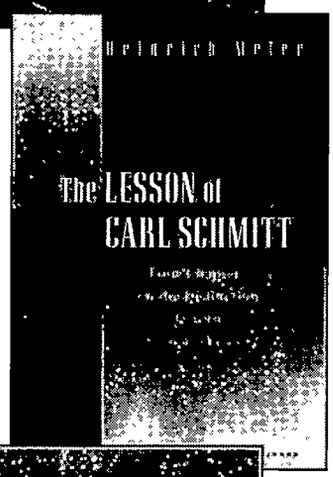

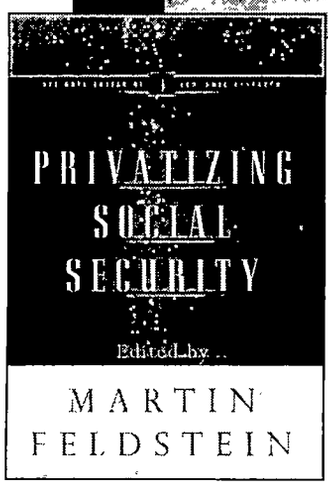

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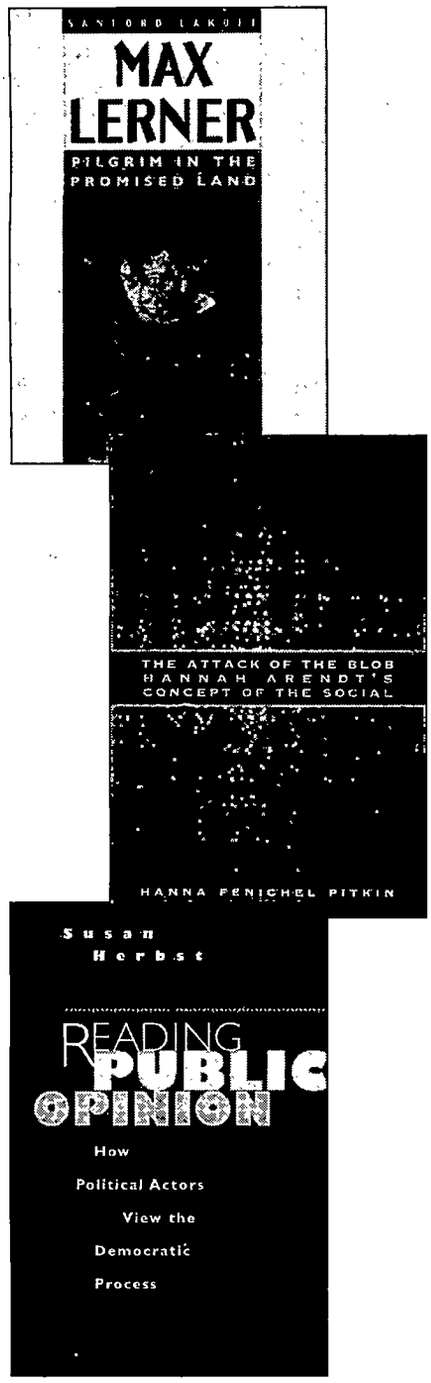

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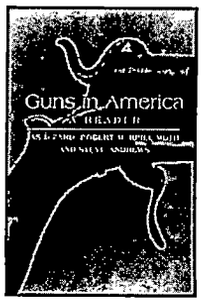

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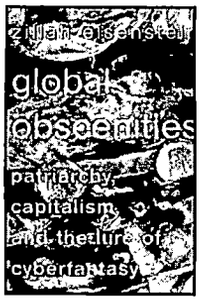

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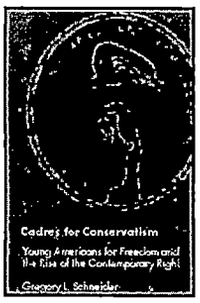

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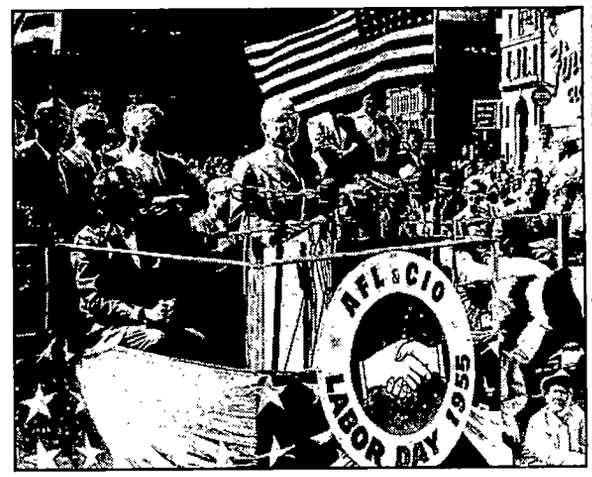

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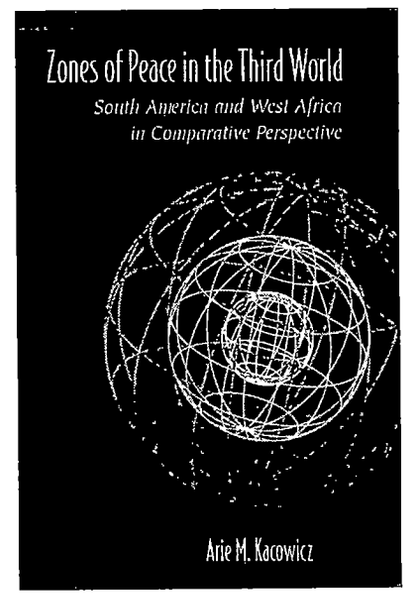

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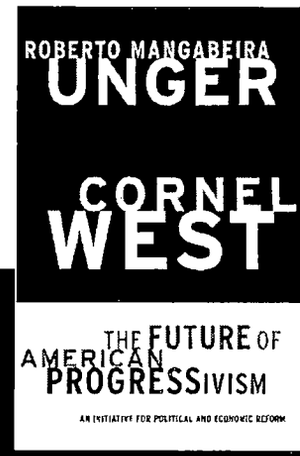

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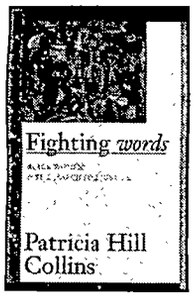

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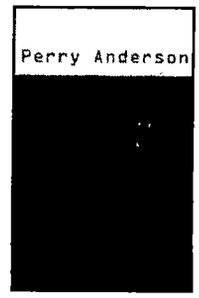

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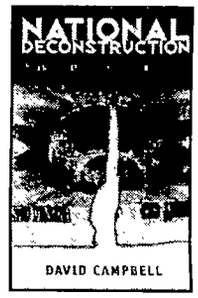

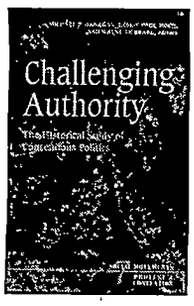

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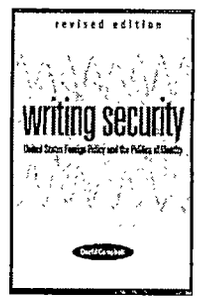

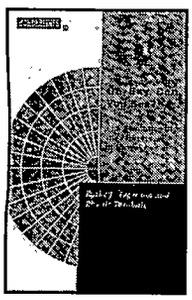

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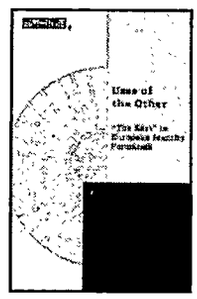

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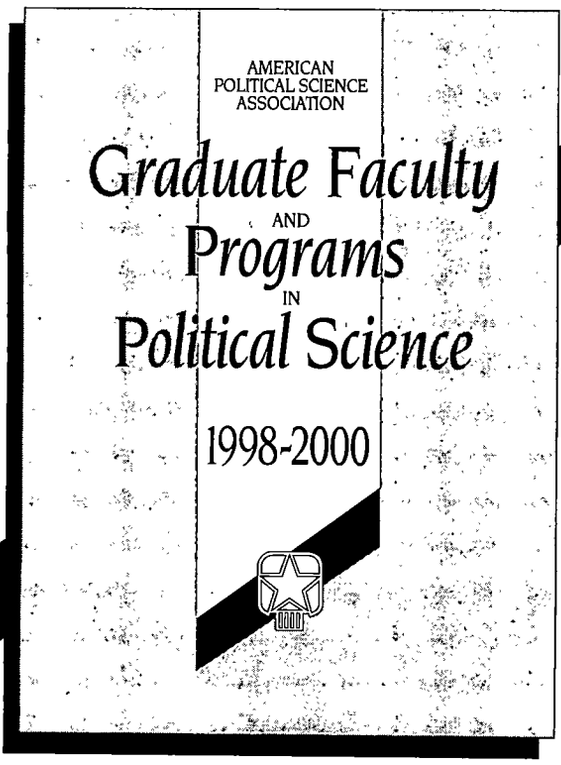

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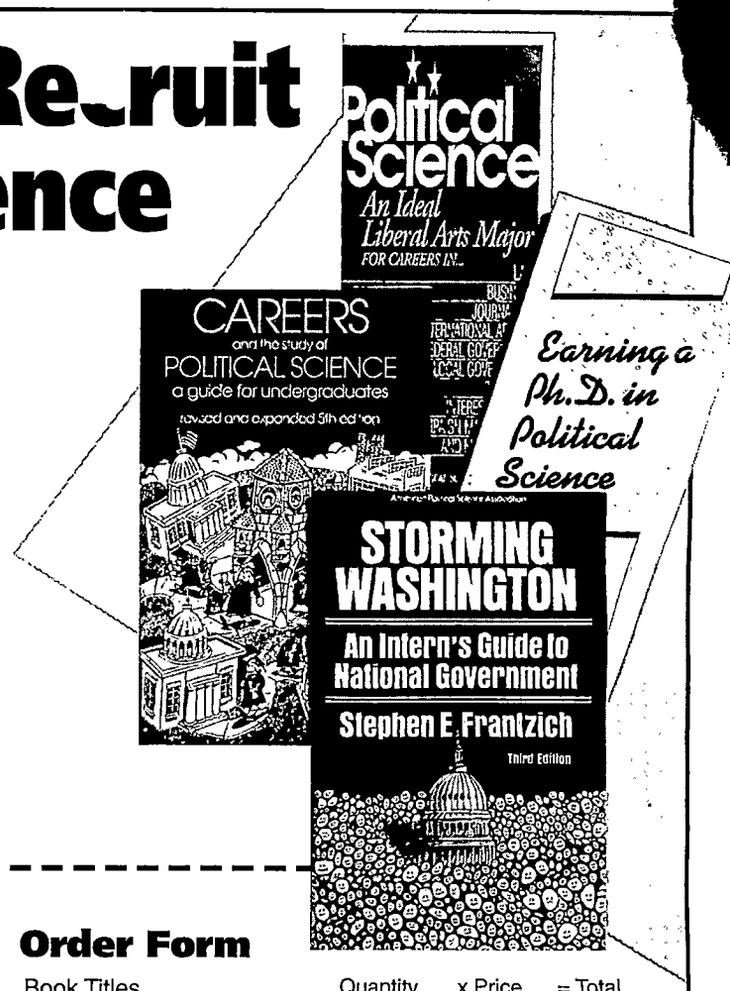

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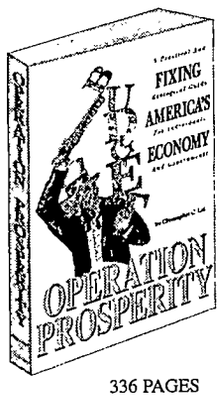

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